This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

MEDICAL LIBRARY
IN THE
A. COUNTWAY
LIBRARY OF

THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW

OR

QUARTERLY JOURNAL

OF

PRACTICAL MEDICINE AND SURGERY.

VOL. III.

JANUARY—APRIL 1849.

LONDON:
SAMUEL HIGHLEY, 32, FLEET STREET,
AND
JOHN CHURCHILL, 46, PRINCES STREET, SOHO.

MDCCCXLIX.

Gift of
Dr. J. C. Warren
5 March 1924.

41.

C. AND J. ADLARD, PRINTERS, BARTHOLOMEW CLOSE.

CONTENTS OF Nº V
OF THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL REVIEW.
JANUARY, 1849.

Analytical and Critical Reviews.

PAGE

Art. I.—1. Einige Bemerkungen über die Asiatische Cholera, &c. &c. Von C. Müller, Dr. Med. 1

2. Report on Asiatic Cholera in Regiments of the Madras Army, from 1828 to 1844; with Introductory Remarks on its Mode of Diffusion and Prevention, and Summary of the General Method of Treatment in India. By Samuel Rogers, F.R.C.S.E., and Surgeon of the Madras Army . . . ib.

3. Observations on the Asiatic Cholera, during a Residence in St. Petersburg in 1848, &c. By Adair Crawford, M.D., Member of the London College of Physicians (Metropolitan Sanitary Commission) . . . ib.

4. Observations on Malignant Cholera, &c. By Andrew Buchanan, M.D., Professor of the Institutes of Medicine in the University of Glasgow, &c. . ib.

5. On the Treatment of Asiatic Cholera. By Archibald Billing, M.D. . ib.

6. Cholera.—Practical Observations on a Successful Method of treating Cholera. By Charles Patterson, M.D. . . . . . . ib.

7. A Few Thoughts on Cholera, in reference to its Origin, the Nature of the Exciting Cause, and the Principle of Treatment. By M. T. Sadler, M.R.C.S.E. ib.

8. Plain Directions for the Prevention and Treatment of Cholera. By Thomas Allen, M.R.C.S.E. . . . . ib.

9. A Letter to Lord Morpeth, &c., on the Question, Is Cholera contagious or not? By William Reid, M.D. . . . . . . ib.

10. Practical Observations on the Prevailing Epidemic, called Cholera, &c. By James Keir, M.D., Knt., &c. . . . . . . ib.

Art. II.—1. Reports from the Select Committee of the House of Commons, on Medical Education; with the Minutes of Evidence and Appendix . 31

2. Reports from the Select Committee of the House of Commons, on the Medical Registration Bill; with the Minutes of Evidence, &c.—July, 1847; March, 1848 . . . . ib.

Art. III.—On the Nature and Treatment of Stomach and Renal Diseases; being an Inquiry into the Connexion of Diabetes, Calculus, and other Affections of the Kidney and Bladder, with Indigestion. By William Prout, M.D., F.R.S., Fellow of the Royal College of Physicians 49

Art. IV.—Erfahrungen über die Krankheiten des Gehöres und ihre Heilung, vom Medizinalrathe Eduard Schmalze, Docter der Medizin, Gehör-und Sprach-Arzte zu Dresden, &c. 66

Practical Observations on Diseases of the Ear, and their Treatment. By Dr. Edward Schmalze, Aurist at Dresden.

Art. V.—Clinical Observations on the Pathology and Treatment of Continued Fever, from Cases occurring in the Medical Practice of St. Bartholomew's Hospital. By Edward Latham Ormerod, M.B., Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital 74

§

PAGE
Art. VI.—A Treatise on Diet and Regimen. By William Henry Robertson, m.d., Physician to the Buxton Bath Charity 101
Art. VII.—Mémoires de l'Académie Royale de Médecine 123
Art. VIII.—1. Females and their Diseases; a Series of Letters to his Class. By Charles D. Meigs, m.d., Professor of Midwifery and the Diseases of Women and Children in the Jefferson Medical College at Philadelphia, &c. 138
2. A Practical Treatise on the Diseases peculiar to Women. Illustrated by Cases derived from Hospital and Private Practice. By Samuel Ashwell, m.d. . . . . ib.
Art. IX.—On Femoral Rupture; its Anatomy, Pathology, and Surgery. With a new Mode of Operating, applicable to Cases of Strangulated Hernia generally. With Plates. By John Gay, f.r.c.s., Surgeon to the Royal Free Hospital, &c. 165
Art. X.—A Dispensatory, or Commentary on the Pharmacopœias of Great Britain. By R. Christison, m.d., &c., President of the Royal College of Physicians, and Professor of Materia Medica in the University of Edinburgh . . 169
Art. XI.—A Course of Lectures on Dental Physiology and Surgery, delivered at the Middlesex Hospital School of Medicine. By John Tomes, Surgeon-Dentist to the Middlesex Hospital 175
Art. XII.—A Practical Treatise on the Use of the Microscope, including the different Methods of preparing and examining Animal, Vegetable, and Mineral Structures. By John Quekett, Assistant Conservator of the Museum, and Demonstrator of Minute Anatomy at the Royal College of Surgeons of England 188
Art. XIII.—1. Hints towards the Formation of a more Comprehensive Theory of Life. By S. T. Coleridge. Edited by Seth Watson, m.d., Oxon. . 194
2. Reflections on Organization; or Suggestions for the Construction of an Organic Atomic Theory. By Henry Freke, a.b. m.b. t.c.d., m.r.i.a. . ib.
Art. XIV.—On the Treatment of Ulcers on the Leg without Confinement. With an Inquiry into the best Mode of effecting the permanent Cure of Varicose Veins. By H. T. Chapman, f.r.c.s., &c. 198
Art. XV.—1. A Practical Treatise on Chemical Analysis, including Tables for Calculations in Analysis. By H. Rose. Translated from the French and Fourth German Edition, with Notes and Additions, by A. Normandy. In Two Vols. Vol. I. Qualitative. Vol. II. Quantitative . . . 203
2. Practical Introduction to H. Rose's Treatise on Chemical Analysis, illustrated by Synoptic Tables and numerous Formulas. By A. Normandy . ib.

Bibliographical Notices.

Art. I.—Surgical Anatomy. By Joseph Maclise, Surgeon. Fasciculus I . 205
Art. II.—1. Elements of General Pathology. By Andrew Stillé, m d., Lecturer on Pathology and the Practice of Medicine in the Philadelphia Medical Association 207
2. The Principles and Practice of Midwifery. By David H. Tucker, m.d., Professor of the Principles and Practice of Medicine, &c. With numerous Illustrations . . . . ib.
Art. III.—Kurt Sprengel's Versuch einer pragmatischen Geschichte der Arzneikunde. Vierte Auflage, mit Berichtigungen und Zusätsen versehen von Dr. Julius Rosenbaum, &c. &c. Erster Band. Aelteste Geschichte der Medicin bis zur empirischen Schule 203

PAGE

ART. IV.—American Medical Biography; or Memoirs of Eminent Physicians, embracing principally those who have died since the publication of Dr. Thacher's work on the same subject. By STEPHEN W. WILLIAMS, M.D., &c. &c. With Engravings 210

ART. V.—Clinical Midwifery; comprising the Histories of Five Hundred and Forty-five cases of Difficult, Preternatural, and Complicated Labour. With Commentaries. By ROBERT LEE, M.D., F.R.S., &c. 211

ART. VI.—1. The Student's Clinical Memorandum Book, or Medical Practitioner's Remembrancer, and Vade-Mecum 212

2. The Clinical Repertory, Synoptical Tabular Form . . . . ib.

ART. VII.—1. School Chemistry; or Practical Rudiments of the Science. By ROBERT DUNDAS THOMSON, M.D., Lecturer on Chemistry in the University of Glasgow, &c. 212

2. An Introduction to Practical Chemistry, including Analysis. By JOHN E. BOWMAN, Demonstrator of Chemistry in King's College, London . ib.

ART. VIII.—The Periodoscope: with its Application to Obstetric Calculations, and the Periodicities of the Sex. By W. TYLER SMITH, M.B. Lond., Obstetric Lecturer in the Hunterian School of Medicine 214

ART. IX.—Chemical Technology; or Chemistry applied to the Arts and Manufactures. By Dr. F. KNAPP, Professor at the University of Giessen. Edited, with numerous Notes and Additions, by Dr. EDMUND RONALDS, Lecturer on Chemistry at the Middlesex Hospital, and Dr. THOMAS RICHARDSON, of Newcastle-on-Tyne 215

ART. X.—Remarks on the Extension of Education at the University of Oxford. By HENRY WENTWORTH ACLAND, M.D., F.R.S., Lee's Reader in Anatomy, &c. &c. . . . . ib.

ART. XI.—Hand-Book of Physiology. By WILLIAM SENHOUSE KIRKES, M.D. Assisted by JAMES PAGET, Lecturer on General Anatomy and Physiology at St. Bartholomew's Hospital 217

ART. XII.—Elements of Chemistry, including the Applications of Science in the Arts. By THOMAS GRAHAM, F.R.S., V.P.R.S., Corresponding Member of the Institute of France, Professor of Chemistry in University College, London, &c. &c. 219

ART. XIII.—Wonders displayed by the Human Body in its Endurance of Injury. From the Portfolio of DELTA 220

ART. XIV.—A Dictionary of Medical Science; containing a concise Explanation of the various Subjects and Terms; with the French and other Synonymes; Notices of Climate, and of celebrated Mineral Waters; Formulæ for various Officinal and Empirical Preparations, &c. By ROBLEY DUNGLISON, M.D., Professor of the Institutes of Medicine, &c., in Jefferson Medical College, Philadelphia . . . . ib.

ART. XV.—Medical Jurisprudence. By ALFRED S. TAYLOR, F.R.S., Licentiate of the Royal College of Physicians, and Professor of Medical Jurisprudence and Chemistry in Guy's Hospital 221

ART. XVI.—Portraits of Diseases of the Skin. By ERASMUS WILSON, F.R.S. Fasciculus IV 222

ART. XVII.—The Natural History of Man; comprising Inquiries into the Modifying Influence of Physical and Moral Agencies on the Different Tribes of the Human Family. By JAMES COWLES PRICHARD, M.D., F.R.S., M.R.I.A., Corresponding Member of the Institute of France, &c. &c. . . ib.

ART. XVIII.—Pathology of the Human Eye. By JOHN DALRYMPLE, F.R.C.S. Fasciculus I 224

Original Report.

PAGE
On Pulmonary Phthisis, as observed at the Hospital for Consumption, Brompton. By Walter Hayle Walshe, m.d., Prof. Clin. Med. Univ. Coll. London, Physician to the Hospital for Consumption, &c. . . . 225

Periscope.

On the Circulation in Insects. By Emile Blanchard . . . 257
Effect of Ether on the Phenomena of Respiration. By MM. Ville and Blandin . 259
A Case of Dichromatic Daltonism. By Dr. Deconde . . . ib.
On the Composition of the Bile . . . . . . ib.
Blue Coloration of the Dressings of Ulcers 260
Nature of the Acid in Gastric Juice . . . . . . ib.
On the Existence of Lactic Acid in the Animal Fluids . . . 261
On the Chemical Properties of Saliva. By M. Bernard . . . ib.
Existence of Urea in the Vitreous Humour 262
Non-existence of Urea and Uric Acid in the Juice of Flesh . . . ib.
On the Clinical Detection of Iodine . . . . . . ib.
On Pectoral Undulation, or Thoracic Vibration in Health and Disease. By M. Monneret . . . . ib.
On the Injection of Various Substances into the Veins. By Drs. Manzolini and Quaglino, of Milan 263
Case of Hydrophobia Spontanea. By Dr. Mombert . . . 264
A Case of Epileptica Peripherica. By Dr. Stumke . . . 265
On Muscular Rheumatism. By M. Valleix . . . . ib.
Case of Rheumatic Blindness. By Dr. Cramer 266
On Rachitis. By M. Trousseau 267
On Bilious Pneumonia. By M. Martin Solon 268
On the Cholera of Infants. By M. Trousseau 269
On Operating early for Hare-lip 270
On the Employment of Aqua Saturni in Strangulated Hernia. By Dr. Neuhold . 271
On Gunshot Wounds . . . . ib.
On the Dropsical Affections of Pregnant Women. By M. Devilliers . . 273
Anomalies of the Menstrual Functions in relation to Fecundation. By M. Paul Dubois 276
On Trismus Nascentium. By Dr. Simms . . . . ib.
Extraordinary Size of Twins. By Dr. Bertolet 278
On the Question whether Calomel exerts any Specific Influence on the Biliary Secretion. By M. Michea . . . . . . ib.
On the Employment of the Seeds of Phellandrium Aquaticum in Phthisis and Chronic Bronchitis. By M. Sandras 279
On the Treatment of Lupus by Cod-liver Oil. By M. Emery . . ib.
On Vinum Sem. Colchici Opiate in Gonorrhœa. By Dr. Froinus . . 280
Belladonna in Eneuresis. By M. Trousseau . . . . ib.
Poisoning by Digitalis. By Dr. Forget . . . . . ib.

Mr. Neill, of Liverpool, and his Reviewer 281

Books received for Review 282

CONTENTS OF N° VI

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

APRIL, 1849.

Analytical and Critical Reviews.

PAGE

Art. I.—1. The Elements of Materia Medica and Therapeutics. By Jonathan Pereira, m.d., f.r.s., &c. 285

2. Traité de Thérapeutique et de Matière Médicale. Par A. Trousseau et H. Pidoux . . . . ib.

3. Traité de l'Art de Formuler, ou Notions de Pharmacologie appliquée à la Médecine. Par le Docteur Mialhe . . . . . ib.

4. Lectures on the Chemistry of Pathology and Therapeutics, showing the application of the Science of Chemistry to the Discovery, Treatment, and Cure of Disease. By Alfred B. Garrod, m.d. . . . . ib.

5. Lectures on the Influence of Researches in Organic Chemistry on Therapeutics, especially in relation to the Depuration of the Blood, delivered at the Royal College of Physicians. By Dr. Golding Bird, a.m., f.r.s., &c. ib.

Art. II.—Physiological, Anatomical, and Pathological Researches. By John Reid, m.d., f.r.c.s.e., Chandos Professor of Anatomy and Medicine in the University of St. Andrews, &c. &c. 318

Art. III.—Guy's Hospital Reports. Second Series . . . 333

Art. IV.—Die Hautkrankheiten durch anatomische Untersuchungen erläutert, von Dr. Gustav Simon 342

Art. V.—Observations on Hospital Gangrene; with reference chiefly to the Disease as it appeared in the British Army during the late War in the Peninsula, with Prefatory Remarks. To which are appended, Cases, showing how extensively applicable the Antiphlogistic Treatment is to other Diseases of the Army. By John Boggie, m.d., Surgeon to Her Majesty's Forces . 358

Art. VI.—On the System of the Great Sympathetic Nerve. By C. Radclyffe Hall, m.d., m.r.c.s., &c. 364

Art. VII.—1. Lectures on Electricity and Galvanism in their Physiological and Therapeutical Relations, delivered at the Royal College of Physicians, in March, 1847. By Golding Bird, a.m., m.d., f.r.s., Fellow of the College, Assistant-Physician to, and Lecturer on Materia Medica at Guy's Hospital . 373

2. On the Efficacy of Electricity, Galvanism, Electro-Magnetism, and Magneto-Electricity, in the Cure of Disease, and on the best Methods of Application. By M. Donovan, formerly Professor of Chemistry to Apothecaries' Hall . ib.

3. De l'Influence de l'Electricité Atmosphérique et Terrestre sur l'Organisme, et de l'Effet de l'Isolement électrique considéré comme moyen curatif et préservatif d'un grand nombre de Maladies. Par Emm. Pallas, Officier de l'Ordre Royal de la Légion-d'Honneur, Docteur en Médecine de la Faculté de Paris, &c. . . . . ib.

4. Elements de Physique Terrestre et de Météorologie. Par M. Becquerel, de l'Académie des Sciences de l'Institut de France et Professeur-Administrateur du Muséum d'Histoire Naturelle, &c.; et par M. Ed. Becquerel, Aide-Naturaliste au Muséum d'Histoire Naturelle, &c. . . . ib.

§

PAGE

ART. VIII.—Traité des Fractures et des Luxations. Par J. F. MALGAIGNE, Chevalier de la Légion-d'Honneur et du Mérite Militaire de Pologne, Membre de l'Académie Royale de Médecine, Chirurgien de l'Hôpital Saint-Louis 388

A Treatise on Fractures and Dislocations. By J. F. MALGAIGNE, Member of the Royal Academy of Medicine, and Surgeon to the Hospital Saint Louis.

ART. IX.—1. Lectures on the Diseases of Infancy and Childhood. By CHARLES WEST, M.D., Senior Physician to the Royal Infirmary for Children, &c. . 406

2. A Practical Treatise on the Diseases of Children. By J. FORSYTH MEIGS, M.D., Lecturer on the Diseases of Children in the Philadelphia Medical Association, &c. . . . . . . ib.

3. Essays on the Theory of Convulsive Diseases. Essay I. On the Convulsive Affections of Infants, and especially Laryngismus. By MARSHALL HALL, M.D., F.R.S., &c. . . . . . . ib.

4. Monograph; On the Cerebral Diseases of Children. By WALTER C. DENDY, Surgeon to the Royal Infirmary for Children, &c. . . ib.

5. Reports on the General and Medical Committees of the Royal Infirmary for Children, Waterloo Bridge Road . . . . . . ib.

6. Bibliothèque du Médecin Praticien.—Maladie des Enfants de la Naissance à la Puberté (Médecine et Chirurgie). Sous la Direction du Docteur FABRE ib.

ART. X.—Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London 433

ART. XI.—On Bandaging and other Operations of Minor Surgery. By F. H. SARGENT, M.D. 452

ART. XII.—1. A Treatise on the Practice of Medicine. By GEORGE B. WOOD, M.D., Professor of Materia Medica and Pharmacy in the University of Pennsylvania, one of the Physicians of the Pennsylvanian Hospital, &c. . . 455

2. Clinical Lectures on the Practice of Medicine. By ROBERT J. GRAVES, M.D., M.R.I.A., &c. &c. Second edition, edited by J. MOORE NELIGAN, M.D., M.R.I.A., Physician to Jervis Street Hospital, Lecturer on the Practice of Medicine, &c. . . . . ib.

3. Clinical Lectures, delivered in the Theatre of Mercer's Hospital during the Session of 1847-8. By JAMES F. DUNCAN, M.D., T.C.D., Assistant-Physician to the Hospital, Fellow of the King's and Queen's College of Physicians, &c. &c. . . . . ib.

ART. XIII.—Elements of Anatomy. By JONES QUAIN, M.D. Fifth Edition. Edited by RICHARD QUAIN, F.R.S., and WILLIAM SHARPEY, M.D., F.R.S., Professors of Anatomy and Physiology in University College Hospital . 478

ART. XIV.—A Practical Treatise on the Domestic Management and most important Diseases of Advanced Life; with an Appendix. By G. E. DAY, M.D., Fellow of the Royal College of Physicians, and Physician to the Western General Dispensary 486

ART. XV.—The Poetry of Science, or Studies of the Physical Phenomena of Nature. By ROBERT HUNT, Author of 'Researches on Light,' Keeper of Mining Records in the Museum of Practical Geology . . . 495

ART. XVI.—The Diagnosis and Treatment of Eruptive Diseases of the Scalp. By J. MOORE NELIGAN, M.D., Physician to Jervis Street Hospital, &c. . 498

ART. XVII.—Chemical Reports and Memoirs. Edited by THOMAS GRAHAM, V.P.R.S., Corresponding Member of the Institute of France, Professor of Chemistry in University College, London, &c. &c. . . . 502

Bibliographical Notices.

PAGE
ART. I.—An Inquiry into the Degree of Certainty in Medicine; and into the Nature and Extent of its Power over Disease. By ELISHA BARTLETT, M.D., Professor of the Theory and Practice of Medicine in Transylvania University 509

ART. II.—Surgical Anatomy. Fascic. II. By JOSEPH MACLISE . . 510

ART. III.—1. Demonstrations of Anatomy: being a Guide to the Knowledge of the Human Body by Dissection. By GEORGE VINER ELLIS, Junior Professor of Anatomy in University College, London 512

2. A Manual of the Dissection of the Human Body. By LUTHER HOLDEN, F.R.C.S., Demonstrator of Anatomy at St. Bartholomew's Hospital . ib.

ART. IV.—The Animal Kingdom, arranged after its Organization, forming a Natural History of Animals, and an Introduction to Comparative Anatomy. By the late Baron GEORGES CUVIER. Translated and adapted to the present State of Science. A New Edition, with Additions by W. B. CARPENTER, M.D., F.R.S., and J. O. WESTWOOD, F.L.S. . . 513

ART. V.—An Essay on the Cerebral Affections occurring most commonly in Infancy and Childhood, including notices of their History, Causes, Diagnosis, Prognosis, and Treatment. Being the Council Prize Essay awarded at the Annual Meeting of the Provincial Medical and Surgical Association, held at Bath, on Wednesday and Thursday, August 16th and 17th, 1848. By VALENTINE DUKE, M.D., Fellow of the Royal College of Surgeons in Ireland, &c. &c. 514

ART VI.—An Introduction to Botany. By JOHN LINDLEY, PH.D., F.R.S., Professor of Botany in University College, London, &c. . . . 515

ART. VII.—Practical Pharmacy: the Arrangements, Apparatus, and Manipulations of the Pharmaceutical Shop and Laboratory. By FRANCIS MOHR, PH.D., Assessor Pharmaciæ of the Royal Prussian College of Medicine, Coblentz; and THEOPHILUS REDWOOD, Professor of Chemistry and Pharmacy to the Pharmaceutical Society of Great Britain 516

ART. VIII.—Lexicon Medicum; or Medical Dictionary. By the late ROBERT HOOPER, M.D., F.L.S., Eighth Edition, revised, corrected, and improved by KLEIN GRANT, M.D., late Senior Physician to the Royal General Dispensary, and Lecturer on the Practice of Physic at the Aldersgate Street School of Medicine, &c. &c. 517

ART. IX.—1. Bibliothèque des Médecins Grecs et Latins, publiée avec le concours de Médecins Erudits de la France et d'Etranger. Par le Docteur CH. DAREMBERG 519

2. Résumé d'un Voyage Médico-Littéraire en Angleterre; Lu à l'Académie des Inscriptions et Belles-Lettres, dans la Séance du six Octobre, 1848. Par le Docteur CH. DAREMBERG . . . . . . ib.

ART. X.—The London and Provincial Medical Directory for 1849 . . 520

ART. XI.—An Essay on some of the most important Diseases of Women, with a Description of a Novel Invention for their Treatment and Relief. By W. JONES, M.D., Physician to the Free Hospital for Women and Children . 521

ART. XII.—The Undercliff of the Isle of Wight; its Climate, History, and Natural Productions. By GEORGE A. MARTIN, M.D. . . . 522

Periscope.

PAGE
An Experimental Inquiry into the Manifestations of Electricity during the organic Process of Secretion in living Animals. By H. F. Baxter, Esq. . 523
On the Absence of Vomiting in the Horse. By M. Flourens . . 524
On the Development and Functions of the Spermatozoa. By Drs. Wagner and Leuckardt . . . . ib.
On the Estimation of Urea 525
Analysis of Human Milk 526
On the Origin of Sugar in the Animal Economy. By Dr. Charles Bernard . ib.
On the Pancreatic Fluid. By Dr. Charles Bernard . . . 527
On Ozone and Influenza. By Dr. Spengler 529
Multiple Fibrous Tumours 530
On the Influence of Physical Agents in Variola. By M. Serres . . ib.
On the Analogy and Differences between Tubercle and Scrofula. By M. Legrand ib.
On the Absorption of Virus. By M. Renault 531
On the Simultaneous Development of Variola and Vaccinia. By MM. Herard and Bousquet . . . . ib.
On Paracentesis in Acute Pleurisy. By M. Trousseau . . . 533
On Asthma occurring in a Child By Dr. Tott . . . . ib.
On Ciliary Neuralgia. By M. Tavignot . . . . ib.
On the Treatment of the Asphyxia of Confluent Variola. By Dr. Herpin . 534
On Polydipsia. By M. Vigla . . . . . . ib.
Œdema of the Glottis . . . . ib.
On Coxalgia. By M. Malgaigne 536
On a New Variety of Luxation of the Humerus. By M. Malgaigne . . 537
On Ligature of the Left Subclavian Artery. By Dr. Mason Warren . . ib.
On the Treatment of Lupus. By MM. Devergie and Petrequin . . 538
On the Treatment of Internal Strangulation of the Intestines by Strychnine. By Dr. Homolle 539
Case of Hydrometra occurring in an Unimpregnated Uterus. By Dr. Grandidier ib.
On Prolapsus of the Funis during Labour. By Dr. Hoffman . . 540
On the Purulent Ophthalmia of Infants. By MM. Von Ammon and Mildner . ib.
On Prolapsus Uteri. By Professor Hohl 541
Case of Uterus Bicornis. By Dr. Generali 542
On the Employment of Iodide of Potassium in Saturnine and Mercurial Affections . 543
Administration of Castor Oil . . . . ib.
On Cannabine. By M. Dorvault . . . . . . ib.
On the Hæmostatic Properties of Cotton. By M. Bourdin . . . 544
On the Treatment of Angina Tonsillaris, by small Doses of Calomel. By M. Rene-Vanoye . . . . ib.
On Absorption of Medicinal Substances by the Schneiderian Membrane. By M. Comperat . . . . ib.
Administration of Rhubarb . . . . ib.
On Sugar as an Antaphrodisiac. By M. Provençal . . . ib.
On the Condition of Drunkenness as a Complication of Wounds, and as a Cause of Prompt or Sudden Death. By M. Tardieu . . . 545
On Poisoning by Sulphate of Zinc. By Professors Platner and Pignacca . ib.
On Death from Suffocation. By Dr. Casper 546
Modes of committing Suicide in France. By M. Brierre de Boismont . 547
A Statistical Inquiry, as to whether the proportionate Frequency, the Nature, and the principal Characters of Diseases have undergone in the course of time the great Modifications they are said to have done. By M. Valleix . ib.
Statistics of the Hospitals and the Medical Profession in Paris . . 547

Biographical Notice of Dr. J. C. Prichard 550

Books received for Review 561
Title, &c.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1849.

PART FIRST.

Analytical and Critical Reviews.

ART. I.

1. *Einige Bemerkungen über die Asiatische Cholera, &c. &c.* Von C. MÜLLER, Dr. Med.—*Hannover,* 1848.
2. *Report on Asiatic Cholera in Regiments of the Madras Army, from* 1828 *to* 1844; *with Introductory Remarks on its Mode of Diffusion and Prevention, and Summary of the General Method of Treatment in India.* By SAMUEL ROGERS, F.R.C.S.E., and Surgeon of the Madras Army.—*London,* 1848. 8vo, pp. 268.
3. *Observations on the Asiatic Cholera, during a Residence in St. Petersburg in* 1848, *&c.* By ADAIR CRAWFORD, M.D., Member of the London College of Physicians (Metropolitan Sanitary Commission). 8vo, pp. 24.
4. *Observations on Malignant Cholera, &c.* By ANDREW BUCHANAN, M.D., Professor of the Institutes of Medicine in the University of Glasgow, &c.—*London,* 1848. 8vo, pp. 42.
5. *On the Treatment of Asiatic Cholera.* By ARCHIBALD BILLING, M.D. —*London,* 1848. 8vo, pp. 14.
6. *Cholera.—Practical Observations on a Successful Method of treating Cholera.* By CHARLES PATTERSON, M.D.—*Dublin,* 1848. 8vo, pp. 8.
7. *A Few Thoughts on Cholera, in reference to its Origin, the Nature of the Exciting Cause, and the Principle of Treatment.* By M. T. SADLER, M.R.C.S.E.—*London,* 1848. 8vo, pp. 51.
8. *Plain Directions for the Prevention and Treatment of Cholera.* By THOMAS ALLEN, M.R.C.S.E.—*Oxford,* 1848. 8vo, pp. 40.
9. *A Letter to Lord Morpeth, &c., on the Question, Is Cholera contagious or not?* By WILLIAM REID, M.D.—*London,* 1848. 8vo, pp. 16.
10. *Practical Observations on the Prevailing Epidemic, called Cholera, &c.* By JAMES KEIR, M.D., Knt., &c. Part I.—*Edinburgh,* 1848. 8vo, pp. 28.

DR. MÜLLER, the author of the first of the works quoted above, appears to have been sent to St. Petersburg by the Hanoverian Government, for the purpose of reporting upon the progress of cholera, and of inquiring into the opinions most generally entertained regarding its nature and treatment. The work consists of four Reports: of which the first was written eight days after Dr. Müller's arrival at St. Petersburg, the second and third were composed at subsequent periods, and the fourth after his return to Hanover. Dr. Müller apologises in his preface for certain inconsistencies and variations of opinion between the first and later Reports. He did not deem it advisable, however, to recall the statements which a more accurate study made him desirous of modifying; but entreats the reader not to pass any opinion till he has read the whole, and is enabled to make the necessary corrections for himself. The work contains a very interesting and simple history of the disease in St. Petersburg, which, at the present moment, may not be unacceptable to our readers. We shall take the liberty of making the corrections to which Dr. Müller refers, and of throwing his observations, without reference to the dates at which they were written, into one consistent whole. In compiling, in this way, a short history of the epidemic in St. Petersburg, we shall avail ourselves of Dr. Adair Crawford's able pamphlet, which supplies certain deficiencies in Dr. Müller's Reports. The joint descriptions will enable us to give a tolerably full account of the phenomena of the disease. Our readers will understand that, when we do not expressly refer to Dr. Crawford, we derive our information from Dr. Müller.

The epidemic of cholera may be said to have commenced in St. Petersburg about the beginning of June, 1848. Dr. Crawford mentions that a solitary case had occurred in one of the military hospitals six weeks previously; the man died in twelve hours, and presented the characteristic symptoms and post-mortem appearances of Asiatic cholera. The number of cases in the first week was small, but nearly all the patients died: the disease increased rapidly during the second week, appearing in every quarter of the city, and chiefly among the poor. It attained its acme, according to Dr. Crawford, at the fifth week, and then gradually declined. Dr. Müller gives a tabular view of the number of cases from the 12th of June (old style) to the 7th of August. On the 12th of June, 100 people sickened, and 33 died; on the 13th and 14th inclusive, 896 were attacked, and 434 died; on the 16th, 595 were attacked, and 356 died. During these days none were reported as cured; that is to say, the recoveries were not perfected. On the 17th, 719 were attacked, 356 died, and 41 were cured; on the 21st, 1000 were attacked, 534 died, and 129 were cured; on the 22d, 1064 were attacked, 553 died, and 134 recovered; on the 23d, 1086 sickened, 548 died, and 167 recovered. This was the highest number reached; so that the acme was attained in the third week, according to Dr. Müller, as the first cases occurred in the early days of June. On the 24th of June, 964 only were attacked, 554 died, and 193 recovered. On the 25th of June, the number of attacks increased again to 1017, and the deaths reached their highest number of 576; the recoveries increased, however, to 222. On the 26th, the attacks fell to 853, the deaths reached 574, and the recoveries were 172; on the 27th, the fresh cases were 764, the deaths only 489, and the recoveries 242. After this date the number of fresh cases decreased daily. Sometimes the number fell as many as 90 in a day; at other times the decrease was only 2 or 4; while, in some instances, the number of attacks surpassed those of the day before. But, on the whole, there was a steady and gradual decline from the 23d of June to the 7th of August, when the table terminates, and on which date 34 fresh cases were reported. Although the decrease occurring on any one day varied considerably, yet there appears, when imperfections in the table are equalized by clubbing the days together into weeks, to have been a very remarkable uniformity in the rapidity of subsidence. The disease was at its height on the 23d of June, when 1086 fresh cases were reported; on the following day there were 964 fresh cases, and the day after 1017. Then followed a fall to 853. Now, taking this latter date as the first day after the acme, the following curious ratio is brought out. In the week from the 26th of June to the 2d of July inclusive, there were 4787 fresh cases; the average daily decrease was 60. In the second week, from the 3d to the 9th of July inclusive, there were 2368 fresh cases; the average daily decrease, if calculated from that of the former week, should be 29·6; it was actually 27·4. In the third week (10th to 16th of July) the number of fresh cases was 1266; the average daily decline, if calculated from the first week, ought to be 15·8; it was actually 15·4. In the fourth week (17th to 23d of July), the number of fresh cases was 687; the average daily decline should be by calculation 8·6; it was 9·7. In the fifth week (24th to 30th of July), the number of fresh cases was 360; the average decline, by calculation, is 4·5; actually 3·8. In the sixth week (31st of July to 6th of August inclusive), the number of fresh cases was 238; the daily decline, by calculation, 2·9; actually 2·1. And when it is remembered how very imperfectly returns, such as those given by Dr. Müller, are made up—how frequently cases that occurred days before are included in the return of a certain day, because they happened to be then first reported—the regularity of the above decline will appear still more striking. The propensity of a morbid poison to develope itself in this way—to attain an acme, and then regularly to decline[*]—is perhaps more marked in the case of cholera than in any other agent of this class; but similar laws seem to govern the spread of yellow fever, bubo-plague, and even, under certain conditions, the specific agents which develope themselves more readily in the human organism, and in the propagation of which contagion is a more important element, such as smallpox and typhus. In the case of all morbid poisons, this law appears to be itself under constant modification from the influence of disturbing causes.

The rapidity with which the epidemic attained its acme at St. Petersburg was much greater than that of its retrocession. From the 12th of June, when Dr. Müller's table begins, to the 22d inclusive, the average daily number of fresh cases was 87·6. It may be considered to have been at its height from the 21st to the 25th inclusive.

In 1832, the disease lasted 90 days in St. Petersburg; this time it only lasted 65 days; it attained its acme much more quickly, and passed away also more rapidly, and yet it carried off more than twice the number of its former victims, and would in all probability have been more fatal, if nearly 100,000 people, a fifth part of the inhabitants of the city, had not at once left. (Müller, p. 26.) Dr. Crawford states that about 50,000 of the poorer class left the city, but that many of them died. Up to the 20th of July, there were attacked in St. Petersburg 20,055 individuals, of whom 11,217 died. If the population be taken at 450,000, and we subtract 50,000, as the number of those who deserted the city (Crawford, p. 12), the mortality to population is as 1 to 35·5; and this terrible mortality does not express the whole amount, as a great number of cases occurred after the 20th of July till nearly the end of August, of which we possess no certain account. The ratio of mortality to attacks is a fraction below 56 per cent., a proportion which we shall hereafter see is singularly in accordance with the ravages of the epidemic in entire Russia. After the 9th of August, there was again an increase in the number of cases; of the amount we have at present no information; but Dr. Müller says they almost doubled, i. e. increased from 34 to 68 per diem. Lately again, in November, the disease is reported to have reappeared with some violence, but we have not yet obtained any information on the subject.

---

[*] In India, the duration of an epidemic attack in a moving body of men, as of a regiment on the march, is about four weeks; the acme is reached between the 5th and 15th day, and the decline occupies from 14 to 20 days. (Lorimer's Report, p. 10.)

Unfortunately, neither Dr. Müller nor Dr. Crawford has furnished us with any precise meteorological observations during these six weeks. There appear to have been very great irregularities, both as to temperature and barometrical pressure. Dr. Crawford says:

> "During the latter end of May and the whole of June, a remarkable change took place in the weather. There were almost constant high winds, shifting frequently and suddenly round to every point of the compass, and often accompanied by torrents of rain and sometimes thunder. This disturbed state of the atmosphere was indicated by sudden fallings and risings of the barometer, sometimes to the extent of between one and two inches. (!) The changes of temperature were equally frequent and rapid, the heat being, for several days together, very great,—as high as from 84° to 90° of Fahrenheit, and the air exceedingly sultry and oppressive, and a damp, relaxing south wind; and then suddenly, on a change of wind, and sometimes on the occurrence of a thunderstorm, this oppressive heat would be succeeded by great cold, the thermometer falling as much as 50° in a few hours, so that it was several times in June as low as the freezing point. (!) Another peculiarity in the condition of the air was the disturbed state of its electricity. This was clearly demonstrated by the fact, that the electric machines could not be charged, and to a great extent lost their power, as generally happens whenever the atmosphere is damp and unsettled." (Observations, p. 5.)

Müller gives a similar account:

> "Throughout the whole duration of the epidemic, the air was oppressive, heavy, and very changeable in temperature; thunderstorms occurred often, but had no influence, either on the number of attacks or recoveries. Rain fell almost daily; the heavens were gloomy, the evenings foggy, and the sun was seldom visible." (Report, p. 5.)

These meteorological conditions and vicissitudes seem to have had but slight influence on the disease; it appears to have held a determinate course, and in the table to which we have alluded, we can find no indication that the fall of the barometer, even to the extent of an inch, and that of the thermometer 50° Fahr. in a few hours, materially influenced the regular development, and as regular decline, of the epidemic. In this attack at St. Petersburg, the cholera appears indeed to have shown on a small, what it has now twice taught us on a large scale, viz. that the conditions which favour its development are comparatively independent of temperature, barometrical changes, and electrical vicissitudes. Dr. Müller says that the analysis of the atmospheric air, as well as of the air in the hospital-rooms, was determined by several philosophers with the greatest accuracy, but without the slightest peculiarity of result. Reagents did not in the least indicate the presence of ozone. "A particular influence of the air on animals and vegetables is not yet determined," he says, "yet the leaves this year withered early, a mortality among the fish was observed, and the swallows altogether left the city."*

Dr. Crawford believes that the development of cholera was, on this occasion, influenced by the long three weeks' fast which is held in the month of June. "It may be deserving of notice, however," he says, "that the decrease of the cholera corresponded with the cessation of the fast, which ended the 29th of June." (p. 12.) But this is evidently a mere hypothesis, and probably incorrect; we have already seen that the disease was at its height on the 21st and four following days, and commenced regularly to decline from the 26th. On the 29th, there were only 692, and on the 30th, 602 fresh cases, instead of 1086 on the 23d. So that its retrocession, and the cessation of the fast, were anything but coincident.

The topographical site and climate of St. Petersburg seem to give the usual conditions which the virus of cholera demands for its development, viz. moisture and effluvia. The city is scarcely raised above the level of the Neva, and is

> "Intersected by several large canals, in which the water has generally very little current, and emits in hot weather damp effluvia. These natural disadvantages are, however, very much counteracted by the remarkably good and beautiful construction of St. Petersburg, the streets being wide and airy, and squares spacious, and by the great attention also paid to their cleanliness." (Crawford, p. 7.)

Many of the dwellings of the poor, we are told, are habitually damp; and from the low country round the city there arises, in the hot summer evenings, a thick damp mist which extends over the whole district. The climate is, in spring and summer, damp and relaxing, and the lower classes are subject to intermittent and typhus fever, bowel complaints, and scorbutus. We do not discover whether any particular part of the town was remarkably affected, nor are we informed whether the island Kristofsky, which was so singularly exempted during the epidemic of 1831, was, on this occasion, similarly spared.

The poorer classes suffered, as usual, much more than the rich. The ratio of attacks among the several classes is thus given by Müller:

Fishermen, labourers in the open air, coachmen, &c. . . . = 4-10ths.
Artisans, hand-workers . . . . = 3-10ths.
Higher ranks . . . . = 2-10ths.

Dr. Crawford mentions that very few of the English residents, who are almost all in easy circumstances, were attacked.

The proportion of the sexes to attacks was as 5 men to 3 women. It should be remembered, however, that at St. Petersburg there is a great disproportion between the sexes; the males bearing to the females the relation of five eighths to three eighths.

---

* Dr. Crawford states, that the crows forsook their usual roosting-places in the public gardens of the city and suburbs, and migrated to the nearest high ground. Dr. Müller relates the same of the swallows, as we state in the text,—they left the city, he says, at the commencement, and returned at the close, of the epidemic! It would therefore appear, that the European crows are much more sensitive than their Indian brethren, who treat the most violent outbreaks of cholera with the utmost indifference.

The amount of mortality to attacks at the commencement and at the end of the epidemic, cannot be correctly determined. At the commencement nearly all died (Crawford), but after the first week the mortality appears to have remained nearly stationary. In Dr. Müller's table it is singular to remark how regularly the number of deaths on any one day is rather more than half the number of those attacked on the same day. Thus, in the early days, before the disease reached its acme, say on the 16th of June, the fresh cases were 595, the deaths 356. On the day when the highest number, viz. 1086, were attacked, 548 died. A month later, on the 23d of July, the fresh cases were only 64, but the deaths were 45; and on the 6th of August, when the fresh cases had diminished to 24, the deaths were 16. And although, from want of detail on various points, particularly as to how many died in the cold stage or in the consecutive fever, or the average duration of life at different periods of the epidemic, the table is of very partial use in this particular, it would seem clear that the mortality of the disease did not undergo any considerable reduction towards the close of the epidemic. Müller states that at first more than half died, and afterwards about three eighths.

Before passing on to consider the various opinions entertained in Russia, on the nature and treatment of cholera, it may be advisable to glance at the course of the disease throughout the various provinces of that empire. The course of the cholera, according to Müller, differed considerably from that of the epidemic of 1831-2. In some places it broke out very suddenly and severely in the greatest cold of winter; and then suddenly appeared to be arrested. In Moscow, on the other hand, it did not disappear for a whole year, but occasionally lessened and again increased. Whereas, in 1831, the disease preserved a regular progress from east to west, it arose this time suddenly in an opposite direction, and passed in a northerly direction to the Black Sea. The wind had apparently no influence upon it; on the contrary, many were of opinion that it pursued a directly opposite course. The water-courses appeared, as formerly, to be chiefly affected; and there was noticed, as in 1831, a considerable mortality among the fishes. (Müller, p. 5.) At a subsequent page (36) Müller states that the present epidemic was chiefly distinguished from the former,—

1. By its sudden spread.
2. By the frequent jumps from place to place in contrary directions.
3. By its long duration and its frequent recurrence in the same place.

It prevailed in every government in Russia, except Poland. Even in Siberia (Tobolsk) it broke out severely. It raged sometimes very severely at considerable heights, and was not, as formerly, almost confined to the plains. In Moscow, it wandered from one quarter to another, and after eleven months its average mortality had not sensibly decreased. From the 16th of October, 1846, to the 23d of June, 1848, the number of known cases of cholera in all Russia was 200,318, of which 116,658 were fatal, or at the rate of 58 per cent., a ratio nearly the same as that of St. Petersburg. If the population of Russia be taken as 56 millions, there was therefore one death to every 479 of population.

Dr. Crawford gives very different numbers from the above. He states that, according to accounts he has lately received, above 800,000 persons have fallen victims, or at the rate of 1 death to every 70 of population. (Appendix, p. 23.)

In the 'Archives Générales' for September 1848, is a very interesting paper by M. Lasègue,[*] on the course of the cholera in Russia. He differs from Dr. Müller, in ascribing to the phenomena of the present epidemic a very remarkable similarity to those of the former attack. Without relying too implicitly at present upon his statements, we may give here a short abstract of his paper. M. Lasègue was sent by the French Government to Russia, and followed step by step the course of the disease. He remarks that it is necessary to take in great distances, in order to judge of the mode in which it travels. It appears to have two modes of propagation: 1st, in obedience to a force which impels it over vast tracts of country, in a certain and determinate direction; 2d, in obedience to subordinate laws, which cause it to disperse itself over towns situated on its course, and to prevail in them for a greater or less space of time. When thus localized, it seems to lose, as far as that particular city is concerned, its tendency to pass in a particular direction; and the laws of its transmission from house to house are not identical with those which impel it from province to province.

After having traversed, in 1846, and the early part of 1847, the country of the Kirguesses, the cholera appeared at Tiflis in May 1847, and passing along the borders of the Caspian Sea, ravaged Daghestan, and invaded Kislar, a town situated on the river Terek, a short distance from the sea. Thence it took two routes: one westward, along the course of the Terek; the other towards the east, by the borders of the Caspian, and the great route from Kislar to Astracan. On the 21st of June it entered Astracan, and prevailed there to the commencement of September. It then passed towards the north, along the Volga. The course of the river represents exactly, as far as Orenburg, the course of the epidemic, and formed its limit to the east. The provinces along the borders of, or traversed by, the Volga were successively invaded. On the 11th of August the disease reached Zaritzyn, and on the 25th, Saratoff.[†] In two months the cholera traversed 900 werstes (675 miles). On the 15th of September it was at Zimbirsk, and towards the end of October at Nijni-Novogorod. Then it attacked Orenburg, situated more to the east, and out of its direct course; and the deviation is the more remarkable, as the same occurred in 1830. While it was thus passing along the Volga, the cholera directed itself westward by Georgiew, Stawopol, the country of the Cossacks of the Don, &c., to the Dnieper, the course of which it pursued towards the north; and this river formed its western limit, which it did not pass. In the country comprehended between these two nearly parallel rivers (the Volga and the Dnieper), the disease almost circumscribed itself. From the end of September to the middle of January, it invaded successively the governments of Poltawa, Kiew, Tschernikoff, and Mohilew. From this last city it passed to Witebsk, where it became extinct at the end of January. In passing thus to Witebsk, it forsook the course of the

---

[*] M. Lasègue states, that the mortality of 1847-8 has been nearly the same as that of 1830-1. The inhabitants generally considered the disease to be much milder than formerly, probably from not regarding it with so much apprehension; but the official returns differ very little in the two epidemics.

[†] M. Lasègue confirms the observation we made in a former number, that Sarepta and other German colonies were this time, as in 1830, preserved from the disease, "although placed on its route, situated on the border of the river, and with the conditions of territory analogous to those of the neighbouring towns." Have the salt springs anything to do with this exemption?

Dnieper, in obedience apparently to the law which impelled it towards the north.

All the governments situated along the borders of the Russian empire, Moldavia, Gallicia, and the ancient kingdom of Poland, were at this time exempted. They became subsequently affected; but at a date comparatively much posterior, in point of time, to the period of attack in 1830.

The peculiar force which impelled the cholera thus towards the north, is not only indicated by the general course of the disease, but by a closer study of its peculiarities. Whenever the malady deviated, so to speak, from its normal direction, and passed towards the west, it seemed incapable of propagating itself, and died away spontaneously, even in places which appeared to be well fitted for its reception. If on its route it encountered a large and densely-peopled town, it arrested itself there by preference; but the existence of a similar town out of the line of its course did not seem to be able to attract it, or to cause it to wander from its singular march. The exemption of the provinces to the east of the Volga might be perhaps explained by the nature of the country, in which the inhabitants are few and scattered; but the rich, fertile, and densely-peopled countries to the right of the Dneiper enjoyed an equal freedom from attack, which can only be explained by the fact that they were situated beyond the line of the disease, which, as already stated, was principally between these rivers.

Although communication was not interrupted, as the system of *cordons sanitaires* was abandoned by the government, no effect was produced by this cause. The most frequent intercourse with the dense populations of the west, living under very unfavorable hygienic conditions, even the lowering of the temperature, which it might be supposed would have caused the extension of the disease towards more temperate regions, produced no effect.

It was difficult to measure the rapidity with which the disease propagated itself, since it was difficult to say at what period of its development it left infected places to pass towards those it was going to attack. It did not advance with any regular and continuous progress: sometimes it traversed very rapidly a certain extent of country, sowing, as it were, the seeds of the malady; then it suspended its march, ceased for some time to spread, and then at last recommenced its movement. On taking, however, great distances into consideration, it was evident that it never travelled faster than from 400 to 500 kilomètres (250 to 312 miles) per month.

Subjected always to the law that the localities were situated on its line of projection, the following circumstances seemed to influence the localization of the disease. The size of cities had a great effect. This is evident from the fact that, through the whole course of the epidemic, no capital of any government was spared. When the disease began to decline at the approach of winter, the great cities were alone attacked, while towns of the second order were spared.

The disease always followed frequented routes and water-courses on which navigation was active, equally in 1847 as in 1830. The governments which were most exempt in 1830-31 were this time also less severely attacked.

In the winter, the cholera lessened or disappeared; it reappeared with the first warmth of spring. In all these particulars the cholera of 1847 has manifested a singular conformity with the laws which seemed to govern it in 1830. In comparing the dates of attack in different governments, in the two epidemics, some very striking analogies are discovered. The following table of the principal cities of the governments, and of the dates of attack in 1830 and 1847, is given by M. Lasègue:

	WHEN ATTACKED.	
	1830.	1847.
Tiflis	July 1st	May 5th
Kislar	Middle of July	End of May
Astracan	July 31st	June 21st
Zaritzyn	August 13th	Middle of August
Saratoff	August 20th	August 25th
Zimbirsk	September 8th	September 15th
Kazan	October 4th	September 17th
Georgiew	End of July	End of June
Stawopol	September 6th	July 16th
Novo-Tscherhask	September 10th	July 13th
Taganrok	October 8th	August 15th
Kiew	January 8th	October 5th
Mohilew	End of January	October 18th
Karkhoff	September 23d	August 23d
Woronesch	End of September	September 5th.

Too much stress must not be laid upon these coincidences, curious as some of them undoubtedly are. Moreover, we find important exceptions. Odessa and Orenburg, also, both suffered at periods differing from each other, in the two epidemics.

In the further progress, there are also some singular coincidences. Thus, cholera appeared at Moscow in the beginning of October, 1830; at the end of September, 1847. It appeared at St. Petersburg on the 25th of June, 1831; and in the beginning of June, 1848. The first cases occurred in Berlin on the 31st of August, 1831; about the 12th of August, 1848. In our own country the coincidence has failed, for cases of cholera have already occurred in places situated in all parts of the kingdom, from Edinburgh to Plymouth, within a short space of time.

If it be correct that the cholera has thus followed a route so similar to that which it took in 1830-31, and has produced a mortality so nearly the same, it is certainly a remarkable circumstance that the two epochs should have differed altogether in their meteorological characters. The summers of 1830-31 were very dry and hot; during the late epidemic, on the contrary, there have been great vicissitudes of temperature, and there was a very great quantity of rain.

Returning now from this digression on the course of the epidemic throughout Russia, to the disease in St. Petersburg only, we may consider the *symptoms and post-mortem appearances* of the disease. In his several Reports, Dr. Müller describes the symptoms rather differently. In his first Report, after mentioning that almost everybody, during the prevalence of the epidemic, experienced certain uncomfortable feelings, diarrhœa, flatulence, and pains in the lower extremities, he goes on to state that three characteristic forms of cholera were distinguished, the rheumatic, gastric, and inflammatory. The propriety of these terms appears, however, even from his own description, very questionable; the first form appeared after exposure to cold, and was denoted by the severe convulsive pains. It was frequently followed by dropsy and scurvy. The second variety was the most common; it was distinguished by longer gastric premonitory symptoms, and by the large quantity of matters vomited and passed by stool. It also passed frequently into consecutive typhus. The "Cholera inflammatoria, or sicca," was denoted by its great intensity, by the unimportance, or entire absence, of the discharges from the intestinal canal, and by its rapidly fatal issue. Vigorous and robust individuals, especially, perished by "sudden paralysis." When it did not prove fatal, dysentery and diarrhœa were its most frequent sequelæ. The symptoms constant in all the forms were the rapidly appearing ice-coldness (even of the tongue), the heavy, sighing respiration, the thread-like, soon entirely-interrupted pulse, and the almost completely suspended functions of the kidneys.

While there is no doubt that cases occur in all epidemics, which come more or less under one of the above forms, the distinguishing appellations used by Dr. Müller appear to us very ill chosen. The violent convulsive form is not allied to rheumatism; it is seen in countries in which rheumatism is very uncommon; it is very doubtful whether rheumatism, if sometimes a sequel of cholera, ever accompanies the characteristic stage. The third, or dry form, again, is not inflammatory; it is a misnomer so to call it—it is merely the extreme form of the disease. If any distinctions are admitted, the "dry" and the "gastric," as distinguishing the severe form with moderate discharges, and the slighter form with great discharges, are perhaps the most accurate terms.

In his second Report, Dr. Müller gives a second and longer description of the symptoms, for the more important portions only of which we can find room. He here abandons all distinctions of varieties, and describes merely the stages of the disease. Of those he makes four, viz. "stadium prodromorum, invasionis, algidum, et reactionis."

1. The *premonitory stage* was generally present; it lasted from two to eight days; the digestion was disturbed; there were flatulence, diarrhœa, languor, &c.

Whether these symptoms were present or not, after some fault of diet, or exposure to cold, or mental emotion, the cholera itself suddenly came on.

2. The *stadium invasionis* was characterised by diarrhœa, vomiting of the peculiar cholera-matter, cramps, and a heavy burning paroxysmal pain at the navel. This condition sometimes lasted minutes, sometimes hours, till—

3. The *stadium algidum* seu *paralyticum* appeared. In this stage the patient fell, sometimes as if struck by lightning; the extremities became ice-cold and blue; the skin became wrinkled; the eyes fell back in the head, and deep blue circles surrounded them; the conjunctivæ were generally reddened with venous congestion; the lips were duskily blue; the voice was peculiarly hoarse, hollow-sounding, and tuneless; the already interrupted pulse was lost entirely, or remained only like a thread; the heart's action was heavy and slow, the rhythm irregular, the first sound unheard; the respiration was heavy and sighing; the evacuations continued; hiccup sometimes occurred; consciousness did not fail; the patient lay with half-closed heavy eyes; he wished merely to satisfy his craving thirst, and complained only of the burning pain at the navel; the abdomen was seldom tympanitic, and pressure gave no uneasiness. The cramps con-

tinued, particularly in the extremities, and ceased and recurred at intervals. This state lasted for hours, or even days.

4. The *stadium reactionis* approached sometimes very rapidly, sometimes slowly and gradually. The skin became a little warmer, and a general warm perspiration appeared; the heart's action was revivified; the pulse became stronger, but remained slow; the respiration was uniformly hastened; the discharges, if they had continued, gradually abated, and all the phenomena of the algide stage faded more or less rapidly. Then ensued affections of the brain and lungs; and the quicker the reaction, the more were these affections and gastro-intestinal inflammations to be apprehended. The affection of the brain often came on very rapidly; persons of whose recovery great hope was entertained sometimes became almost suddenly soporose; others fell into delirium, with stupid heavy glance, and a slow (40—60) labouring pulse. The temperature of the skin sank again. Dissection showed only congestion of the brain and its coverings; very seldom did true meningitis occur. Erysipelas faciei, furunculus, and a peculiar exanthem (which consisted of red spots, running a course of from one to three days, and terminating in extensive epidermic desquamation), often followed this state.

The affection of the lungs was almost always combined with this cerebral congestion, or with the next form of reaction, which is termed typhus.

This peculiar typhus, which Schönlein considers a fifth stage of the disease, is not so constant that it can be considered absolutely necessary. It differs from true typhus, which in many respects it closely resembles, in running a rapid course, with comparatively little intestinal, but much cerebral and lung affection, especially hypostatic pneumonia, and œdema.

Another sequence was a very peculiar one; there occurred, particularly in feeble subjects, a cachectic state, with a dry cool tongue, entirely prostrated nutritive power, watery intestinal discharges, and rapid emaciation. This form generally proved fatal, and after death, there appeared to be an entire absence of the epithelium of the intestinal mucous membrane, and considerable redness of the mucous coat itself.

In other cases again, in perhaps an eighth part of the whole, recovery took place rapidly in a few days; these were the slightest cases, in which treatment had been early applied. In such cases, the tongue was soon warm and moist, critical sweats occurred, dark soup-like intestinal motions, mixed with bile, followed, and recovery was at once completed.

Other sequences, such as dysentery, dropsy, and scurvy, occasionally showed themselves, and seemed to be attributable to the profound disorder of the assimilating processes consequent on the disease. They varied in frequency with the type of the disease, and with the locality in which it prevailed.

In this description of the several forms of reaction, which agrees closely with the admirable delineation of the four primary forms of consecutive fever by Dr. Keir,* viz. inflammation or sub-inflammation of the intestines, inflammatory irritation of the lungs, synochus biliosus, or bilio-nervosus, and congestive sub-inflammatory condition of the brain and spinal cord, we notice one remarkable omission. It is, that the state of the blood, particularly as to the enormous quantity of urea it contains in some forms of consecutive fever, is not even hinted at. Indeed, throughout the whole of Dr. Müller's treatise, the blood is passed over with comparative neglect.

---

* A Treatise on Cholera, as it prevailed in the City of Moscow. By James Keir, M.D., &c. &c Edinburgh, 1832. p. 62.

The *prognosis* was considered very unfavorable if the algide stage had once set in. If it reached its highest point, not one in 13 was cured. Yet cases were certainly seen, in which patients utterly pulseless, and with a most ghastly countenance, did recover. If the disease was seized in its premonitory stage, its further progress was easily arrested. If the stadium frigidum was moderately developed, before medical assistance was obtained, it was hardly possible to save 1-5th or 1-6th of the patients. Age, rank, and profession seemed to have no certain influence on the prognosis, except that children were generally less severely attacked. Individuals enfeebled through excess, and particularly through drunkenness, generally died; yet it could not be denied, that the strongest and most healthy men were sometimes struck down like lightning, and carried off. Indeed, there was some reason to suppose that adult men, of strong robust constitution, were particularly endangered; for, among 22,000 patients, 17,300 were in persons between 18 and 40 years of age, only 450 were under 2 years, and 227 over 65 years of age. Suckling and pregnant women were frequently attacked, and generally died.

The peculiar *predisposition*, although generally admitted to be necessary for the production of the disease, was not at all understood. One singular observation was made by Dr. Pirogoff, that out of 500 bodies, there was almost no case of recent tuberculosis, a disease at other times very frequent. The higher ranks were, during this epidemic, more affected than in 1830-1. Dr. Müller states, that throughout the whole epidemic the observation has been made that the use of unripe fruits and of greens predisposed to cholera. There was remarked also a singular desire for bracing nourishment, meat, and wine.* Dr. Crawford makes similar remarks. We are of opinion, nevertheless, that the evidence on this point is incomplete. Cholera has affected all countries, however different may be the modes of living and the habits of the inhabitants, with such uniformity, that we suspect the peculiar predisposition is less dependent on diet than on other circumstances. Yet, of course, while doubts exist on the subject, every prudent person will concede the necessity of caution. Dr. Müller after condemning vegetable food, states that to their better nourishment, and to their more regular mode of living, the higher classes owed their comparative exemption. But then he has introduced at once another element into the argument, viz. the use of animal food. It would be just as fair to argue, that it was the want of animal, and not the use of vegetable, food, which aided the action of the poison on the lower classes. He has disregarded also the other circumstances, in respect of which the higher classes are more favorably placed than the lower.

The *post-mortem appearances* are detailed at some length, both in the first and second Reports. We need only extract the following points of interest:—

The membranes of the brain, and the brain itself, were generally much congested; all the vessels were loaded with a peculiar blood; there was much serum in the meshes of the pia mater and in the ventricles, and

* Op. cit., p. 36.

this was most marked in the prolonged cases. The brain was generally tough and firm, and of a remarkably white colour. At the commencement of the epidemic, and chiefly in the most rapid cases, the brain was, however, soft. The spinal cord was also firm, with congested vessels and serum in the subarachnoid space. In the arachnoid were often little corpuscles. At Moscow, this appearance (which otherwise is often seen) was held to be characteristic of cholera. (?) [The note of interrogation is the author's.]

In most cases, particularly at the commencement of the epidemic, and in rapidly fatal cases, there was a peculiar oily feel of the pleura, and, in a less degree, of the pericardium.

The heart was full generally of black fluid, or clotted oily blood; in many cases, also, there were large fibrin-coagula; sometimes there existed ecchymotic spots on the pericardium, in other cases there was great flaccidity of the muscular substance.

There was a remarkable anæmic condition of the capillary system in the substance of the lung, while from the larger vessels black thick blood, in greater or less quantity, flowed. The lungs were pale, whitish, and very dry to the feel. If the patient died after reaction had occurred, there were seen ecchymotic spots, hypostatic pneumonia, or acute œdema. At a still later period, there was still more œdema, and hypostatic lobular pneumonia, with soft red hepatization.

The liver was anæmic in the capillary system; in the larger vessels there was a variable quantity of blood. The gall-bladder was generally filled with a blackish thick and oily bile.

The spleen was small; its coat was shrivelled; there was nothing else remarkable about it, unless it had been affected by previous intermittent fever, which was observed to be a frequent antecedent of cholera.

The mucous membrane of the stomach was soft and white, and very often there were ecchymotic spots. In a third part of all the cases, undigested food was found in the stomach.

At the commencement of the epidemic, the small intestines were generally found pale and anæmic. Afterwards appeared, throughout the whole small intestine, and particularly in the jejunum, intense "hyperæmia," and in protracted cases, there was effusion of blood or a bloody mucus. The solitary and Peyerian glands were enlarged, and there was swelling of the whole villous coat. On the Peyerian patches this swelling resembled exudation-formation.

The epithelium was throughout thrown off, and appeared to form part of the tough masses lying upon the mucous coat. The peculiar white flocks of the cholera-contents of the intestine seemed to be composed, sometimes altogether, of epithelium.[*]

In protracted cases, the Peyerian glands and all the lower portion of the ileum presented the phenomena of the typhus-process in an early grade; the typhus-product[†] assumed a peculiar punctiform arrangement, and did not readily proceed to its proper ulcer-formation.[‡]

---

[*] Müller remarks in this place, also, that the intestinal contents were *acid*, and contained much albumen. We have invariably found the intestinal contents alkaline, unless there is so much dilution as to make the reaction so faint as to be called neutral. Epithelium, also, according to our observations, forms only an occasional element of the cholera flakes.

[†] The "fièvre typhoide" is the typhus abdominalis of most Germans. In the true "English typhus" there is no exudation into or under the patches of Peyer.

[‡] There appears to be another difference in the morbid anatomy of the cholera-typhus; the spleen is not so much affected, although it is often somewhat enlarged and softened.

The large intestine was often anæmic; the mucous and muscular coats were corrugated and plaited in a remarkable manner. Brine-like gray masses, composed of mucus and cast-off epithelium, lay on the surface. In cases with long-continued diarrhœa, there were ecchymotic spots.

When the masses adhered firmly to the mucous membrane, it was generally observed that there were ecchymotic patches beneath them. In a few cases, and especially in the sigmoid flexure, grayish exudation-masses were found, with which were sometimes very much altered blood-corpuscles. When these were stripped from the mucous membrane, ecchymoses were disclosed. Apparently, Dr. Müller intends to state that these last appearances were present in the protracted cases, and particularly when there was dysentery.

Mesenteric glands. In all true cases they were somewhat developed; infiltrated as in typhus, and pale.

The *Urinary and genital organs*, showed nothing remarkable. The peritoneum, like the pleura, had an oily feel.

The bodies of those who died in the algide stage were easily recognised by the universal, rapidly-developed, and intense rigor mortis, and by the dark colour of the skin.

The above description proves, if such proof were necessary, how exactly the characters of the disease at the present time correspond with those witnessed in 1830-2. To this description of Dr. Müller's, or rather to certain portions of it, we shall have occasion to return on a future occasion; at present we must proceed to a very interesting subject, viz. the opinions entertained by the Russian physicians as to the *nature and essence of cholera.*

On these points, the majority of the physicians of St. Petersburg were divided into two parties. The one assumed, as the primary lesion, a sudden affection of the nervous system, and especially of the spinal cord; the other believed in a disease of the intestinal canal, with passage of the fluid elements of the blood, and explained the prostration of the nervous force as a sequence of the diseased blood.

The first party alleged for their opinion the following arguments:

1. Disturbances in the nervous system preceded the outbreak of the epidemic; all diseases bore a nervous character, while diseases of the blood, and particularly inflammations, were seldom seen. Among the lesions of the nerves, intermittent fevers were the most frequent; neuralgias of all kinds, and particularly pains of the face and head, and dragging, tearing pains in the extremities, were common.

2. The premonitory symptoms were non-essential, and merely indications of the deranged innervation. In many cases the disease broke out so suddenly, and destroyed the action of the centres of the vital force, and especially of the spinal cord, so rapidly, that it is impossible to name any other seat for the evil.

3. Recovery followed so quickly in many cases, that it was impossible that any important lesions of the intestinal canal should have occurred. There occurred, not seldom, severe cases, in which the patients were restored to their ordinary health, after three or four days of unimportant debility.

4. When diseases remained as sequences of cholera, they were always affections of the nerves; there was disease of the brain, sopor, delirium, mania, and these symptoms produced a disease resembling typhus, but not

true typhus, as the kind of pulse and the post-mortem appearances proved. Other diseases were considered accidental.

5. A cure was never obtained through the customary modes of controlling irritation of the intestinal canal.

6. This sudden lesion of the nervous system was produced by the variations of the general electricity. The electrical equilibrium was destroyed, and from all bodies, even when isolated, electricity passed off. The air was overladen with electricity, as proved by the frequent storms. Many physicians, adopting this opinion, devised the following method of treatment: they wrapped the patients in woollen covers, and laid them on beds completely isolated; they then rubbed strongly the whole surface with oil, covered the patient warmly, and maintained that they could thus rapidly reinduce the bodily warmth. In this way they supposed that the electricity of the nerves was promoted, and its escape into the air hindered.

On the other hand, the supporters of the second opinion, that cholera has its seat in the organs of nutrition and in the blood, advance the following grounds of belief.

1. A premonitory stage was almost always found by careful observation. Epigastric oppression, nausea, and diarrhœa preceded all nervous symptoms. If this stage was properly treated, true cholera hardly ever appeared. The lightning-like sudden attacks of cholera are altogether denied. After vomiting and diarrhœa had lasted a long time, and a great part of the fluid elements of the blood had passed into the intestinal canal, then followed rapidly the nervous lesions, the icy coldness, and the other terrible symptoms.

2. The rapid recovery was only possible when the disease was slight; yet even such cases were rare. A sensitive state of the intestinal canal remained long after cholera, as denoted by the frequent relapses, the red dry tongue, &c. &c.

3. Dissection gave such constant characteristic appearances, that the observer was constrained to derive the primitive disease from injury of the circulating system and the chylopoietic viscera, rather than from the nervous system.

In reference to these two opinions, Dr. Müller remarks:

> "The debate on the nature of cholera will not long endure; it will disappear, with the examination and knowledge of other epidemics; more frequent observation, and especially the estimation of the pathological results of dissections, will throw more light on the subject. In the meanwhile, the opinion that the intestinal canal is the seat of the disease finds daily more followers, particularly among those who diligently study the morbid anatomy. It is unquestionably the most correct." (Op. cit., p. 23.)

We do not think that the discussion thus briefly introduced redounds particularly to the credit of the Russian physicians on either side. The arguments for the "nervous" hypothesis are, for the most part, peculiarly weak, and fall almost without controversy; those which are not so, speak nothing for the hypothesis. We particularly object to the final argument derived from treatment; in the first place, it is entirely gratuitous to argue that disturbed electrical equilibrium is an invariable coincident phenomenon with cholera; it is only occasional and unnecessary; although, when it does occur, we will not deny it a possible, but as yet undetermined, influence. Secondly, that patients were cured in the mode assigned, we do entirely, and without reservation, disbelieve; where is the evidence, the potent figures, which we in these days require, before a statement of this kind can be held to be justifiably made?—how is it that, if this method were effectual, the mortality in St. Petersburg remained so high? It is very certain that, if any method be successful in cholera, it will soon meet with universal adoption. The uniformity of the mortality in all countries proves the inadequacy of all treatment.

The arguments of those who locate the disease primarily in the intestinal canal are also, it appears to us, overthrown by a closer study of the phenomena of cholera. It is also decidedly erroneous to assert, that premonitory symptoms invariably precede cholera; while it is as decidedly an error, on the part of those who advocate the first hypothesis, to underrate their importance.

This is not the proper occasion for a discussion on the nature of cholera; fresh light may be thrown on the question by the study of the epidemic now traversing this country; to enter now into the subject would be premature, and would indeed require more space than we can at present accord to it.

We pass on to a very brief notice of the Russian mode of treatment.

In several places Dr. Müller states that the arrest of the premonitory symptoms prevented the full development of the disease; and so important was this deemed, that Dr. Crawford informs us that printed instructions were issued, to enforce upon the poorer classes the necessity of attending to the state of the bowels. Whether these proclamations were unheeded, or whether the measures recommended were inert, does not appear; but it is a matter of deep importance to us, who are now threatened with this plague, and are about to attempt to ward it off by measures of a similar kind, that the mortality in St. Petersburg was not reduced below that of 1830-1, by acting upon the knowledge of the fact that arresting the premonitory diarrhœa often arrests the disease. The fact is certain; its practical applicability to a dense population very doubtful.

The treatment of the developed stage of cholera was at St. Petersburg, as everywhere, as manifold as the opinions on the nature of the disease. In the third Report, Dr. Müller gives the following as the treatment which claimed the largest share of success. It was that pursued in the Military Hospital at Krasne-Selo, four miles from St. Petersburg, where 70,000 men were assembled. Whilst at all the other hospitals the mortality averaged between 50 and 60 per cent., at this hospital, out of 432 severe cases occurring from the 10th of July to the 11th of August, only 145 died, or at the rate of 33·5 per cent. As soon as a patient was in the stadium algidum, 12 cupping-glasses were placed along the spine; the whole body was strongly rubbed with a mixture of pounded ice and salt (two parts ice to one part salt); then the patient was enveloped in woollen coverings, and drank every hour a glass of water, with from one to two teaspoonfuls of rock salt dissolved in it; and, according to circumstances, in addition, calomel was given with or without camphor. Stimulating remedies were employed with great moderation and care, and at the least sign of reaction immediately laid aside. If, after these measures, warmth did not return in from one and a half to two hours, the frictions were repeated—once or twice generally sufficed, but occasionally they were repeated three or four times.

The other modes of treatment were very various; the following were those chiefly in use:

In the premonitory stage, sometimes an emetic of ipecacuanha, and various warm stimulating medicines, such as valerian, peppermint, a small quantity of ipecacuanha wine, with opium.

In the developed stage, antiphlogistics were at first used, but afterwards stimulating remedies were more in vogue. An emetic of ipecacuanha was sometimes very serviceable. In stadio frigido, vapour-baths, which were produced by the evaporation of vinegar and water on hot bricks placed under the bedclothes, were much used. Frictions with various substances, especially with a strong solution of rock salt, found many advocates; the frictions were repeated two or three times an hour; cloths wetted in the solution were applied, and the patients then covered with woollen clothes. Calomel in small doses, with camphor, was employed by some. Valerian, musk, serpentaria, the various ethers, &c., were much used. A large mustard poultice, over the whole abdomen, was found to be beneficial.

At the least appearance of *reaction*, it was found necessary to discontinue all stimulating remedies, and to use antiphlogistic measures and calomel in large doses. For the head affection, blisters, tartar-emetic ointment, local bloodlettings, were employed.

During *convalescence* great care was necessary to prevent relapses. In the diarrhœa which sometimes remained, nitrate of silver and nitric acid were found efficacious.

The homœopathic physicians boasted of considerable success. In the homœopathic sick-houses, the accuracy of whose lists Dr. Müller investigated, the mortality was three-eighths of the sick, viz. 37·5 per cent.; these lists, however, included all the slight cases. The treatment consisted in swathing the body in cloths wetted with salt water, in frictions with spirituous substances, and the internal administration of veratrum album, ipecacuanha, and nux vomica. The veratrum, in particular, was considered a specific, and was supposed to act on the solar plexus and the vagus.

We do not think the homœopathists had much reason to congratulate themselves on their success. If the choleroid cases are included in the lists, the mortality of 37 per cent. is very high. We suspect that their success was about commensurate with the philosophy of their creed, and with the general truth of their statements, in neither of which do we put the slightest faith.

In the interior of Russia, after some experience, buttermilk, or "serum lactis alum." was recommended as an excellent medicine in cholera. Although it has not yet been sufficiently tried, "yet," observes Dr. Müller, "the method does not appear so irrational, because, perhaps, it repairs in the most rapid way the great loss and injury of the blood-serum." (Op. cit. p. 38.)

Seventeen cholera hospitals were established in St. Petersburg; they were admirably fitted up, very judiciously arranged, and furnished most liberally with everything necessary. Admission was denied to no one; the treatment was entirely gratuitous, and the use of remedies (including musk and champagne in some cases) unrestricted.

But most unfortunately, no one seemed to anticipate the rapid outburst of the malady; everything was prepared slowly, and when cholera suddenly and unexpectedly appeared, all the beds were rapidly filled, attendants could not be found, and many sick died altogether without help; some expired in the street, because it was found impossible to take them to the hospital. However, this neglect was very soon repaired; the magistrates, with the greatest zeal and promptitude, prepared places of refuge for the sufferers; the exhausted nurses were replaced with soldiers; the government appointed private physicians, who were richly remunerated, to the hospitals; and, as Dr. Müller remarks, when we consider the short space of time for the arrangement, the indifference as to expense, and the skill with which the preparations were made, it is impossible to refuse our admiration to the government.*

Dr. Müller goes on to remark, that the fitting-up of a cholera hospital is no such easy matter as some may think, and points out the necessity for the constant and unremitting attendance both of nurses and physicians.

We may indeed apply this lesson to ourselves, and from the example of St. Petersburg, may learn not too long to delay our own preparations, particularly in our overgrown metropolis. It is not probable that this plague will pass over our great city, with its impoverished, crowded, and miserable inhabitants, without more evidence of its power than we have yet seen.

What is it, indeed, which has already anticipated, so to speak, the normal march of cholera, and shown us, in London, cases of the disease four months before the time at which, according to our experience of the last epidemic, we should have witnessed them? It is certainly possible that the disease has wasted its force on continental Europe, and is about to pass lightly over us; but it is unfortunately more probable that the wretched sanitary condition of London has afforded a nidus for even the scattered drops thrown from the outskirts of the cholera shower, which otherwise would have fallen inert and harmless. At any rate, if it be true that Cholera has shown so remarkable a disposition to repeat the periods of its appearance in the localities it has now twice visited, it is as well to remember that it was in February 1832, more than five months after its appearance at Sunderland, that it reached London; it may, therefore, possibly acquire increased intensity in February 1849, and it would be wise to make in time the preparatious which such an occurrence would necessitate.

The importance of the point may excuse a short digression from the main subject.

The arrangements which an attack of cholera in an English country town would render necessary, may be easily made; they will differ only from those in London in their comparative simplicity and ease of formation. We confess ourselves anxious, however, for this huge city, with her two millions of people, a number of whom, also, are inevitably doomed to great distress during the approaching winter.

The means at present ordered for the purpose of affording aid to the poor attacked with cholera, are as follow:

---

* As in 1830-1, the people fancied they were poisoned, and a serious riot took place, which was quelled by the courageous and personal interference of the Czar.

1. The establishment of cholera dispensaries, at which medicines to restrain the premonitory diarrhœa may be at once obtained.
2. House-to-house visitation by local boards.
3. The establishment of houses of refuge, to which the sound members of a crowded house, or locality, may be conveyed, in order to remove them from the causes of the disease, and to lessen the dense population of the place.
4. The establishment of cholera hospitals.

Of the first two recommendations we shall say nothing; they come to us backed by strong evidence; let them therefore be tried in good faith and earnestness.

The establishment of "Houses of Refuge" is not a novelty in the preventive treatment of cholera; as they were tried and found very useful in Edinburgh in 1832. Their utility might indeed have been anticipated by a study of the mode of spread of cholera. No point appears more undoubted, than the way in which the disease commences in, and in the first instance often confines itself to, a particular locality of small extent. If a whole town is, as at St. Petersburg, favorable for its reception, it spreads itself rapidly thence, over the entire city; if, as at Moscow, some portions are favorable, while others are not so, its retention in these cholera districts is more or less prolonged, and occasionally there seems to occur a succession of circumscribed epidemic attacks,—to use a phrase not perhaps strictly correct,—in different quarters of the town. There is little doubt that the circumscription of cholera in one locality, and its diffusion over another, can be readily explained by considering the several geographical, meteorological, and sanitary conditions, which constitute or develope the atmospheric peculiarities of a place. The establishment of Houses of Refuge deals especially with this occasional, or indeed frequent circumscription of cholera. Advantage is taken of the fact, that the disease has been known to rage furiously in one spot, while not a single case has occurred in the adjoining neighbourhood. In some singular instances, indeed, a space of a few yards has separated a region in which the mortality has been appalling, from one in which the ordinary health of the inhabitants has been preserved. This fact is so well known in India, that it is customary for a regiment attacked by cholera on the march to shift ground daily; and it is also well known that the natives of countries in which cholera prevails, abandon their houses and villages, when the disease passes through them, and fly from this invisible and intangible foe. Whether in this country, still greater advantage could not be taken of this knowledge, and whether it might not be advisable at once to draw off some of the population from places which suffered severely in 1832, such as Bilston, we are not prepared to say. The General Board of Health have wisely recognised the principle, and have determined to act upon it, as far as they deem practicable. They have therefore ordered throughout England the establishment of Houses of Refuge on the plan pursued in Edinburgh in 1832, and sanctioned by the authority of Dr. Alison.* Whenever cases of cholera occur in a dirty and crowded situation, the Board recommend that the inhabitants of the affected house, or even of the whole court or alley, be removed to appropriate, well-aired, and comfortable houses in a healthier locality; while the affected district is thoroughly cleansed, all dirt and rubbish removed, and the houses ventilated and fumigated.

---

* Official Circular of the General Board of Health. London: Charles Knight.

In Edinburgh this plan is now in use, and, we are informed, with remarkable benefit. Out of 180 persons who have been removed, a single person only has been attacked in the houses of refuge; and, taking into account the numerous instances in which second and third attacks occur in a house in which a first case of cholera has happened, this proportion is, no doubt, very much less than might be expected. Considering the importance of the preventive treatment of cholera, and how completely these houses of refuge effect two important objects,—viz. remove susceptible persons from an infected district, and improve the district itself by lessening the density of its population,—we do not hesitate to urge strongly this recommendation of the General Board on the notice of the profession.

But the principle thus recognised by the General Board has for us an importance and significance beyond the present moment, and the immediate object it is intended to direct. If to relieve the poorer classes of this country from the infliction of a terrible disease, the General Board, or, in other words, the Government, is authorized to employ such measures as we have now discussed, if it is justifiable to remove families, perhaps even against their will, to cleanse their dwellings, to substitute the air of heaven for the impure atmosphere which in our great cities envelopes poverty and misery, and in this way to destroy some of the accessory causes of cholera, it follows as a logical consequence, that the duties thus assumed do not expire, when the occasion which at the moment prompted them has passed away. Why should we refuse to do that for typhus or scarlet fever, which we are about to do for cholera? Cholera is, as it were, concentrated to a point, evanescent, and deadly. Our endemical fevers are diffused, persistent, but not less deadly than cholera. On what grounds can we compel our poor to remain in houses of refuge, that they may be secured from the transitory epidemic of cholera, and refuse to oblige them to live always in such a manner as science indicates will most effectually control the ravages of typhus? If this assumption of authority is justified momentarily, by the presence of an immediate danger, it is justified also permanently by the existence of dangers not less real, though less conspicuous, and equally fatal, though not in so terrible a form. This is only one of the questions which is now opening up before the General Board of Health, and the solution of which will profoundly affect the mode of life and social condition of an immense body of the people of these kingdoms.

We pass on, however, to the 4th recommendation of the Board, the establishment of cholera hospitals. We are happy to find that the General Board of Health has authorized the erection of cholera hospitals, and that the cholera committee of the College of Physicians has given them the sanction of its approval. The question is, how are they to be raised, and where are they to be placed? On a former occasion we attributed a portion of the great mortality of the cholera hospitals, in 1832, to their bad position, their wretched ventilation, their overcrowding, and the way in which they were allowed to be filled with patients who were treated at their own houses until they were on the point of death, and then were hurried off to the cholera hospital.

The only way, it appears to us, in which, with the short time allowed us, we can prepare well-ventilated hospitals, fitted up with all necessary appliances, and not too remote from the infected districts (for cholera patients do not bear transport), yet not completely immersed amidst the miserable dwellings where cholera chiefly ravages, is by using the public hospitals, and by preparing dwellings in which the ordinary patients of these institutions may be for a time accommodated. Or if this is impossible, a certain portion of each hospital might be assigned for cholera.

In order to relieve the public hospitals of a portion of the patients who now occupy them, Government should hire houses in the immediate vicinity. There are always a certain number of patients in our large hospitals, who would be but slightly injured by a little overcrowding, insufficient ventilation, &c.,—circumstances which are fatal to cholera patients.

Into the hospitals we would have every unequivocal cholera patient at once sent. He should not be treated in the vitiated air of his own residence; and perhaps allowed to die in the room where a whole family are compelled to sleep with the dead body unremoved from among them. We do not deny that there are disadvantages attending the removal of patients to hospitals; but in the case of London, not so great in our opinion as would accompany the plan of attending them at their own houses.

Instead of calling on the over-worked union surgeon to treat cholera cases, we would have his duties limited to a simple supervision of his district. He should be empowered to treat premonitory symptoms only; and when the disease has unequivocally set in, he should be ordered by the Poor Law Commission, instantly to have the patient conveyed to the hospital. In densely-populated districts, one or more surgeons might also be appointed by the Board of Guardians, to assist the union surgeon in this inspection of his district. There is no danger in removing cholera patients at an early period, provided the distance be not too great; if a patient has unfortunately passed into the last stages, then the union surgeon should not cause him to be removed, but should report the case to the nearest hospital, from which medical aid should be detached.

The minor details, such as the mode of conveying the patients to the hospital, the supply of nursing and necessaries to the bad cases seen where removal is impossible, &c., need not now be entered upon.

The staff of the several hospitals should be augmented, and it should be arranged that an extra number of skilled attendants should be procurable at a moment's notice. The medical staff should be appointed by a special board, under whose orders they should consider themselves as acting. The board would be naturally formed by a union of the General Board of Health, or of its able medical commissioner, and the Cholera Committee of the College of Physicians. This board should exert an active superintendence over the hospital accommodation. The General Board of Health and the Government should confer on them at once an absolute power to make all necessary arrangements: such as preparing the hospitals to a certain extent; taking care that more room should be available if required; entering into communication with every union surgeon in London, so as at once to receive intelligence of all suspicious cases, &c. &c.

The members of this board, all of them physicians of the highest eminence, should also have the entire control of the medical staff. While it would be unnecessary or impossible for them to lay down any formal modes of treatment, they might yet suggest particular plans likely to be followed by benefit, or might determine what plans having been tried and found quite useless, should not be tried again. They might cause experiments to be made with new remedies, and might check the conclusions of one hospital by the experience of another.

It would also be possible, with such a complete organization as we contemplate, to obtain a minute record of every case, which should be public property, and at the close of the epidemic should be made use of by the General Board of Health in preparing a full history of the attack in London. The board should have authority to appoint experienced chemists to undertake the necessary examinations into the blood and excretions, which the attending physicians would not have time to do.*

The expenses should be defrayed by Government; they would not be considerable, but it is only fair when a national calamity assails us, that the nation should make provision for its poor, with as much liberality as private charity does, in ordinary years, for our current amount of sickness. All the medical men, from the Cholera Committee downwards, should be paid well; we are of opinion that the work which is done gratuitously, is often done ineffectually. We need not enter into further details; we are desirous merely of indicating the path we think it desirable to follow, not of exploring the country to which it leads.

The General Board of Health have already made admirable provisions for the cleansing and purifying of houses, the removal of nuisances, &c.; on these points, therefore, we need not enlarge.

Dr. Müller's Fourth Report contains a few remarks on the differences between the phenomena of the disease in Stettin, Berlin, and St. Petersburg.

In his last Report Dr. Müller seems to have finally arrived at definite conclusions as to the light in which the symptoms should be regarded, as indicative of varieties of the disease.

> "They generally adopt here" (Berlin), he says, "Romberg's acutely-drawn distinction between the enteric and asphyxial form, which nevertheless, in my opinion, can be only distinguished, because in the last case the common collapse succeeds more quickly than in the first form, whilst the fundamental lesion is for both the same. It appears to me that the so-called asphyxial form is only the distinctly-marked type of the rapidly-developed stadium algidum of the disease, when vomiting and purging have almost ceased, and the slow, deep-drawn, sighing respiration appears, denoting the commencing injury of the vagus." (p. 45.)

We have purposely delayed all reference to Dr. Müller's opinions on the nature of the active cause of cholera, in order that we may be able to discuss the opinions, on this point, of some of the other writers whose works are now before us. To this subject we shall therefore return.

The volume of Reports on Asiatic Cholera, published by Mr. Rogers, is a most valuable contribution to the literature of this subject. Most of our readers are probably aware that, under the judicious management of Sir James M'Grigor, an Annual Report is sent in by the surgeon of every regiment in Her Majesty's service (no matter in what quarter of the world the regiment may be serving), in which all particulars relating to the health of the corps are carefully detailed. If any unusual sickness or violent epidemic has attacked the regiment, a Special Report is devoted to this subject; and we can ourselves testify, from personal examination of some of these documents, that these special reports are often drawn up with remarkable ability.* This system has of late years been adopted also in the Company's service.

---

* In connexion with this subject, we would call attention to the excellent plan for recording cases, proposed by the Western Medical Society of London. If a thousand, or two thousand cases could be taken according to this form, the record would give most valuable information.

Mr. Rogers, while serving in the Madras army, established and edited with great talent a quarterly journal, called the Madras Medical Journal; and perceiving the valuable nature of the reports on cholera, as well as of those on other diseases, sent in by the medical officers of the Queen's and Company's service to their respective authorities, he procured these documents, and published a selection of them in his journal. As the journal itself is, however, very little known in England, and is rapidly becoming scarce, Mr. Rogers has republished thse reports in the present form.

We had occasion, in a former Number, to refer to two or three of these reports; and we are now happy to express our gratitude to Mr. Rogers for presenting them to us in so compact and accessible a form. We can recommend them to our readers as giving most valuable and indeed indispensable information to all who are desirous of studying this important subject. It appears almost invidious to select any report for especial commendation; but we may particularly mention those of Dr. Lorimer, Mr. Macgregor, Drs. Mouat, Kellie, and Parry, as characterised by a consummate knowledge of the disease. There is in this volume a report, and a clever one too, by Dr. Dickson, the author of the 'Fallacies of the Faculty,' who served for some time in India, in H. M. 30th regiment.

In addition to these reports, there are two original chapters in the volume before us by Mr. Rogers himself: one on the exciting causes and modes of diffusion, and the other on the treatment of the disease, as practised in the Madras Presidency.

To the first of these chapters, as being new to us, and as containing the résumé of the opinions of an able and acute observer, after many years' investigation of cholera, we shall more particularly allude at the present time; being prevented by want of space from entering more minutely into the opinions held by the several authors of the Reports.

Mr. Rogers sums up his conclusions on the mode of diffusion of cholera, in the two following propositions:

> "1. Cholera has its origin in other than human sources, and the communication of the sick with the healthy is not necessary for its diffusion.
>
> "2. Cholera may be in many cases averted, and its mortality much lessened, by proper precautionary measures being adopted." (Op. cit. p. 3.)

That we most cordially agree in these statements, need scarcely be said, since we formerly did our best to expound and enforce similar views. We regard their recognition and application as the most important step which has yet been made in the preventive treatment of cholera. If we cannot discover any antidote to the poison, as quinine is to marsh miasma, or any method of destroying the susceptibility of the receiving organism for the disease, as vaccination does for smallpox, we have at least a method almost as sure, if we choose properly to employ it, of limiting the spread of the disease by setting bounds to the activity of its active cause.*

---

* Mr. Thom's work on Cholera at Kurrachee, which we formerly reviewed, is one of these Special Reports.

Mr. Rogers's two propositions may form a convenient arrangement for our discussion.

It will be observed that the first proposition is very cautiously worded. Mr. Rogers asserts that the disease does not spread, that is to say, does not ordinarily extend, by the communication of the diseased with the healthy. He does not in this place call cholera "non-contagious." He does not do more than inferentially negative, in the majority of cases, the influence of contagion. Here again we fully agree with him. We have already endeavoured, on several occasions, to point out that the possession of a certain property does not necessarily imply the exclusion of other properties; that the disease which multiplies itself in the bodies of men or of animals, is not *therefore* incapable of increase apart from these organisms; that, on the other hand, the virus which spreads usually without reference to animal systems, is not *on that account* incapable of being, under certain circumstances, influenced and augmented by the living chemistry of the animal frame. We have not hesitated to dissent from those physicians who would bind the phenomena of epidemic diseases within the limits of a rigid definition, and who would ignore the most obvious phenomenon if it agreed not with their *a priori* conclusion. We have insisted also on the necessity of judging each epidemic disease by itself, and on the impossibility of finding any single malady which can be an accurate type and standard for all others. It is, we believe, proved beyond a doubt, that cholera spreads irrespective of the action exerted upon its virus by passage through living bodies; that its increase in this latter way is unusual and exceptional; and that if such an increase be assumed in order to explain the phenomena of its attacks, the assumption will be found utterly incapable of accounting for the peculiarities of its course. But we do not doubt that cholera, like yellow fever, which in this respect it closely resembles, may occasionally, under the influence of circumstances not yet thoroughly investigated, become endowed with, or may manifest, the property which it is customary to call contagious.† But from this occasional and infrequent manifestation of contagion, we are not surely entitled to conclude that in all cases the disease is possessed of this property, and that it spreads in no other way. One of the most eminent physicians of the day, from whose opinion we differ with great reluctance, has told us to apply the same reasoning we used in the case of the Eclair steamer, to the case of the brig Amelia, which seemed to carry cholera from New York to Folly Island, near Charlestown. We have, on a former occasion, fully admitted the importance of this case, and then called it "the best authenticated instance of importation in the whole history of cholera;"‡ we admit it now. But this case merely proves, or rather in part proves, for cholera, what the Eclair did conclusively, and altogether for yellow fever, viz., that this disease *might be* propagated by contagion.* But when on a former occasion we came to this conclusion, our next question was, is yellow fever, or a distinctive form of yellow fever, always and solely so propagated? And for various reasons, we decided that it was highly probable that it was not. Did our space permit, we might go over the same argument with cholera.

---

* These principles, so ably enunciated by the Sanitary Commissioners, have been fully recognised in the manifesto of the College of Physicians.

† The College of Physicians has given the weight of its authority to an almost similar view. Dr. Reid, also, in his able Letter, which we much regret that we cannot discuss at length, arrives in reality at the same conclusion.

‡ British and Foreign Medical Review, April, 1847, p. 329.

Mr. Rogers, however, like almost all Indian writers, advocates in the subsequent pages a view exclusively non-contagious. All the phenomena which appear to countenance contagion may be accounted for, he thinks, by the fact that the poison may be transported by bodies of men, and then that such persons suffer as come within the range of this infected atmosphere. This doctrine of portability, a novelty in the history of contagion, was first propounded by Jameson, and every year has added additional weight to the evidence in support of it.

Mr. Rogers believes the "first material cause of cholera to be a specific poison," which may be present in all situations in India in a dormant state, but which requires for its development certain accessory causes, such as sudden meteorological changes; animal and vegetable effluvia; emanations from large bodies of men; the effluvia from persons crowded into insufficiently ventilated buildings. Of all these conditions he relates characteristic examples.

The Russian physicians have adopted views almost identical. In his Third Report, Dr. Müller writes thus: "Cholera is caused by a specific hitherto unascertained, poison, mixed with the atmosphere, the ordinary constitution of which is unchanged. This poison penetrates also the uppermost stratum of water, and is therefore especially transmitted along water courses." (p. 36) In the Second Report he writes thus: "The contagiousness of the disease is equally unascertained. In Russia, almost all physicians doubt of a true contagion; they believe only in an epidemic—telluric—atmospheric cause. By far the smallest section of physicians, but certainly those who have accurately followed the course of the disease, maintain its contagiousness with the greatest resolution." (p. 23.) Dr. Müller goes on to observe, that it is certainly very remarkable that the first sufferers in any place should frequently be those who had just arrived from an infected place, and he proceeds to cite six instances of this kind. This is not a great number for all Russia, and out of the six there is only one decided example in which those nearest to the patient were the next sufferers. Dr. Müller then continues:

> "Although so many examples may speak for the contagiousness of cholera, yet it is remarkable that persons who have had daily and hourly intercourse with the sick, have seldom been attacked. Here, as in Moscow, the most hardy experiments have been made: some have put on the clothes, others have slept on the beds, of cholera patients, yet none have been attacked. Very few attendants, nurses, or physicians have sickened; out of 500 physicians, only 11 died in St. Petersburg. It is an important observation that journeys from an infected place, and atmospheric variations, appeared to increase the disposition to the disease." (pp. 24-5.)

Dr. Müller's remark that the poison of cholera penetrates the superficial layer of the water of rivers, is new to us,—we are inclined to doubt its correctness. Rivers are the sites of the poison, because they so often have muddy and dirty banks, and because there is considerable moisture in the atmosphere hanging over them.

---

* We believe we may state, that a link in the chain of our argument, in the case of the Eclair, has been strengthened by evidence lately received from Sierra Leone, which proves that there was no epidemic yellow fever there at the time the Eclair left. This increases the probability of the truth of Dr. M'William's proposition, which we adopted; viz., that the contagious "Eclair" fever was derived from the common, so-called non-contagious, marsh remittent fever.

Mr. Rogers's second proposition is one which we have formerly discussed at such length, that we must beg our readers to refer for themselves to the Reports, for the examples and additional illustrations therein contained.

To one case, however, we must briefly allude. No less than four Reports refer to attacks of cholera at Bellary, in the Ceded Districts. Now, while in almost all cases we can predict the kind of country which cholera will chiefly infest, and can often prophecy at what point a regiment on the march will be most likely to suffer, Bellary has frequently been supposed to offer, not only an exception to the rule, but an exception which bids defiance to inquiry and explanation. That this apparent exception is not a real one, has, however, long been known to those who have looked closely into the point; and the accessory causes of the disease have never been more clearly made out, than in the Reports before us. Let us endeavour, however, to put the case before our readers.

About 250 miles north-west of Madras, a range of lofty hills, called the Copper Mountains, forms the western boundary of an immense plain. A spectator standing at the base of the hills, and looking to the North and East, gazes over a vast expanse of country, which, nearly as far as the eye can reach, forms an almost uninterrupted and level surface. About six or eight miles away from him, however, a rounded mass of rock towers up abruptly from the plain, and breaks at this point the uniformity of the view. This is not one of those limestone rocks common in some parts of India, which are formed by the evaporation of water deeply charged with calcareous salts. This rock is granite; about half a mile in diameter, and some 500 feet high. On its bare surface only a scanty vegetation grows, and the soil at its base is equally sterile and dried up beneath the fiery rays of the Indian sun. Such is the rock of Bellary; and till within the last three or four years, the European barracks were situated in the Lower Fort, built on one of the inferior portions of the rock. There are no marshes, no rivers, no dense and exuberant vegetation which may afford to cholera a congenial soil. And yet since 1818 cholera has never for a single year been absent from Bellary, and the place of burial of each successive regiment bears sad testimony to its permanent and unrelaxed activity. And it is evident that it is something connected with the rock itself, which has to do with this development of cholera. The disease hangs about the Lower Fort, and rarely travels any distance from it; it prevails severely in the barracks on the rock, in the native town and bazaars immediately adjoining, but it has almost uniformly spared the native lines and the adjacent buildings which are situated some three miles away. It is one of those striking examples of localization to which we formerly referred. The causes of this prevalence are to be found in the fact, that never were men placed under worse hygienic conditions than those living or quartered in the Fort. For military reasons, the space given to the buildings of both Europeans and natives is exceedingly limited; the houses are densely crowded, and a numerous population is, or used to be, compressed into a space inconceivably small. Close to the European barracks are two dirty bazaars, which have long been considered as public nuisances. A large tank, which in the hot season becomes nearly dried up, furnishes its supply of effluvia; and a very inefficient supervision of the barracks themselves till lately allowed the emanations of a large body of men to furnish assisting conditions in aid of the poison. As an illustration of our case, we will take the most severe attacks of cholera which have occurred of late years at Bellary, viz. that among H. M.'s 39th regiment and the 13th Light Dragoons in 1839, and which are recorded by Mr. M'Gregor (p. 59) and Mr. Ferriar (p. 118).

The 39th Regiment arrived at Bellary in February 1839; the strength of the corps was nominally 730 men; in five months it was reduced to 645. Mr. M'Gregor shall describe the way in which the regiment was accommodated.

> "The regiment was quartered in the two barrack-squares in the Lower Fort, each of which, by the Government regulations, is estimated to contain 500 men. The space by regulation for each soldier, including doors and windows, is only 4 feet laterally, and subtracting the space for 60 doors and 146 windows, which are respectively 5 and 4 feet wide, the space allowed for the rest of the men must be proportionably curtailed, if 220 men do not sleep opposite to these. The height of the wall is 12 feet in one barrack, and 10½ in the other; the height of the roof above is about 6 feet more. The breadth of the rooms is 20 feet. Hence the area cubically for each man is, on an average, as follows:
>
> "Lateral space, 4 feet, height of wall, 11 do.; half the breadth, 10 do.; height of the roof, 6 do.; half of the roof, 6 do., by 10 and by 4, divided by 2, giving for each man an area of 560 cubic feet over the whole barrack. If we place the men's cots so as to prevent their being opposite to doors and windows, there will not be any interval between the cots, which are 2 feet 7 inches in breadth." (p. 67.)

This is the Government allowance; but as a certain number of men are always in hospital, and a certain number on guard, and as the married men at Bellary, as in most other stations, live chiefly out of barracks, Mr. M'Gregor calculates that, if 280 were deducted from the whole strength, each "man would have had nine feet of lateral space. But, unluckily, the canteen, the spaces for pay-sergeants, the band-room, the serjeant's mess-room, &c., lessened the accommodation so much, that the men never had six feet of absolute lateral space."

Mr. M'Gregor then continues:

> "I need not enlarge on the bad ventilation of the barracks, nor on the want of space in the verandahs, in which the men ought to have tables and forms for taking their meals respectably at, nor the want of convenience for salutary ablution, and of wells of water within the square; neither need I mention the overwhelming nuisance of bugs, which swarm on the cots and the patchwork roofs in such numbers, that the peace and rest of the men are absolutely *destroyed*, and lead to disease indirectly in various ways, especially from the men being obliged to resort to the open verandahs to escape their everlasting torment." (p. 68.)

In spite of his disclaimer of enlarging on the above topics, Mr. M'Gregor has stamped them pretty strongly.

To appreciate the misery of men exposed to these hardships, our readers must call to mind the climate of Bellary. During the hot months in March, April, May, and part of June, the heat is insufferable. The unclouded sun glares from a sky of brass, upon a parched and desiccated soil; its fierce beams strike on, and are reflected from, the granite rock; the thermometer rises in the shade to 90°, and even 98° at mid-day and to 130° in the sun; the intense heat is rendered more oppressive by the sultry stillness of the atmosphere, which is broken only by occasional violent whirlwinds of a dry hot wind gushing from the south and west. This wind, burning as if it had passed over a furnace, is more intolerable than the still atmosphere; it produces great evaporation, and, in spite of its warmth, chills the frame. Very little rain falls; occasionally a thunderstorm occurs; thunder is heard almost daily, and in the evening vivid lightning is often seen flashing from the earth.

In such a climate, one would think the authorities should give the men sufficient breathing-room; and we are happy to state that repeated remonstrances have at last had their effect, and that larger barracks are now built for the Europeans on the plain below; but for more than twenty years, the men born in 50° north latitude were subjected to this "ordeal by fire."

Let us, however, pursue our brief account of the attack of cholera in 1839.

"The subject of dress," continues Mr. M'Gregor, "attracted our attention, and as the febrile attacks were increasing among the men on the various guards, their state from *being obliged to wear their full dress* was inquired into, and the *distress from it found to be so great,*[*] that an official letter was addressed to head quarters, in consequence of which the European troops were allowed to wear white jackets on all garrison duties." (p. 68.)

Certainly the authorities must have wished to try if the human body were not fusible, where they insisted on all the paraphernalia of coatees and pipeclay. Even in this country we have seen the household troops appear, in July, uncomfortably warm when they have been on sentry. We should like to know how our Guardsmen would look if they were *incinerated* like the 39th. We suspect they would not *wait* to have their "*distress*" found out.

Let Mr. M'Gregor continue:

"On the 21st and 22d of March there were a few showers and much lightning, which, as it was without thunder, was passing from the earth to the clouds. With the exception of these partial showers, there was a great scarcity of water, and the large tank on the south-west of the fort was nearly dried up. How far the exhalations from this source may have conjoined with the winds, the heat, the economy of the dress, the bad guard-rooms, and improperly ventilated barracks, swarming as they are with bugs, is an important question, and one which must be taken into account in the estimate of the salubrity of the fort generally, and in hot dry seasons especially. Under the combination of heat, draught, dress, unfitting accommodation, want of protection against prevailing winds, bugs, miasms, febrile and choleraic, certain it is that cholera appeared on the evening of the 21st of March. (p. 69.)

Just at this time (on the 27th March) a wing of H. M.'s 13th Light Dragoons arrived at Bellary. It became a question how to accommodate them. Our readers will conclude that as cholera was now ravaging the 39th regiment, the last place in the world to which the dragoons would have been sent would be the Lower Fort; they will imagine that, at any rate, the 39th regiment and the locality they inhabited were avoided.

On the contrary, the authorities, deliberately, and in the face of remonstrances, took away one barrack-room from the 39th regiment, ordered the dragoons into it, and *confined the 39th, now suffering from cholera, into a space half the size of that which, incontestably, had been much too small.*

[*] The italics are ours.

Nor was this all. The dragoons' horses were, at first, picketted outside the fort; but some difficulties occurring, we believe in watering the horses, *they were ordered also within the fort.* The barracks and the barrack-square were crammed to suffocation with men, horses, tents, &c. &c.

The authorities had now done their best to aid the cholera. The disease soon increased among the 39th, and invaded the dragoons, and became so severe that the 39th were at last moved out of the fort, and placed under canvas. But the disease once located in the regiment could not be so easily shaken off; it travelled with the corps, and continued its ravages for nine days longer. The native regiments were perfectly healthy.

Remarking at the end of his Report on the exertions he had made to remedy these evils, Mr. M'Gregor continues:

> "We reported officially, at different times, many circumstances which appeared to us *remediable* causes of disease among the men, for which, however, it appeared *we got very little credit with the local authorities;* but this we cared little about, as we found many (*most*) of our suggestions ordered to be adopted from head quarters." (p. 95.)

With all its heat, Bellary is not an unhealthy station; and Mr. M'Gregor concludes his Report with the observation, that if these "remediable causes" were removed, "Bellary might become one of the favorable stations of India for the health of H. M.'s troops."

We cannot afford space to continue the analysis of this attack. We have alluded to it for two reasons: first, it illustrates completely the great influence which external circumstances have upon cholera; secondly, it affords a striking instance of the extraordinary way in which men's lives have been thrown away by the ignorance of those in authority. We write that men's lives *have been* sacrificed, we wish we could say also that they are so sacrificed no longer. But this we cannot do. The monster evils at Bellary have been, it is true, redressed; but through the length and breadth of India, the English soldier is still exposed to inconveniences which destroy his comfort, grievances which paralyse his zeal, and unhealthy influences which contaminate the very source and spring of life. In cantonments and in marches, the advice and cautions of the medical advisers are still neglected or ignored; and if a medical officer fearlessly performs his duty in pointing out "*remediable causes*" of disease, there are other places besides Bellary in which he "gets very little credit with the local authorities." It is true that at head quarters the hygienic condition of the soldier has, of late years, received its due consideration, and good results have already been attained; but the battle is not yet won, and the ignorance of subordinates has often thwarted the wise orders which have issued from superior authorities.

In connexion with the question of the development of cholera, Mr. Rogers moots one question of great interest. He believes in the local origin of the cholera; that is to say, that certain localities do not merely give a proper habitat to a poison which has reached them, but actually, at times, generate this poison itself. As an illustration, he gives the following case:

> "The Coom river winds very circuitously through Madras, and in its meanderings it nearly encircles the village of Chintandrepett. This river was made a necessary of by hundreds of natives daily throughout the year, and when the monsoon was heavy, and the bottom of this *Augean* stable was thoroughly cleansed,

no ill resulted from it; but if the monsoon failed, and the river remained uncleansed, when the hot weather returned, the water became low, and the filth at the bottom was exposed to the heat of the sun, the smell was most offensive, and an attack of cholera was the certain result, its only victims being the inhabitants residing within a short distance of its banks." (Op. cit., p. 5.)

We wish that Mr. Rogers had pursued the same train of thought a step beyond, and had inquired what it is that has caused cholera to acquire so extraordinary a development in our time, and why the local circumstances which he supposes can generate it, and which have been for centuries existent in India, should have been inoperative to produce the disease until within these last few years. For although there is every probability that cholera, or a disease like it, did exist prior to 1817, there is no doubt that its prevalence was partial and limited, and that in India especially, it was so little known at the end of the last and at the beginning of the present century, that the outbreak in 1817 not only astonished the English surgeons who had been acquainted with the diseases of Bengal for nearly sixty years, but equally surprised the Hindoos, who throughout India regarded cholera as a new and unheard-of pestilence, and who knew not to what Deity of their infinite mythology they could appeal for aid against so terrible a malady.

When Mr. Scot ransacked the writings of the older English and Dutch surgeons, and the records of Hindoo medicine, to discover evidence of cholera being known to his predecessors at Madras, could he have overlooked the fact that at Chintandrepett, at his own door, cases of the disease he was investigating occurred year after year?

We think Mr. Rogers has to prove his position, that these local circumstances are anything more than important conditions of development of the specific cause, essential no doubt to the vigorous action of the poison, but not necessarily proper to its existence. If these conditions are to be viewed as absolute generative powers of the poison, it is incumbent on the supporters of such an opinion to demonstrate that the common belief in the origin of cholera in 1817, or (admitting its pre-existence) in its immense propagation at that time, is erroneous.

Our space, however, forbids us to enlarge on these topics, however interesting. We were anxious to discuss them in connexion with the question of the nature of the peculiar force which impels cholera at intervals over the world in determinate directions. We were desirous also to refer to some speculations which attribute cholera to electrical conditions. These speculations appear to us to admit of easy and complete refutation; they may be shown to be destitute of plausibility, and, as they are now put forth, to be even impossible. But we must defer these inquiries to another opportunity, when we shall return to these Reports, as well as to some of the other works cited at the commencement of our article. We could not, in the space that remains to us, do any justice to Dr. Buchanan's, or Dr. Kier's excellent treatises. We therefore entreat the indulgence of these and of other writers for our delay.

One word more in reference to Mr. Rogers's Reports. The reader will find in them almost all kinds of treatment alluded to and discussed. This is not the least valuable, though, as far as success goes, a negative section of the work. We beg to refer to these Reports all those gentlemen who have found in their own practice one mode of treatment uniformly successful.

ART. II.

1. *Reports from the Select Committee of the House of Commons, on Medical Education; with the Minutes of Evidence and Appendix.*—1834.

2. *Reports from the Select Committee of the House of Commons, on the Medical Registration Bill; with the Minutes of Evidence, &c.*—July, 1847; March, 1848.

IN two articles published in our last Volume, we took occasion to point out the relations of the medical profession to religion and morals, and its position and prospects in society. We proposed certain principles as the foundation of that organization of the profession which would best serve its interests, as well as the interests of society. We have now to consider the details of a suitable organization.

And, in the first place, let us consider what the phrase *organization of the medical profession* implies. We need scarcely say that it does not imply an organization in corporations, as now exists. That is the organization of a very small minority of the profession even in the metropolis, and the objects of which are rather selfish than professional—rather personal than public. Nor does it imply a mere numeration of medical practitioners, as a college list, for example; nor a nominal combination, as is the case with the licentiates of the College of Physicians, and of Apothecaries' Hall; with the graduates of the Universities of London and Edinburgh, and with the members of the College of Surgeons. With regard to all these, we may say that they can exercise no direct influence on, or in, their respective corporations whatever; they have no share in the government, no voice in the council; they have less of either, in short, than the forty-shilling freeholder possesses in the government and councils of his country. By an organization of the medical profession, we mean the reverse of all this. It should place the practitioner in such a position that he has the opportunity of sharing in the government of the profession, so that, while it subjects him to discipline and rule, it permits him to have a voice in the legislation that binds and controls his action. It should enable the profession to express its sentiments as a unity, whether as to professional ethics and moral relations, or mere worldly interests. In short, the organization should be that of a republic of men of letters and men of business. At present we have disorganization and disunion, and to a privileged class alone is accorded the privileges we have mentioned. This incontrovertible defect in the organization of the profession is at the root of all the agitation, disturbance, and recrimination which have annoyed, vexed, and unsettled it for at least the last half century. During the whole of that period there has never been an entire cessation in the clamour; and at intervals the whole profession has been profoundly agitated. What is more astonishing is, that scarcely any progress has been made towards a state of repose and order. The Medical Registration Committee of 1848 is hardly in advance of the Medical Education Committee of 1834. We have similar facts, similar narrow notions, similar prejudices expressed, and even in several instances by the same men. Nay, we have, in 1848, hardly so perfect a reiteration of plans of education and organization as in 1834. We repeat, that the want of a means whereby the *profession at large* can *express and enforce* its opinions as to medical politics *is solely at the root of all these evils*, and until that defect be removed, there never can be peaceful progress in the medical republic.

It were a weary task to our readers, not less than to ourselves, to trace the history of the medical-reform question during the last fifty years. We will touch, however, on some of the more salient points. In the years 1797, 1798, the Electoral Academy of Useful Sciences at Erfurt published the following prize questions:

1. "Is it necessary, and is it possible, again to unite both branches of the healing art, medicine and surgery, in learning, and in practising them?

2. "What were the causes which separated them, and by what means are they to be reunited?"

Fourteen answers were sent in, of which thirteen were in favour of the union of the two departments. Such, half a century ago, was a question of importance; such its solution. Need we say that it is of as great importance, and would have a similar solution now?

In the year 1806, a numerous meeting of the medical profession was held at the house of Sir Joseph Banks, President of the Royal Society, at which Dr. Harrison presented the answers he had received to circular letters ordered to be issued at a previous meeting, together with a plan of medical reform. It was resolved that Sir J. Banks and Dr. Harrison be requested to wait upon the Right Hon. Henry Petty. The plan comprised regulations as to a professional education very similar to those in force at the present day, including a public register of all medical practitioners in the United Kingdom. The manner in which these efforts to improve the position and prospects of the profession were met by the organized minority of the corporations, was highly characteristic. The London College of Physicians repudiated it altogether: the King's and Queen's College of Physicians in Dublin, complained that surgeons and apothecaries engaged to treat *medical* cases; that degrees were given in Scotland with too great facility; that quacks and pretenders abounded; and that the cause of those irregularities was the want of competent restrictive powers in the College. In Scotland they wanted no reform; abuses might, and doubtless did, exist in England and Ireland, but Scotland was exempt. Dr. Thomas Beddoes published, in 1808, a letter to Sir Joseph Banks, "on the causes of the prevailing discontent, imperfections, and abuses in medicine," which contained, amongst other proposals, a plan to collect the whole medical experience of the country, by enforced reports to a central board, at fixed periods, from all the hospitals and medical charities in the kingdom. In his curriculum of study, he devotes the last year or two to hospital practice at Paris or Vienna, if accessible, which at that time they were not.

In 1812, a society of general practitioners was formed under the designation of the "Community of Associated Apothecaries and Surgeon-Apothecaries." Deputies from various districts of the provinces were in communication with the metropolitan body: a step opposed at first, but afterwards found to be as politic as it was just; and the heads of a bill were prepared. The objects they sought were an improved education and an examination as to the qualifications of those proposing to practise as general practitioners, a system of apprenticeships, an annual registration and license, *the division of the kingdom into districts for the better government of the profession*, and a superintending body in London; an improved system of medical relief of the poor, and a legal claim to moderate remuneration for professional services. The agitation for this bill drew forth numerous communications to the journals, one of the best of these being a 'Memoir on Medical Reform, by a disinterested Physician.' In this document the unity of the profession is insisted on, and the propriety of constituting the important class of general practitioners, licentiates of the two London Colleges, is demonstrated. Unfortunately for the profession, these colleges opposed the reforming association and neglected all "the repeated and respectful overtures" made to them by it, and a simpler and more harmless measure was sought for, from Parliament. How zealous the profession at large was in the attempt thus made, is sufficiently shown by the fact, that the sum of £2000 was subscribed on this occasion. We have lately had a similar effort, beginning with equally liberal views, and about to end apparently in the same way.

A few years having ended this, the first great agitation of the century, by the passing of the Apothecaries' Act of 1815, there was a lull in the discussion of medical reform; but as that act had only added another to the already too numerous examining boards, it speedily recommenced, and in 1818 we find (amongst others) the late Dr. Barlow, of Bath, entering the field. We cannot withhold our humble meed of praise from the views and principles laid down in the papers published by Dr. Barlow in the 'Edinburgh Medical and Surgical Journal.' In the first article he recommends that a committee of the House of Commons inquire into the state of the medical profession, and gives an outline of the heads of inquiry. He proposes that "the profession of physic be organized by the legislature conformably with its natural tendencies." That "the corporations, at present so numerous, inconsistent and unprofitable be consolidated, and one general medical institution be established for each division of the United Kingdom, to include all members of the profession resident therein, whether physicians, surgeons, or general practitioners; each institution to be governed by a council, to be annually elected in certain proportions from the practitioners of each class resident in the metropolis." These institutions were through the council to regulate everything relating to the profession; to direct the education and admission of students, and the establishment of professorships, libraries, and museums; to publish a national Pharmacopœia, and also a popular Pharmacopœia, and thereby abolish the sale of quack medicines; and to superintend the whole profession generally. In the sixteenth volume of the same Journal Dr. Barlow published a second, and in 1827 a supplementary essay on the 'Natural Unity of the Medical Profession" in the twenty-eighth volume. In these papers Dr. Barlow shows that the class of general practitioners is, in fact, *the* profession, and this, without derogating in the least degree from the higher rank and attainments of the physician and consulting surgeon. But the corporate struggles in behalf of the latter, and their separation from the profession, have led to great evils; and it is demonstrable, he maintains, that it is only owing to the public will being exercised in defiance of corporate rights and legislative enactments, that greater injury has not resulted.

Amongst many other writers of this time may be mentioned Dr. John Thomson, of Edinburgh, and Sir James (then Dr.) Clark. These discussed the subject of education, however, rather than of organization, and few treated the latter question in the comprehensive, unimpassioned, and lucid manner of Dr. Barlow. Time, so far from calming the agitation of the question, only increased it, and projects of every kind were brought forward. At last a select committee of the House of Commons was appointed to inquire into medical education, and their report was ordered to be printed in August 1834. Mr. Warburton was the chairman, and the principal, if not sole, medical legislator in the House of Commons. Nothing, however, resulted from this, unless the establishment of the University of London by royal charter may be considered as flowing indirectly from it. The negligence and narrow-mindedness of the powers of the day were manifested on this occasion; for the interests of a large number of students who had matriculated at the then so-called University of London, now University College, and had diligently contended for honours in the class examinations on the faith of promises then made, were altogether neglected in the arrangements for the charter; and thus a number of gentlemen who had intended to graduate in the metropolis had their arrangements entirely set aside, and had only the choice between a recommencement of their scholastic studies at a mature age, or the cheap and not very dignified honours of some small northern university.

The Report from the Select Committee above mentioned, seemed only to stimulate still more the desire of the profession for comprehensive changes, and associations sprung up in all parts of the United Kingdom; partly with the view of securing what was and is termed Medical Reform, and partly to carry out those objects which we have stated as being the objects of Medical Organization. To maintain the honour and dignity of the profession, to advance medical science and art, to cultivate friendly and honorable feelings, to protect the poorer members from the rapacity and avarice of boards of guardians, were leading objects of these associations; and the bare mention of the fact is sufficient to show how much the organized minority in the corporations had neglected their duty. The united efforts of these associations again forced the question on the legislature, and the government at last took it up. The bills and charters of Sir James Graham are of too recent date to require any notice from us; we need only say that when the Secretary of State went out of office, another bill was introduced (the Medical Registration Bill), the opposition to which by the medical corporations led to the appointment of another select committee in 1847. This committee, at the instance of Mr. Wakley, was reappointed at the commencement of last session, and has received a vast mass of evidence, part of which only has as yet been made public.

It is our task to lay before our readers, in as concise a form as possible, the various principles, propositions, and modes of action used and recommended by many leading members of the profession; and while thus placing them before our readers, to measure them by the tests of practicability, adaptability, and utility.

One of the fundamental elements in Medical Organization is the designation of the component parts, or an accurate knowledge of the members of the profession as to their academical qualifications and mode of practice. In other words, the registration of every member of the profession. Facile as such a step might appear at the first, it has been involved in a cloud of difficulties, although all witnesses concur in thinking it a good thing. A model alphabetical list was prepared, in which the qualification of each practitioner, and his mode of practice, were placed opposite to his name. This simple plan met with an almost unanimous rejection by the corporate authorities. The great objection to it was, that it tended to destroy the distinctions in the profession, and to establish "one faculty." Dr. Hawkins put in an amended plan. By this the two Colleges in London, the Apothecaries' Hall, and the two Universities, were to issue annual lists of their respective graduates, licentiates, fellows, and members; classing them under the three heads of persons legally qualified as physicians occupying one list, as surgeons in another, and as apothecaries in a third. There was also to be a general alphabetical list, with a letter to each name, signifying in which grade the individual practised. It does not seem to have occurred to Dr. Hawkins, the proposer, or to Sir B. C. Brodie, and the other gentlemen who so promptly gave it their sanction, that such a list would in no way prevent the feared commingling of grades. In the first place, the consulting surgeons in all the provincial towns practise midwifery, at least, and many of them pharmacy; secondly, many provincial physicians are also consulting surgeons—there are scarcely a dozen pure even in Edinburgh; thirdly, physicians are legally qualified to practise both surgery and pharmacy. There is, indeed, an act of parliament, passed at the instance of the London College of Physicians, which expressly declares surgery to be part of medicine, and, therefore, to be practised by the physician. We need scarcely refer to the annals of the College for proof that the notion of anatomy and surgery being the exclusive domain of the pure surgeon, or of these and midwifery being the work-circle of the "general practitioner," is of modern growth, and has originated rather in the effeminacy of modern courtly physicians than in time-honoured custom, or in the wants of society. We may add, too, that there are physicians in the provinces who would resist any attempt to fix the unmasculine conventionalism of a few metropolitan physicians on the country at large: and that such would have their names entered in all the three lists, rather than yield their right to practise in any way they might think the best. But those gentlemen, in objecting to the apprehended commingling and confusion of grades, forget also that those grades have been called into existence, and are maintained in it, by the public at large, and not by the corporations; that these grades indeed created the corporations; and that, in fact, however the mere nomenclature might be altered or abrogated, the substantial subdivisions would themselves remain. There would still be men of more gentlemanly manners, or of better family, or of better education, or of greater natural genius, occupying the position of consulting physicians and surgeons, and of attendants in ordinary to the titled and the wealthy; there would still be the more numerous but less aristocratic and courtly general practitioners for the poorer and commercial classes; and there would still be the cultivators of special departments of medicine and surgery. Again, while this registration would not accurately define membership, it would afford few facilities for the attainment of another object, namely, the information of the public at large as to the qualifications of the registered persons. Sir R. H. Inglis proposed that the Registrar-General should transmit lists, supplied to him by the respective examining boards, to the municipal authorities throughout England and Wales, to be sold by them at a cheap price; but we cannot find that any one of the leading members of the two London Colleges entertained even the notion of giving the information to the public; *they* would issue a bulky list of many thousand names, and leave the public to read it, if they pleased to wade through such a modern book of Numbers. We think it possible to propose something better than that. Further: the numerous examining boards have sent forth a heterogeneous mixture of qualifications, and there are many hundreds of practitioners who are practising in their respective departments of practice illegally. By the proposed enactments, these gentlemen would have been legalized without examination, and without the payment of an additional fee. Such a step can hardly be considered strictly equitable; it seems to us to be somewhat unfair to place the English surgeon-apothecary on the same footing as the mere apothecary; or the latter on the same footing as the licentiate of the Glasgow faculty. The qualifications of each to practise medicine and surgery may be equal; but in the one case the law has been complied with at an outlay of time, capital, and anxiety; in the other, this outlay has been saved, and the law broken. We think, then, that those persons who are practising in each tripartite division of the kingdom, without having the legal qualification demanded by the existing law from those desiring to practise in that division, should pay some sum, by way of fine or acknowledgment, from which payment the legally qualified practitioner should be exempt.

What right and privileges should registration confer? The London College of Physicians thought that the effect of the Medical Registration Bill, if passed, would be the reduction of all practitioners to one grade, whatever might be their respective qualifications, and that they might act as physicians, surgeons, or apothecaries, interchangeably. This opinion was adopted extensively, and apparently without any reference to what actually takes place. Now, in the first place, every apothecary is certified to be fit to practise medicine; and as by an act, already referred to, passed in the reign of Mary, medicine includes surgery as one of its parts, they can also claim the right to practise surgery without further examination. That many apothecaries do so practise, is amply shown in the evidence before us, even to the extent of being surgeons to county hospitals. Sir B. C. Brodie states that it would be very unjust to prevent a person, not a member of the College of Surgeons, from practising surgery; nay, he adds that if the person "had not a diploma of any kind, but had been received by the public as a practitioner in surgery, you should not turn him away, and say 'you shall not practise.' Some of our best country surgeons are in that position." Neither is there any existing law which will prevent the apothecary, or surgeon-apothecary, from prescribing and receiving fees as a physician, although he may never take the title; we need scarcely say that such a style of practice is becoming more and more common. So, then, as far as the *act* of practising is in question, the proposed law would give no greater facilities for the lower members of the profession to practise as the higher, than exist at present; it would not confer the title of doctor, and, therefore, the apothecary could not assume the rank or title of physician; nor would it make the licentiate of a college into a fellow; or a general practitioner into a consulting surgeon. Nay, even a law expressly adapted to this end would be altogether inoperative; for the pretenders to higher rank, experience, and position, would quickly find their level both in the profession and with the public. By both, the existing grades would be most certainly maintained, any law or act to the contrary notwithstanding.

As the one great evil of inequality of legal rights would be removed by the registration referred to above, the recurrence of the evil would have to be prevented by an equalization of the curricula and examinations of the various examining boards, and by the naturalization of those which are *quasi* foreign; or else summarily provided for by the extinction of the greater number. With regard to the latter proposition, we regret to find that it has not been mooted in the Committees of the House of Commons, as we think such a step would materially facilitate the equalization of the curricula and examinations of the remainder. One board for each great division of the United Kingdom would be sufficient for all useful purposes, and the remainder might either have their powers merged in this board, or compensation granted for the abolition of their privileges. Some difficulty that might arise as to the means, might be solved by the payments which we have proposed to be made by those whose qualifications would be legalized by the legislature, and who are now practising illegally. As regards England and Wales, the step would be easy; the existing metropolitan boards each contributing to the constitution of a general board, and merging their powers in it, as we formerly recommended, (Vol. II, p. 316). In Scotland, the boards might be partly abolished, partly consolidated in the same way. Thus, by reducing the number, the chances of unity of action would be increased. This method of consolidation had several influential supporters before the Committees, and is partly recognised by Sir James Graham in his bill of 1845; as to the necessity of an equalization of curricula, all parties seem agreed.

An examining board for each division of the kingdom being thus formed by the consolidation or abolition of existing boards, there is still required some means, by which their curricula and examinations might be made and kept equally extensive and rigorous, and by which greater regularity as to the preliminary and finishing elements of training might be attained. Sir James Graham provided, in his Bill of February, 1845, that a Council of Health should be established for these ends, in which the principal educational colleges and universities were to be represented,--the universities by professors *ex officio;* the colleges, each by one member of the college, nominated and chosen by the college. By this plan there would have been five university professors, six collegiate representatives, six other persons chosen by the Crown, and one of her Majesty's principal Secretaries of State,—in all eighteen. The details need not be given; we need only add, that the collegiate representatives were to go out of office every three years. On the 7th of May, Sir James Graham brought in an amended Bill, containing an alteration in the formation of the council, to the effect, that four persons only should be chosen by her Majesty, and that two general practitioners should be chosen by a college of general practitioners, which Sir James proposed to constitute. On the 28th of July, Sir James Graham brought in another amended Bill, in which the former constitution of the council was entirely annulled, and proposed to consist of one Secretary of State and twelve persons, to be nominated by the crown.

There is no portion of the evidence taken by the parliamentary committees, which so forcibly displays the crude and imperfect notions held even by the most eminent in the colleges, with reference to the fundamental principles of medical polity, as that which has reference to "a Council of Health." The gentlemen examined seemed to think that it would be a supreme court of Hygiene for the United Kingdom, as well as an authority set to watch over the conduct of the Colleges; and while some manfully asserted that the latter needed no control, they fully agreed as to the utility of the Council in sanitary matters. The debates on the Public Health Bill must have convinced these gentlemen how easy it is to talk innocently on this subject.

By the Medical Registration Bill, the duties of the Council were provided for by a more direct method: instead of a formal council deputing one of their body, the duty of visiting the examining boards and colleges was intrusted to the Secretary of State, who was authorised to depute an assessor for the purpose of supervision and correction.

We have only one remark to make upon the schemes just described,—namely, that the history of the agitation for medical reform shows that a mere extension of the curricula of education, or a greater stringency in examining students, has never had the effect of allaying discontent, or in the slightest degree satisfying the demands of the profession. We make this statement without any qualification or reserve, and we can therefore hope for no better result from the establishment of the proposed new methods. How far a representative council, that is to say, a body composed of physicians or surgeons elected by the profession at large, would satisfy the profession, can only be matter of inference—much would depend upon the way in which it exercised its powers; but of this we are sure, that a body of individuals irresponsible to the profession for their acts, will never command its respect; and we fully concur in the judicious opinion expressed by Dr. Holland, that the substitute for the proposed Council provided in the Medical Registration Bill, namely, the nomination of an assessor by the Secretary of State, "would be unsatisfactory to the profession, and far from advantageous to the public."

The question of a preliminary general education for the student was mooted repeatedly in the committees, and we find an opinion as to its necessity frequently and strongly stated; so that, on this head, we have little to add to that which we have stated in our last Number. It is a manifest defect in all the bills, that no provision is made for rendering this education and examination, previously to admission to studentship, imperative on every candidate for admission; for, although no exact curriculum of study should be defined, at least the general principle ought to be asserted.

Yet we need be the less surprised at this omission, when we consider that throughout the whole of the examination, the ethical and technical interests of the profession are altogether neglected. No question is put as to the propriety of establishing medical libraries and museums in the provinces; nor as to the encouragement or reward of industry and genius displayed in adding to the art and science of medicine; nor as to the better government of the profession, the quashing of disputes amongst its members, or the arrangement of controverted points in etiquette; nor as to the regulation of the relations of the profession to the public and the sick poor, and those other various topics, as to which the heart of the profession yearns for some rule and law and orderly arrangement. On looking into the effects of the existing powers in the body politic of medicine, we find much indeed to lament, in powers misapplied and time and opportunities wasted. The Royal College of Physicians—the elder sister of the Royal Society—might now, if a spirit of utility and honorable enterprise had directed its councils, have been *facilè princeps* in the medical commonwealth, and have even rivalled, in its accumulated honours and its list of scientific heroes, the great Society itself. Without characterizing its conduct, let us note a few of the mistakes, to use the gentlest word, which the authorities of the College have, from time to time, committed. In the first place, up to a recent period they spurned all self-taught genius from their threshold, or all who had not that essential badge, the diploma of Oxford or Cambridge. Without this, the College recognised no merit, genius, ability, skill. A body of men who could thus sacrifice the real to the nominal, would not display much prudence or tact in other matters; and we therefore find them shutting the door of their library upon their brethren the licentiates, and Dr. Yelloly has left in a record, that the loan of a book was denied him, even although he begged the favour through a fellow—himself being one who would have graced the presidential chair. So also, during the late discussion on medical politics, we learn that no trouble was taken to ascertain the opinion of the licentiates on the points in controversy. There are 160 fellows, 276 licentiates *intra urbem*, and 255 licentiates *extra urbem*; of the governing body—the fellows—forty or fifty only took any share in the opposition to the bills or the application for charters, but we have it in evidence, that no meeting of the licentiates of either grade was convened, or their opinion asked in any way. Yet the difference between the fellows and licentiates is confessedly only nominal.

Then what has this same College done for science? When the Hunterian Museum was offered to it by the nation, the offer was refused, although up to the early part of last century the College was distinguished for anatomical knowledge. By accepting that valuable museum, it might have become an imposing body before the public. So again, when the Royal Medical and and Chirurgical Society sought to remedy the negligence and indolence of the Collegiate authorities in the advancement of medical science, and to become more efficient by the possession of a charter, the College of Physicians not only petitioned against the grant of such charter, but passed a resolution prohibiting the fellows from furthering the objects of the Society in any way without permission!

The possession of the Hunterian Museum has given the College of Surgeons a higher professional position than the College of Physicians; but having provided for its custody, increase, and utility, and for the examination of students, the College seemed to think that it had done all that was necessary. Sir B. C. Brodie stated there were "no important questions for the council to consider, except those relating to the management of the museum and library, and the education of the students. If there is a government of the College, it is a government where there is nothing to govern except what I have just stated." If the profession at large had had a due share in the government of the College, this dull routine would have been broken up long ago, and we should never have heard of a College of General Practitioners, and of their attempt to add another to those medical corporate bodies of the metropolis, which, containing but a very small minority of the profession, affect to represent its wisdom and its interests, while they are with difficulty either persuaded or driven from the narrow circle of their corporate and personal interests, prejudices, and prepossessions, regardless alike of the feelings, the prejudices, or the interests of the profession at large.

From past experience, we are impressively taught that nothing is to be expected by the profession in general, from metropolitan corporations constructed on the exclusive and sectarian principles advocated by those now existing. Even the much-vaunted libraries and museums are of no practical benefit to the profession in the provinces. It is for our country brethren to ponder well whether any plan of reform should be accepted by them, which does not give them a share in the government of the profession at large, in the management of all affairs that concern their interests (such as the education and examination of students), and a ready access, at their own homes, to museums and libraries, with, at the same time, the means of local self-government. Until this be done, dissatisfaction cannot fail to arise.

The question then to determine is, how shall the interests of the corporations, the profession at large, and the public, be reconciled? and how, out of the chaos of conflicting interests, can order and organization be evolved? To this end, let us ascertain what the profession demands, and then see what arrangement will best serve the exigencies of the case.

In the first place, we find that the profession at large, feeling their exclusion from the councils of those individuals who represent the profession before the public and government, are actuated by a desire to destroy this anomaly, independently of any object to be gained thereby. Their pride is wounded; their natural wish for a share in the management of their professional institutions is disappointed; they are but impotent spectators of proceedings carried on in no conciliatory spirit, if not with self-sufficiency and presumption. This *esprit de corps* finds no vent except in institutions unrecognised by the law, and with no stronger bond of union than the volition of its members. That feeling, in short, which leads to great political changes, when the mass of a nation is excluded from any share in the government, and when a narrow-minded, presumptuous oligarchy assumes to itself the wisdom of the whole people, is and has been strongly operative in the medical profession; and never can be, never will be, and (we will venture to add) never ought to be satisfied, until the full enfranchisement of its members is secured. For proof of this, we need only turn to the pages of the medical journals, and the records of the numerous associations which have been successively formed; a *representative government is the universal theme,* and by this means only can the multitudinous voices of twenty thousand scientific men be harmonised into an expression of their wishes, or of the workings of the vast professional mind.

With the greatest admiration of Sir B. C. Brodie's professional character, and esteem for his private worth, we cannot but dissent from his remarks when questioned upon this part of the subject, and consider his statements as unworthy of him. To the question (2688) "Do not you think that the profession would express generally a desire to know how the council discharged its duties, rather than be ignorant of the transactions and conduct of such an important body?" Sir Benjamin replies, "I do not believe that what I call the profession would care at all about it;" and when asked what he meant by the phrase "the profession," added, "I mean that the men of good sense do not care anything about it." Independently of the singular doctrine here expressed, that those only constitute the profession whom Sir Benjamin thinks to be "men of good sense," the expression of which, indeed, the highest professional position cannot warrant, how can that gentleman term those individuals "men of good sense," who take no interest in the splendid museum and library, and, above all, no interest in the education and examination of those who have to engage with them as junior colleagues, in the cultivation and practice of so great and glorious a science and art as that of medicine? How can individuals so dead to what is honorable and important in the affairs of the profession, so deficient in true *esprit de corps*, so wrapped up in the mere trading department of their calling, as to take no interest in its science or literature, be termed "men of good sense?" Or, to make a specific application of this declaration, with what justice can the numerous and influential members of the Provincial Medical and Surgical Association be referred to that shadowy portion of the profession which does not comprise any "men of good sense?"

It is remarkable, however, that while the representative principle has been so sedulously and emphatically put forward as an essential element in the organization of the profession, there has been no plan set forth by the leading associations, which is at all adequate to the end of representing the mind of the profession, or which bears marks of a knowledge of those principles upon which a representative system should be based. The constitution adopted by the Provincial Association, contains the *elements* of the most effectual organization of the profession at large that could perhaps be devised, and only wants enlargement and adaptation to render it complete; yet we do not remember that the Council of that body has ever come forward with a well-defined plan, or indeed with any plan whatever, except possibly that crude and impossible scheme which would lump the profession into one huge overgrown College, with no other privilege or bond of union than the right to vote for officers. This, we need hardly say, is not organization; for organization is a *combination* of *organs*, and not of mere homogeneous particles. Such an arrangement might satisfy, for a short time, the spirit of change; but so soon as it was found ineffective in advancing the material interests and scientific progress of the profession, the agitation for reform would recommence, and the centralization of power would be attacked.

The views of the profession in the metropolis and provinces, as to medical organization, have been fully stated from time to time during the last few years by various associations constituted for this purpose; so that we can have no difficulty in ascertaining them. Of these bodies, the most important and influential is the Provincial Medical and Surgical Association. Dr. Barlow, to whose labours in the cause of medical organization we have already referred, was one of its first and most influential members; and in its earlier reports on the subject of medical reform, we think we can detect his *ipsissima verba.* "The principles" which the association have put forward are: Firstly, an efficient primary qualification, without which no man should be allowed to practise; secondly, equal right to practise in every part of her Majesty's dominions for all persons so qualified; and, thirdly, the recognition of the representative principle in the constitution of the councils or governing bodies. More recently we find the association supporting the Medical Registration bill.

In the metropolis, the British Medical Association, and in Ireland, an association formed on the model of the Provincial Medical and Surgical Association, were the exponents of the more active reformers in the metropolis and in Ireland; while the North of England Medical and Surgical Association, and various local bodies in Scotland, had similar objects in the north. The British Medical Association, with Dr. Marshall Hall as its president, put forward a programme of its views in July 1839, containing the outline of more sweeping changes. "All legally qualified members of the medical profession in the British dominions" were to be united "into one faculty, to be termed the British Faculty of Medicine," which was to elect periodically by ballot, in each division of the kingdom, a governing body, to be called "the General Medical Senate," to act in London, Edinburgh, and Dublin, and to meet from time to time, and "act unitedly in passing and administering all necessary laws, for the government and protection of the faculty." These senates were to superintend the public medical police, and the education of the profession and of chemists, and to keep a public register of both. The Irish Association, with Mr. Carmichael at its head, adopted substantially the views of its English sister, and in 1839 petitioned Parliament for an uniform system of preliminary and professional instruction, for perfect similarity of qualifications and equal privileges throughout the British dominions; and for "one superintending body in each of the three divisions of the kingdom, founded on the same principles, and governed by similar regulations, through whose examinations, and by whose license alone, shall admission to the profession be in future attained."

The North of England Association, which included some of the most influential physicians and surgeons in the district, adopted, in 1841, views in all respects analogous to those of the English and Irish Provincial Medical Associations: but at the anniversary meeting in 1842, another plan was brought forward, proposing to incorporate physicians, surgeons, and general practitioners in separate incorporations, the last mentioned as "licentiates in medicine;" each faculty or college to elect a council or board of management periodically, which was to nominate certain persons, two thirds of whom were to be selected by the Secretary of State, and these deputies from each college to form conjointly with laymen a general council in each division of the United Kingdom. These councils would be the governing medical body for each division, and would appoint examiners for the license to practise, without which no person could be a candidate for admission into any of the colleges. In addition to these matters, the plan provided for a general registration, reciprocal privileges, the right to recover charges for attendance; and for the transformation of the Apothecaries' Company into a college of pharmacy.

It will be seen that the bills of both Sir James Graham and Mr. Wakley were constructed with the desire to carry out the suggestions and views of these various associations. With the want of that practical or worldly tact, for which the medical profession is as remarkable as the most purely scientific body, these associations contented themselves with the enunciation of bare principles and outlines, as if that were sufficient for their purpose. This conduct is, indeed, but a counterpart of that now witnessed on a large scale on the Continent, where the constitution-makers invariably proclaim comprehensive and abstract general principles, overlooking, in the largeness of their conceptions, the necessity there is for a more humble and painstaking acquaintance with details. Thus, for example, while the representative principle, and even the use of the ballot-box, were recommended by the various associations above mentioned, we find no hint as to the means whereby the principle might be applied, or as to how the ballot-box could be sent round amongst the twenty thousand practitioners scattered through the country. The courteous attempt made by Sir James Graham to apply this representative principle to the constitution of the supreme council, is just what might have been expected from a statesman who has had no, or at least not much, experience of the components of a literary or scientific body like the medical, and who excusably forgets that such a body knows little of political economy, or of the mysteries of government.

The phrase of Dr. Barlow, that the profession should "be organized conformably with its natural tendencies," is full of wisdom, and should be writ on the memory and judgment of every medical reformer. It is the one great principle of all successful organization; it has a universal applicability, not to mankind only, but even to those lower creatures which man compels to serve him. The Turk or Greek, the German or French, the Englishman or Irishman, can alike be best governed by an organization conformable to their natural tendencies; none other will indeed permanently serve the purpose.

It is much to be regretted that Dr. Barlow neglected to apply this great principle of organization. We think it is Professor Smyth who, in his Lectures on the French revolution, gives reformers the advice to avoid all subjects of debate and collision as much as possible, and "to hasten on to practice, and to the real wants and wishes of their fellow-citizens, as they see them plainly existing before them, not expecting too much from themselves or others, and, above all things, losing no time." If this had been done by Dr. Barlow and the leading members of the Provincial Medical Association at its commencement, a complete system of medical registration might now be in full operation (in the provinces at least), by its agency, and thus one great object, for which they have been and are still struggling, would have been attained. Again, if with a careful eye to the wants of their fellow-practitioners, and their desires to attain the medico-political, ethical, and sciential objects for which they are associated, they had turned their efforts to the attainment of a royal charter for the Association, and to the formation of a liberal constitution and plan of government, and thus have given the humblest practitioner an interest in the success of the corporation, such a powerful body would have arisen, that the other objects they have sought, namely, equalization of curricula and privileges, and a reconstruction of the metropolitan colleges on a more liberal basis, would by this time have been fully attained.

The natural tendencies of the profession are amply disclosed in the history of the agitation for medical reform. In the first place, *union and communion* are earnestly desired; and this in accordance with the characteristic instinct of man. To take the first instance that comes to hand (and this, although modern, is in all respects analogous to what was felt and expressed a half century ago): At a numerous meeting of the Royal College of Surgeons of Ireland, held on Thursday, January 24th, 1839, it was resolved, with one dissentient voice,—"That it is greatly to be desired that measures should be taken to make this college the *centre of union* for the practitioners of Ireland;" this we find recorded in the 'Dublin Medical Press,' and in twelve successive numbers of that journal there appeared fifteen responses to the key-note thus struck. And if we take up any one volume of any medical periodical open to communications on medical reform, we shall find expressions of earnest wishes for union, and deep regrets for the existing disunion. It was to gratify this desire that the Provincial Association itself was called into existence; it was because this human instinct has been and is ever operative, that not only have all our existing medical corporations and societies been formed, but that, if the whole profession were united as is desired, new centres of union would be developed, and the attraction of homogeneous particles to them would at once be commenced. This natural tendency must therefore be not only gratified but provided for.

The love of the art and science which an artist or philosopher cultivates, impels him to another step, or gives him another natural tendency, namely, to the improvement of his beloved art or science, and to converse with the like-minded. This has also been a leading motive in the formation of medical societies and associations, and will, like the preceding, be always operative. Hence the necessity of providing for its legitimate and regulated action in an organization of the profession. Without facilities for professional communion and scientific cultivation, a plan of organization would certainly be found to be imperfect.

The continual personal contact into which professional men come, renders it necessary that there should be laws for the regulation of their conduct towards each other, and a power to judge as to the infringement of those laws, to see that they are obeyed, and to punish the disobedient. This, which is a fundamental principle in society, even amongst irrational creatures, cannot be overlooked in a plan of medical organization, and its application must be provided for in any scheme of the kind. *A fortiori*, the principle is equally applicable to the relations of professional men to their patients and to society at large.

The mere technical or material interests of the profession demand also mention here, for we find that union for their protection is one of the natural tendencies of the profession. Thus the medical relief of the sick poor has ever been a fruitful source of discontent and complaint, and would of itself, independently of the management of public hospitals and dispensaries, by whatever name known, demand the particular attention of the medical legislature. It is only by a study of the natural tendencies of the profession, that the question can be fairly grappled with. There can be no doubt, indeed, but that if the profession were fully organized, and the duty of relieving the sick poor were intrusted to it in full confidence, such duty would be effectually and economically performed, in harmonious cooperation with the public authorities. At present there is nothing but discord and neglect.

We might add other examples of these natural tendencies, but the above must suffice. Our readers may find further illustrations in the reported proceedings of the various medical associations and societies, and in the numerous letters published in the weekly journals. Indeed, to a large proportion of the profession, the columns of the latter are the only means of attaining those objects which we have alluded to. If there be an instance of poor-law hardship, the weekly journal receives the complaint; if there be a breach of etiquette, or a flagrant case of quackery, it is the 'Gazette,' 'Lancet,' or 'Medical Times,' which constitutes the tribunal of appeal—how imperfectly need not be shown.

The objects of an organization of the profession naturally divide into two classes, namely, the local and the general. The local objects would be, firstly, the defence of local interests, as, for example, against the encroachments of quacks, or of Boards of Guardians or of Health: this could only be done effectually by the local organization of the profession; a council sitting in London could only have influence as a court of appeal, and as an influential medium of communication with the Government or Legislature. An effective registration must be local, for reasons already stated; an effective government of the profession, in relation to etiquette and ethics, must also be local; to carry cases of this kind to a metropolitan council (except as a court of appeal) would be as feasible as trying all Assize or Quarter-Sessions trials before the Lord Chancellor or the Court of Queen's Bench. So, also, whenever the profession, as a body, undertakes to bring its moral influence to bear on society, that influence will be most effectually, if not solely, exercised by a local organization, whether it have reference to the suppression of quackery, to the management of medical charities, or to large and important questions of hygiene.

When we turn from a consideration of the material and moral interests of the profession to those which are sciential and artistic, we again find that the mere conglomeration of the profession in one or more Colleges will do little, as compared with an effective local organization. The Library and Museum of the Royal College of Surgeons are, *de facto*, inaccessible to the provincial practitioner; for a journey to London, solely to refer to a book or a preparation, would scarcely be undertaken by the most enthusiastic. The want, in the country, of those facilities for physiological and literary research, checks the efforts of inquiring minds or drives them to London; and thus the beneficial influence which the more gifted would exercise over their less fortunate brethren is lost. Add to this, that in numerous country districts no opportunities for intercommunication of ideas are afforded by medical societies, such as the metropolis and the larger towns present; and we have strong inducements for an organization which shall combine the practitioners of the provinces into local societies, the object of which shall be the legalized cultivation of medicine and the collateral sciences, and the authorized regulation of all matters appertaining to the profession. Thus every large town, with the adjoining district, would have its Royal Medico-Chirurgical Society, composed of every registered practitioner resident therein—membership being compulsory—with full power to execute the laws regulating the etiquette and ethics of the profession, and acting as its mouthpiece to the public.

The profession being thus organized by districts throughout the provinces (and even in the metropolis), a basis would be afforded for its representation in a supreme or central council. Each district might send its representative; or, as these might render the council too numerous, each might nominate six, or such other number of medical members of council as might be determined upon, by a majority of votes, and those candidates who had the majority of nominations by districts should be considered as elected to represent the profession; the districts would thus become electoral colleges. Nor would we let the representation of the profession rest here; but, using the same machinery, we would have the profession represented also in parliament, both in the Houses of Lords and Commons. The President of the Council should be a member of the faculty, with a seat in the House of Lords, *ex-officio,* while the electoral colleges or districts should vote for representatives in the House of Commons. The growing importance of public hygiene, and the still greater importance it will attain to, with the increase of population, and the growth of towns into large cities, render such a representation of the profession in the great council of the nation far more necessary to the public than to the practitioner; for there can be no doubt that much valuable time might be saved, and much more precision given to legislative hygienic measures, by the co-operation of (say) four medical members of the House of Commons, elected by, and having the confidence of, the majority of the profession.

In establishing these proposed district faculties, we would not allow any confusion of those divisions of the profession, which the public demands, and which long custom has sanctioned. The two classes of consulting practitioners (whether physicians or surgeons) and general practitioners, should be maintained, and the relative position of each assigned; probably the organization of the town-councils under the Municipal Reform Bill would present the best model, the consulting practitioners being the honorary class, or fellows, and the general practitioners the commonalty, or members, but all voting equally. The management of the affairs would not be by one select body, or *council,* but by several select bodies, or *committees,* thus dividing the honours and duties,—an important point where so many individuals are equal in wealth, station, and acquirements. This assignment of two classes would necessitate a corresponding degree of preliminary training and professional education to each, and a corresponding division of classes in the metropolitan colleges.

With regard to the latter, we would leave them untouched, except in such minor details as would reform abuses which the governing authorities desire to be removed, and except such provisions as might be necessary for placing them under the general control of the supreme council, so that their curricula and examinations should be equalized. As we have already proposed, the Apothecaries' Company should cease to examine students, and its functions be transferred to the University of London and the Colleges of Physicians and Surgeons. The grades of fellows and members should continue in each college, but all the licentiates now practising as physicians should be made "junior fellows," while the licentiates practising as general practitioners should be designated members of the College of Physicians. We would also suggest that general practitioners be admitted, after a not too rigid examination, to membership of the College of Physicians, on producing satisfactory testimonials from the faculty of their district, and paying a suitable fee. We would have it understood, however, that doctors of medicine, or even fellows of either college, should not rank in the district faculties as consulting practitioners, if engaged *ordinarily* in the practice of midwifery or in pharmacy; both physicians and consulting surgeons might, however, be consulted in obstetric cases, and treat ordinary medical or surgical cases, but be forbid pharmacy. General practitioners might, if they pleased, keep open shops, and fellows of colleges might practise midwifery exclusively, and keep their rank.

We would place no restraint on the system of apprenticeship or private pupilage, further than this, that no youth should be received, as either apprentice, private pupil or student, before the age of sixteen or seventeen, or before he had undergone a preliminary education and examination, such as we have before alluded to. We feel inclined to think that, with these restrictions, the present system of pupilage might be left undisturbed.

We would thus reconcile the interests of the corporation and the profession at large; but the question will arise, whether this proposed organization —efficient as it would be for evil as well as for good—should not have some check upon its action from the public at large, and thus we shall have to discuss how far the interference of laymen should be permitted in professional matters. All experience teaches that mere literary and scientific bodies are apt to degenerate into merciless cliques, the bitter opponents of novelties and the defenders of dull mediocrity; but experience does not as certainly teach that an infusion of laymen counteracts this tendency. Much of the oppression or harshness that would arise in local faculties would take its origin in professional jealousy and envy; no legislation could counteract the manifestation of these; but any extreme measures might be prevented by giving the oppressed individual a facility of appeal to the higher court; as, for example, by one or two members of the council making a quarterly visitation of the various local faculties, and hearing and determining all cases of dispute not settled by the local authorities of the faculty. By sufficiently extending the districts, much local rivalry might be checked; and by the admission of distinguished noblemen, gentlemen, or men of science, as honorary members, fellows, or vice-presidents, a dignity might be given to the proceedings which would serve to keep the froward and unruly in check. With regard to the constitution of the supreme council, there can be no doubt of the policy of having a proportion of laymen; the sentiments of Dr. Seymour are perfectly apposite, who observed, in his evidence before the Medical Registration Committee of 1847, that as parties would certainly be formed amongst the medical members, the lay members would act as arbitrators and umpires.

As the plan of local organization which we propose would involve the necessity of a local habitation, it is necessary to consider the important point of the ways and means. In those towns in which there are medical schools, or literary or philosophical societies, there would be no difficulty as to at least a temporary place of meeting; and a small annual contribution, together with the fees payable on admission and registration of new members, would suffice for the ordinary current expenses. There can be no doubt but that an appeal to the wealthier classes for assistance in the erection of suitable premises would meet with a liberal response; while the stability and perpetuity which a legislative enactment would give the institution, would encourage a continued support by legacies and donations, especially of books, preparations, &c.

We perhaps ought now to state and answer objections to our plan, after the approved method of theorists. We are certain, however, that this is unnecessary, since such objections would be not less applicable to all medical societies and associations, for our plan only carries out their objects by a more extensive and permanent organization. For example, if it be objected that physicians, surgeons, and general practitioners should be incorporated separately, we answer, that in no medical society or association, established for common professional objects, is there any such separation of grades. As well, in fact, might there be separate journals for each class; and so on with all similar objections.

In presenting this, our promised method of organization, we feel, however, that we have imposed upon ourselves an arduous task. We trust, however, that it will recommend itself to the profession at large, and to the Government and the corporate authorities, by its simplicity, by its adaptation to the wants and wishes of the profession, by its recognition of corporate rights and duties, by its regard for the demands of the public; and, above all, by its elevation of the medical profession, for the first time, probably, in the history of the world, into a comprehensive corporation, able to act as a whole on society, and fulfil as a whole those high duties which Providence has allotted to it. And while it would thus act as the applier of science and skill, it would also become more and more the depository and guardian of the great and important SCIENCE OF LIFE—a science which, requiring a special training for its study and advancement, is being abandoned by those scientific corporations that combine with it the study and advancement of the physical sciences. But whether the method which we here advance with diffidence be approved or not, we would desire no greater honour than that which will crown the statesman, who shall succeed, by some abler method of organization, in the attainment of those great objects which we have endeavoured to set forth. It would be a mighty step in European civilization, and another guarantee for the security of religion and morals, and for the improvement of mankind. Such a step might indeed be taken at once by the Provincial Association. Let that body appeal to the profession at large for support in obtaining a charter of incorporation, to enable it to carry out its present objects; let its constitution be remodelled, so as to give the most free and full franchise to its members; and especially let it provide ample securities against local cliqueism; and appealing to the Crown, with the support of the whole profession in the provinces (and we think we could promise it that support), let it show that the PUBLIC GOOD is the great end of its establishment. There could be no doubt as to the result of the application, and as little doubt, provided the objects of the Association were fully carried out, that all the other objects now sought by reformers would appear of minor importance and of easy attainment.

ART. III.

On the Nature and Treatment of Stomach and Renal Diseases; being an Inquiry into the Connexion of Diabetes, Calculus, and other Affections of the Kidney and Bladder, with Indigestion. By WILLIAM PROUT, M.D., F.R.S., Fellow of the Royal College of Physicians. Fifth Edition, revised.—*London,* 1848. pp. 596.

IT is the boast of the present age, that every new discovery, no matter how abstract apparently in its nature, soon receives some practical application. Thus it is observed that metals deposited by galvanic influence, accurately represent in relief the slightest scratch upon the surface they have been applied to; and in a short time the art of electrotyping becomes familiar to every schoolboy. It is remarked that light favours crystallization; and ere long the daguerreotype is the chief portrait painter. The influence of light in promoting the action of the salts of silver on organic matter is no sooner known, than ingenuity is exhausted to obtain photographic landscapes. The xyloidine of Pelouze soon becomes the gun-cotton of Schönbein; and this agent of destruction is rapidly changed by the unquiet spirit of experiment into a powerful styptic. The vats of a soap-boiler are found to be corroded; the principle which produced this effect is detected in burnt sponge, long known as a popular remedy for goitre; and the specific *par excellence* for scrofula is at once isolated and recognised. Nitrous oxide, ether, and chloroform,—the latter especially, as the construction of the name implies, a very recent chemical invention,—are extensively used to lap the senses in forgetfulness, during operations the most painful. It has been for some time known that metallic salts decompose hydrosulphuret of ammonia, and that the latter compound constitutes the most unpleasant portion of the effluvia from decomposing animal matter; the practical application of these facts is now being extensively tried; and nitrate of lead, chloride of zinc, and sulphate of iron vie with each other in deodorizing the nauseous emanations from bilge-water and cesspools. The inspissated milk of an Indian tree is observed to acquire plasticity when heated, and to become inelastic and tenacious when cold; and in a wonderfully short period the practical applications of *gutta percha* have become innumerable. In every pursuit we may perceive the same incessant and objective activity. Science rushes towards art with railroad velocity; and in the Old World, as well as in the New, *go-a-head* seems to be the generally accepted motto.

But as on the *chemin de fer* stations are requisite, not only to permit passengers to get in and out, but also to enable the stoker and engineer to inspect their stock of coals and water, and the conductor to examine whether the axles require their necessary supply of palm oil; so in the rapid progress of discovery, it is well to have pauses for reconnoitring the direction in which we travel, to examine whether the principles upon which we proceed are well founded, and the things that we have hitherto relied on as facts are really ascertained and stable. Practical rules are of extreme importance; but it is equally a truism, that the success of practice is always proportional to the soundness of theory. Now, the revision of a standard work, at once transcendental and practical like Dr. Prout's, affords a favorable opportunity for this survey; to return to the analogy we have referred to, a new edition like the one before us may be considered a station on the railway of science; it is our business, as guards on the line, to see that "all is right."

At the time that Dr. Prout first promulgated his pathological views, now upwards of twenty years ago, Broussaism, or, as its supporters delighted to call it, the physiological doctrine of disease, was that which principally prevailed. The majority of physicians in this country, it must be acknowledged, professed to be anti-Broussaists; and actually did combat, and that in some instances successfully, certain hypotheses advanced by the great founder of this school of pathology; but without being themselves aware of it, their treatment and language were both practically influenced by the dogmas which Broussais enunciated. For the most part, they seemed to imagine that in denying that typhus and intermitting fevers were symptomatic of gastro-enteritis, they had done enough towards refusing all allegiance to the theorists of the "French School," as they were pleased to call it; little aware that when they regarded all diseases as proceeding from the too great or too little activity of the organs, and that this plus or minus activity depended on a local plus or minus vitality, they were but reflecting the doctrines promulgated by the ex-military surgeon of the Empire. The fundamental notions of Broussais' system were just as prevalent at the time in England as on the Continent; although it must be acknowledged that they did not lead to as much mischief in practice. We consider his fundamental errors to have chiefly consisted in the opinions, that all diseases had a local origin in the solids of organs; that disease, no matter how long its persistence, never changed its nature; and that medicines produce the same physiological effects in all doses, and in all pathological conditions. We repeat that these doctrines were at the time equally prevalent in England and on the Continent, although it is possible they were not here stated with equal precision, nor so consistently influential as regards practice; and it was during the general reception of these opinions, that Dr. Prout first promulgated the views repeated in the fifth edition of his work now before us.

The translation of Andral's work on Pathological Anatomy led the profession to a new line of thought. The consideration of "lesions of the blood" diverted attention from an exclusive regard to the alterations of the merely solid constituents of the organism. We do not mean to say, that the medical mind was not prepared for this transition. On the contrary, we hold that no revolution in doctrine can be successful, which is not pioneered by the general preparedness of ordinary practitioners. On this occasion an universal consent was at once accorded, and the reviewers spoke as if Andral's views had been long their own. So strong is the force of truth, when it comes to be applied at the instant at which the progress of knowledge has made ready the way for its reception.

It was in this state of pathological doctrine, that Dr. Prout first announced those views of nutrition and secretion, which have since developed a theory of disease more consonant with clinical experience, and more extensively applicable than any which had preceded it. Broussais and Andral regarded the phenomena of disease as results; Prout looked on disease in action. The revelations of the former concern the statics of disease; those of the latter, its dynamics. The former ask, if an organ be in a state of phlogosis or sthenic hyperæmia, congestion or asthenic hyperæmia, hypertrophy, or atrophy,—if its secretions be excessive or suppressed, &c. Andral would, in addition, inquire whether the fibrine, albumen, and colouring matter of the blood be increased or diminished. But Prout does not so much interrogate the organ, as the functions of the system; and that in regard not so much to their past performance, as to their present action. It must be admitted that the morbid anatomist did not neglect the performance of the functions; he recognised increased vitality and diminished vitality, increased nutrition and diminished nutrition, increased secretion and diminished secretion; he moreover availed himself of the known phenomena of nervous agency, to explain symptomatic fever and metastasis; and he acknowledged the functional termination of disease in crisis. But with him, the disordered performance of the functions was the cause of the disease; the visible and tangible condition of the organ, the disease itself. Prout, however, regards local phenomena, whether during life or after death, as but the marks and tokens of the true disease, which latter essentially consists in the disordered function. Disease is not a self-existing and persisting entity. Like the living organization in which it occurs, it is a perpetual vortex of renewal and decay. You watch the mist that seems to rest upon the hill side, or the column of blue smoke surmounting the cottage chimney; or you listen to the long-drawn note of the thrush or blackbird, and you scarcely can persuade yourself that your senses are informing you of things that are instantly beginning, and as instantly ceasing, like the defined flame of a taper. Such are organisms and diseases. Death may arrest them in a particular stage; but how imperfect the knowledge that can be collected from an examination of that stage. Like the flight of a bird struck down by the fowler, you cannot tell whence it was coming, or whither it was going.

However, the correct definition of disease would be, after all, but a trifling gain, if the inquiry became arrested at that point; nay, it is possible, that if the exact knowledge of morbid anatomy did not become united to an equally exact knowledge of disordered function, the theory of disease which considered the former as the effect, and the latter as the disease, might have operated mischievously. The knowledge of the existence of a force of attraction with whose laws he was not acquainted, would have badly compensated the mariner of the olden time, for a shaken confidence in the celestial phenomena which experience had given him for his guide. In like manner, if the attention of the physician, instead of being directed solely to the local signs and symptoms, was diverted to look on them as merely secondary to an obscure and vague alteration of the function of nutrition, his diagnosis would be mystified, and his treatment paralysed. It was necessary, therefore, that the functions should be subjected to a strict analysis, their operations defined, and their relations determined; moreover, to render this knowledge serviceable to the diagnosis and treatment of disease, it was requisite to mark the connexion between local phenomena or symptoms, and disordered functions, as well as the influence of food, medicines, and physical agents, on the performance of the functions. This task Dr. Prout has undertaken, as far as regards the function of nutrition; and although we may not, in every instance, adopt his conclusions, we would do full justice to the originality and research from which they have proceeded.

In fact, to Dr. Prout must be attributed the honour of being the founder of that school of chemical pathology, which has at present so numerous a class of followers in the medical profession. Setting out with the proposition that the compound principles which enter into the formation of the organs of animals are presented to them ready prepared in the aliment, the alumni of this sect proceed to trace the stages by which those principles are converted into blood, and then into the solid tissues. They subsequently endeavour to explain the products of the disintegration of the fabric—a process as constant as its growth,—until, by a series of decompositions, the ultimate excretions are formed, which it is the business of certain organs to eliminate. The essential aliment, when taken into the stomach, becomes dissolved; it also undergoes that which chemists denominate an isomeric change, by which its properties become modified, without any corresponding alteration in composition. It then becomes absorbed, and is changed into blood; and being brought into contact with the tissues, it solidifies within their structure. The above is what takes place in the albuminous tissues: but those tissues which by ebullition form gelatine, have not necessarily the same composition as the food, for they will grow although there may not be any gelatine in the latter. Thus the egg contains no gelatine, and yet the chick has cartilages, ligaments, and tendons: it is therefore necessary to suppose that gelatine in this case becomes formed by the decomposition of the albuminous constituents of the blood. In the foregoing views all iatro-chemists agree. They may each employ a nomenclature differing from that of others; but the essential doctrines are identical. Thus Dr. Prout gives to the entire process of nutrition and secretion the name of "assimilation." That portion of it which consists in the gradual formation of blood, and the growth of tissues, he calls "primary assimilation;" whilst to the disintegration of the tissues, and the production of the secretions and excretions, he gives the name of "secondary assimilation." Besides these he uses a number of technical terms, such as, reduction, conversion, completion, albumification, gelatification, which are intended to express the doctrines we have referred to, but which we think render them more obscure. Liebig, Mulder, Dumas, and others, who entertain similar views, use a different nomenclature. But the theory we have indicated, is the skeleton upon which all chemical physiology and pathology rests; and for this theory, whatever it may be worth, we are indebted to the inventive genius of Dr. Prout.

No doubt, in many details, Dr. Prout's doctrines differ considerably from the views of those who have followed him in traversing the same chart. This philosopher does not entertain the first proposition which we have stated, in as exclusive a manner as most other chemical physiologists. He does not agree with Mulder in considering that the proteine constituents of the blood, tissues, and milk, must inevitably have been contained in a proteine form in the food: nor does he think with Dumas that the oleaginous matters found in the animal are always previously contained in the aliment. Again, he agrees with Tiedemann and Gmelin, in regarding the process of blood-making as a very complicated one, which requires several stages for its completion, and to which the functions of many organs are subservient; in this respect also he differs from the opinions latterly expressed by others. Moreover, he does not so absolutely connect animal heat with the chemical results of respiration, as most chemists now are accustomed o do; but, on the contrary, is inclined to regard the solidification of lbumen, gelatine, &c., in the tissues as a principal source of caloric. We ay, in the last place, allude to another peculiar opinion, namely, that ch tissue yields by its waste some peculiar product; thus in the urine, supposes the urea to proceed from the waste or secondary assimilation gelatinous tissues; uric acid from the waste of the albuminous tissues; the phosphates from the destruction of nervous tissue. It would be nature to decide as to the truth or falsehood of these views. We shall l for a short time on each of them in turn, for they involve subjects xtreme importance; but whether they may be ultimately received or rejected, and those now or at a future period advanced by other chemists in opposition be adopted in their stead, these considerations cannot affect Dr. Prout's claim to priority in the introduction of a system of physiology, of which these are but subordinate departments.

As to the inquiry first put, we believe, by Beccaria, "Is it not true that we are composed of the same substances which serve as our nourishment?" we do not think that science can as yet give more than an approximative answer. Indeed, in one point of view, we are entitled to give it a negative reply. There are two classes of tissue which, undoubtedly, do not require for their growth that principles identical with them in composition should be contained in the food. No one maintains that anything like nervous matter actually exists in vegetables, if we except Dutrochet, whose notions on this subject were never received. To say that there is an analogy between the oleaginous matters of plants and the fats of the brain, is completely begging the question, which is one not of analogy but of identity: not about similarity of properties, but of quantitative composition. Again, it is notorious that the bones, ligaments, tendons, &c., will grow, although there may not be any gelatine in the food. The chick not yet escaped from the shell, has all its tissues more or less completely formed, although its nourishment has been confined to the albumen and reddish oil of the yelk. Therefore, the nervous and gelatinous tissues being excluded from this consideration, the question becomes narrowed to the albuminous tissues. Must the food contain an albuminous material identical with that forming the albuminous tissues of the animal? No! the muscle of the infant is composed of a matter different from the caseine of the milk which nourishes him; all parties admit that caseine and fibrine are not identical in composition. So that it must be admitted that the animal has the power of modifying the composition of its food, to fit it for the growth of its tissues. But although we may admit that the albuminous constituents of the food are capable of becoming modified within the organism, so as to become identical with the materials of the existing albuminous tissues; or even, of becoming decomposed to form pabula for the nutrition of the nervous and gelatinous tissues, are we entitled to suppose that albumen can be formed in animals, in a manner and by a process similar to that by which it is in vegetables? Liebig, in his late work on the 'Chemistry of Food,' seems to think we are. In opposition to his formerly-published views, he considers that we have a right to believe, that if the elements necessary for a proximate constituent of a tissue meet within the organism under conditions favorable to their union, they may unite and form the compound proper for the nourishment of the tissue. Dr. Prout has always maintained a similar opinion. He even goes further, and imagines that in extreme cases the elements may be converted into each other, or even generated. As the subject is one of great importance, we shall extract Dr. Prout's views at length; and we find it the more necessary to do so, as we feel called upon to dissent from them.

"*Of the converting powers of the stomach, &c.* Though the proportions of the different ingredients of the chyle, as ultimately formed, are liable to be varied by the nature of the food; yet, whatever the nature of the food may be, the general composition and characters of the chyle remain always the same. The stomach, therefore, must be endowed with a power or faculty, the agency of which is to secure this uniform composition of the chyle by appropriate action on such materials as circumstances may bring within its reach. Two, indeed, of the chief materials from which chyle is formed, namely, the albuminous and oleaginous principles, may be considered to be already fitted for the purposes of the animal economy without undergoing any essential changes in their composition; but the saccharine class of aliments, which form a very large proportion of the food of all animals, except those entirely subsisting on flesh, are by no means adapted for such speedy assimilation. Indeed, one or more essential changes must take place in saccharine aliments, previously to their conversion either into the albuminous or the oleaginous principles.

"Under *ordinary circumstances,* the essential changes which alimentary matters undergo in the stomach are most probably altogether chemical. That is, the changes are such as do take place, or rather such as would take place, if the elements of the substances thus changed in the stomach could out of the body be so collocated, as to bring into action the affinities necessary for the changes produced in the stomach. Thus, as we know, the saccharine principle spontaneously becomes alcohol, which is merely an oleaginous body of a weak kind. When, therefore, in the stomach it is requisite that sugar be converted into oil, it is probable that the sugar passes through precisely the same series of changes it undergoes out of the body during its conversion into alcohol. We cannot trace the conversion of sugar into albumen, because we are ignorant of the relative composition, and of the laws which regulate the composition of these two substances. The origin of the azote in the albumen is likewise at present unknown to us, though in all ordinary cases it seems to be appropriated from some external source.* That the oleaginous principle may be converted into most, if not all, the matters necessary for the existence of animal bodies, seems to be proved by the well-known fact, that the life of an animal may be prolonged by the appropriation of the oleaginous and other matters contained within its own body.

"Under ordinary circumstances, then, the converting powers of the stomach must essentially consist of the three kinds mentioned, viz. the conversion of saccharine aliments into albuminous and oleaginous principles; the conversion of albuminous principles into oleaginous principles; and the conversion of oleaginous into albuminous principles. These three forms of the same function may, in fact, be considered as so many subordinate functions, each distinct in its characters, and each liable to be affected by disease, &c., independently of the others. Of these three subordinate functions, the first, viz. the conversion of saccharine into albuminous and oleaginous aliments, is by far the most important. And as the appropriation of saccharine aliments is not only common to many animals, but even to vegetables, it may perhaps be viewed as the lowest step of alimentary renovation; in other words, as constituting the immediate act, as it were, by which the stamina or groundwork of organized beings is formed. Hence, though the appropriation of saccharine aliments by animals must be a very different process from the appropriation of such aliments by plants, yet it is probable, that in those animals destined to subsist, in a greater or less degree, on saccharine aliments, such power of appropriation is more firmly established (in the liver? see preceding note) than the appropriation of albuminous and oleaginous principles. In short, that the power of appropriating the saccharine principles is the last that ceases to exist in an animal; and thus often remains, to a certain extent, long after the power of appropriating albumen and oil has ceased. The derangement or partial suspension of the power of converting the saccharine principle in man, not only constitutes a formidable species of dyspepsia, but the unassimilated saccharine matter in passing through the kidneys, gives occasion to the disease termed *diabetes*.

---

* "The azote may, in some instances, be derived from the air, or *generated*. But my belief is, that, under ordinary circumstances, much of the azote employed in the assimilation of saccharine matter is furnished by a highly azotised substance, secreted from the blood chiefly into the duodenum; and that the portion of the blood thus deprived of azote is separated from the general mass of blood, either by the stomach, in the form of lactic acid, or by the liver, as one of the non-azotised constituents of the bile; and that the lactic acid and non-azotised substance thus separated are ordinarily excrementitious. The above circumstances, mentioned in the former edition of this work, I still think probable; and long and repeated attention to the functions of the liver, both in health and in disease, have satisfied me that this organ, in its assimilating functions, is analogous to, or identical with, the assimilative functions of vegetables; that the liver represents, in short, the original vegetative system, on which, in animals, the animal system is, as it were, superimposed."

"The reader will have remarked, that we have frequently used the term *ordinary circumstances*; and perhaps it may not be amiss to explain the meaning attached to that term.

"When an animal is duly fed, according to that diet which is natural to it, and for which its organization has been adapted, a regular and *ordinary* series of changes takes place within the animal, and the alimentary matters are converted into chyle. But one general characteristic of organized beings is, that within certain limits, and for a certain time, animals possess the power of varying their habits, and of accommodating themselves to circumstances. Under *extraordinary* circumstances, therefore, extraordinary changes must and do take place. In some instances, these changes out of the ordinary course are to an extent altogether astonishing, and such as defy our utmost calculation. The assimilating organs appear even to decompose principles which are still considered as elementary, and thus to *convert* them into azote or carbon; so that it is impossible to define what, on an emergency, these organs are capable of doing. But what is thus done on an emergency, will usually be found to constitute an exception to what is done in ordinary; their ordinary mode of action being always that which is *most simple*." (pp. 474 et seq.)

We have extracted the foregoing section at length, in fairness to Dr. Prout, and because it constitutes the pivot upon which nearly all his peculiar physiological, and consequently pathological, doctrines turn. We have already shown that we respect highly Dr. Prout's originality and talents; but there must be a limit to subservience even to the highest authority; and no one can be warranted in receiving that on credit which requires proof. We do not deny that if Dr. Prout's theory be admitted as the expression of truth, many things, now very difficult to comprehend, may receive a plausible explanation. How indeed can there be difficulty in accounting for any phenomenon, no matter how much at variance with existing experience, if we are permitted to say, such an element has been *generated* in this case, or such another element has been produced by *conversion* from something else? If this mode of reasoning be admitted, there need be no limit to speculation; and, unfortunately, speculation will then become useless, for you could not then be sure that the same condition would be followed by the same effects. But have we really any credible title to assume that, at this time of day, matter is ever created or converted? Suppose the possibility of such things, and chaos must come again. What! that animals should possess the *extraordinary* power of increasing the quantity of matter on our globe! Then if gravity be true, we must approach nearer to the sun. And if one kind of matter can by their agency be converted into another, then may it result that all the phosphorus in the world will, by a series of *extraordinary* changes, be converted into nitrogen, and the bones must look out for new conversions, mayhap, of potassium or sodium, to furnish the elements for their necessary earth. All organized nature becomes in this manner a kind of dissolving view, in which everything is misty and confused, and from which observation can learn nothing definite or real. We cannot admit the possibility of these *generations* or *conversions*, even on the authority of Dr. Prout.

But these, according to our author, are only occasional and *extraordinary* phenomena. Under ordinary circumstances, the converting powers of the stomach are essentially of three kinds: viz. the conversion of saccharine aliments into albuminous and oleaginous principles; the conversion of albuminous principles into oleaginous principles; and the conversion of oleaginous into albuminous principles. Now we do not pretend to say that the stomach is not possessed of these functions; it is not our duty to dogmatise, especially as to a negative; but we may be permitted to say that we doubt if Dr. Prout can bring forward a single proof of the existence of such powers. More albumen is found in the contents of the duodenum during digestion, than in the stomach, but Berzelius has shown that this depends on admixture with the albuminous pancreatic juice. Tiedemann and Gmelin fed dogs upon sugar and albumen, and the contents of the lacteals were found to resemble those of the lymphatics, being destitute of the milky appearance which they would have presented had oil become formed by conversion. The sugar in this case was found in the contents of the lacteals, so that it had not changed into albumen. We well know what becomes of the oil of the food; that with the alkaline secretion, poured into the duodenum, it forms an emulsion, which is subsequently in great part absorbed by the lacteals, and is the cause of the milky appearance of their contents during digestion. Bouchardat and Sandras have lately studied the transformations of starch and sugar in the alimentary canal, and their results closely correspond with those of Tiedemann and Gmelin already referred to; they found these substances transformed into dextrine, glucose, and lactic acid, but they never found them to be changed into oil or albumen; these substances were in part absorbed by the lacteals, although chiefly by the veins, and when introduced in too great quantities, were eliminated from the kidneys, otherwise they disappeared completely within the organism. All observations, all experiments, go to prove that the stomach has no such functions as those ascribed to it by Dr. Prout.

But perhaps we misunderstand our author; it is possible that he does not mean to ascribe these acts to the stomach itself; he may employ the name of this viscus in a very extensive sense. In another part of the section we have extracted, he speaks of the liver as the organ that especially appropriates saccharine aliments, considering that the liver has functions analogous to, or identical with, the assimilative functions of vegetables. If this be his view, we are to ask, what proof have we that the liver can and does convert sugar into albumen? and we confess we cannot recollect any single observation or experiment which will afford us an answer. But we may learn something from analogy; and Dr. Prout points out to us where to look for it. "The liver represents," says he, "the original vegetative system, on which, in animals, the animal system is, as it were, superimposed." So then we have a right to examine how albuminous substances are formed in vegetables; for the liver, according to him, "in its assimilative functions, is analogous to, or identical with, the assimilative functions in vegetables." Now modern research has shown us, that the formation of albuminous matters out of saccharine is a change that is not performed by any organ at all. It is a production that precedes organization. The cytoblastema of Schleiden, or the protoblastema of Mohl, contains proteine before it becomes organized into cells. The fluid contents of the youngest cells are loaded with the greatest quantity of nitrogenous matter, according to Payen and Mirbel; and Hartig and Mulder have demonstrated that, according as the cells become older, the proteine matter becomes absorbed into the cell-wall, abandoning the liquid in its cavity. The formation of albuminous matter is a necessary preliminary to all vegetation. This is a law of the vegetable kingdom; is it so of the animal? That sugar absorbed into the blood becomes ultimately thrown off from the system as carbonic acid and water, we know. Have we any evidence that, as an intermediate step, it combines with other elements to form albuminous compounds? We think not. In the vegetable kingdom, numerous and various molecules combine to produce complicated associations; in the animal, these polymolecular compounds become decomposed into the binary substances of the mineral kingdom. Addition and multiplication rule in one department of organic nature; subtraction and division in the other. The plant takes water, carbonic acid, and ammonia, and combines them into sugar, starch, gum, proteine, &c.; the animal swallows these latter compounds, and throws them back into the external world in their original forms. This is the sum of our knowledge; we are not acquainted with a single fact that will authorise us to suppose that animals possess any of the assimilating functions of vegetables; and we cannot be permitted to assume such an hypothesis, to smooth a difficulty, or facilitate an explanation. We have seen that in vegetables the production of albuminous matter is the result of forces, which prepare the material that is subsequently to become organized. If animals be possessed of similar forces, why do they not live on the same food as vegetables? Dr. Prout says that saccharine appropriation may be looked on "as constituting the immediate act, as it were, by which the stamina or groundwork of organized beings is formed." But he knows, surely, that sugar must be formed before it can be appropriated, and that the original material of vegetable cells is identical with it. He will not deny that there is every reason to believe that sugar is formed in plants out of water and carbonic acid. Will he maintain that the reaction by which this substance is produced can take place in the liver of animals?* The production of sugar and albumen in vegetables appears

* Dr. Prout actually does admit the possibility, even contemplates the probability, of this reaction. In page 499, he says, "The association of water, first with carbon, and afterwards with azote, has been maintained to be peculiar to plants. It is undoubtedly *the* peculiar function of plants; but it is not limited to them. In all animals there is a vegetative organ (if we may be allowed the expression), capable in a greater or less degree of performing the same functions as vegetables, i. e. of combining water with carbon; or, if not beginning at this low point of the scale, at least of combining the organized saccharine principle with azote, &c., so as to form albuminous products. This vegetative organ is the *liver*." We need not say that all the foregoing assumptions are entirely gratuitous.

to be either synchronous, or immediately consecutive. If the liver be the seat of vegetative forces, why is it that an animal will starve, although supplied with an abundance of solution of carbonate of ammonia, and of the salts of the organism. But the whole hypothesis is utterly untenable. With every respect for Dr. Prout, we feel that in arguing against it, we are combating a phantom.

We have said that Dr. Prout differs from other physiologists of the chemical school, in regarding the process of blood-making in animals as a very complicated and gradual one. In the passage already quoted, he says,—"Though the proportions of the different ingredients of the chyle, as ultimately formed, are liable to be varied according to the nature of the food, yet, whatever the nature of the food may be, the general composition and characters of the chyle remain always the same. The stomach therefore must be endowed with a power or faculty, the agency of which is to secure this uniform composition of the chyle by appropriate action on such materials as circumstances may bring within its reach." The blood he supposes to be formed out of the chyle and lymph, and to undergo in the lacteals, lungs, &c., certain changes, by which its constituents are rendered more and more perfect, until at length it becomes fitted for the nutrition of the organs. It follows, from this mode of viewing the subject, that the albumen of the chyle is *imperfect*, that of arterial blood *perfect;* and that the globules of the chyle and lymph are the incipient representatives of the red globules of the blood. So satisfied is Dr. Prout of the truth of all these gradations, that he considers them to be "a simple expression of facts." From what we have already written, it will be perceived that we are not equally sure of their accuracy. The proposition which forms the basis of the edifice is contrary to ascertained facts. It is not true, that "whatever the nature of the food may be, the general composition and character of the chyle remain the same." If an animal be fed on sugar or fibrine, the chyle will be clear, in consequence of the deficiency of oily particles; and if fed on butter, neither will the chyle be milky, if the ductus choledochus communis has been previously tied. This latter fact, first ascertained by Sir Benjamin Brodie, was supposed by him to prove that bile was indispensable to the production of chyle; so it is, but only by its alkaline properties, by which it converts the oil of the food into an emulsion; for, in truth, what is called chyle, is nothing more than an oily emulsion, which being absorbed into the albuminous lymph of the lacteals, each globule becomes coated according to the law discovered by Ascherson. And then as to the albumen of the chyle and lymph being an imperfect albumen, we have no proof whatever of such being the case. The albumen of the chyle differs somewhat in properties from the albumen of the blood, just as the latter substance differs from the white of an egg; but what right have we to assume that one variety is more perfect than another?

With respect to the red blood-globules; although many microscopists have maintained their identity with the chyle-corpuscles, we do not think that this is by any means satisfactorily made out. We shall not, however, dwell further on Dr. Prout's physiological views, as we wish to devote some space to the consideration of the pathological doctrines, of which they form the basis.

The controversy which has so long been waged between the supporters of the local nature of disease and the advocates of its essential or general origin, has not even yet subsided into the definition of any exact or universally admitted boundaries. Even the Broussaists admitted that the same causes locally applied did not always produce the same effects, and they accounted for this difference by the variety of temperaments. Thus, inflammation in one person gives rise, according to their hypothesis, to suppuration, in another to tubercle, and in a third to gangrene ; and they explained these differences, in part at least, by the sanguineous, lymphatic, or melancholic temperament of the individual. But a person of decidedly sanguineous temperament may die of phthisis pulmonalis; while normal suppuration may take place in a temperament the most lymphatic. The observation of such facts as these, led to the doctrine of predispositions; by which, from a modification of the nutritive process produced by certain alimentary or physical conditions, a tendency to particular diseases was produced. Still there was something too vague and uncertain in these explanations, to satisfy the scrutinising and progressive spirit of the age. The advocates for local diseases met with cases where dissimilar symptoms had accompanied the progress of cases, in which the autopsies displayed precisely analogous sensible appearances; while, on the other hand, similar symptoms were found to correspond with apparently dissimilar alterations of structure. Under such circumstances, the morbid anatomists saw reasons for distrusting their unassisted senses, and therefore called in the microscope to their aid. Immense benefits have been conferred on science by the employment of this instrument; we now know much more of nutrition than would be possible without its assistance; and the phenomena of inflammation, and other forms of disordered nutrition, have acquired in our minds a precision which our fathers could scarcely have hoped for. Yet the microscope can generally only tell us effects; and even these effects require to have proceeded to a certain amount of maturity, and be in a particular state, before they can be rendered available for the purposes of observation. The microscope cannot reveal to us disordered function, nor show us whether the functions so disordered were those of a single organ, or of the entire system. Those pathologists who inclined to the essential or general origin of disease, sought in the resources of chemistry a clue to the real nature of predisposition; the various liquids and solids of the organism, both in health and in disease, were subjected to analysis, and the relations between the food and inspired air, on the one hand, and the various excretions, on the other, were more or less carefully examined. But we do not think that this method, whatever other advantages have proceeded from it, has yet thrown much light upon the subject. We respect the labours of Andral and Gavarret, Lecanu, Becquerel and Rodier, Simon, and the others who have ardently and skilfully worked this mine; as we equally admire the exertions of Valentin, Lebert, Vogel, and the many who have disclosed to us the minutiæ of diseased organization; but we feel that, notwithstanding all that has been done, we are not yet in a condition to subscribe to any formula which might presume to prescribe how much of a given disease consists in perverted local function, and how much is to be attributed to the general mal-performance of either primary or secondary assimilation.

The very title of Dr. Prout's book shows which side he has espoused; the object of it is to point out the connexions between diabetes, calculus, and other affections of the kidney and bladder, with indigestion. The appearances which the kidneys or bladder may present after death are to him matters of minor importance. These appearances he regards as consequences of the same morbid action which produced modifications of the urinary secretion. This morbid action he looks on as taking place in remote organs, or rather generally throughout the system; and the modifications of the urinary secretion (for example) are, in his opinion, valuable, not so much as proving this or that lesion of the urinary organs, but as indices of the general condition of the nutritive functions.

> "The processes by which alimentary matters are assimilated in animal bodies are constantly liable to be deranged, both in degree and in kind; such derangements of the assimilating processes are manifested by corresponding derangements in the various products secreted from animal bodies by the organs designed for such purposes, and particularly by the kidneys and liver; and the relations between the deranged products of secretion, and the proximate principles by which animal existence is maintained, and of which animal bodies consist, enable us in many instances to acquire a more correct knowledge of the organ deranged, as well as of the nature of the derangements, than can be attained by any other means." (p. 3.)

The foregoing being the principles which guide Dr. Prout in the study of what are called urinary diseases, he is led in every case, when he meets with a derangement of the urinary secretion, or a modification of the urine not usual in health, to seek for some derangement of the assimilative functions as its cause.

A persistent perversion of the assimilative functions he names a diathesis; and thus if sugar be found in the urine, he conceives there must exist a saccharine diathesis, or such a derangement of the assimilative functions as will permit sugar to pass from the stomach, through the system, to the kidneys, without undergoing those alterations to which, in a normal state of things, it is subject. When uric acid exists in excess in the urine, he attributes it to a lithic acid diathesis, or a disorder of the functions of secondary assimilation, by which an unusual quantity of the albuminous tissues becomes disintegrated, and the uric acid eliminated is caused to abound. In like manner, he describes a phosphatic diathesis, an oxalic-acid diathesis, &c.

We cannot suppose that Dr. Prout believes that an alteration of the urinary secretion invariably proceeds from a perversion of the assimilative functions. He must allow some instances, certainly, in which causes purely local produce modifications in the appearance and properties of this liquid. We are sure he would not be led by adhesion to theory so far as to assert, that pus in the urine is the result of a diathesis; or that the presence of blood in this secretion is an evidence of anything essentially wrong in the reducing, converting, or vitalising functions of the stomach, or of any other distant organ. He must know that in gonorrhœa, contusions of the kidney, and many other cases, pus, albumen, or blood, may and do proceed from causes strictly local.

Dr. Prout sets down to a diathesis the occurrence of what he calls "chylous urine." This disease, scarcely known in these latitudes, is common in the Isle of France, in the Brazils, and other tropical climates. It is characterised by the occurrence of large quantities of albumen, and frequently of fibrine, in the urine, together with oil that gives this liquid a milky appearance, and often, from the preponderance of albuminous matters, the liquid becomes like blancmange in cooling; at the same time the serum of the blood has been found to be lactescent, from the presence of an unusual proportion of oil. The latter circumstance is certainly a strong argument in favour of considering the cause of the peculiar condition of the urine to be a general one, affecting the assimilating functions, rather than a local one confined to the secreting organs: but when we inquire as to the origin of the disease, we find it commence with a simple hæmaturia endemic in those countries, which commences at an early period of life, and usually ceases at puberty; but occasionally, instead of ceasing, it merely changes its sensible characters, the colouring matter of the blood no longer escaping with the urine, while the liquor sanguinis continues in this secretion in variable quantities in different cases; by and by, oil becomes superadded to both the urine and the serum of the blood. As we trace this history, the argument for the essential origin of chylous urine loses its force; and we find the subject to become again enveloped in obscurity.

The foregoing disease may continue for a great length of time, with very little injury to the general health. Not so with another form of albuminous urine, in which also we generally perceive a slight opalescence from admixture with oil, and in which it is likewise not uncommon to find the serum milky. This disease certainly differs from the former in some other remarkable characteristics; the normal constituents of the urine become remarkably diminished, according as it progresses; and the functions of the skin are very much interfered with. Is this disease also the result of a diathesis, or is it the result of a local lesion? Does it proceed from defective assimilation; or is it caused by an altered nutrition of the kidneys? Dr. Prout can derive very little assistance in this case from the scalpel, or the microscope. He quotes the researches of Bright, Christison, and Rayer, of Simon and Johnson; but all that he learns from them is, that "the preceding description of the *apparent post-mortem* phenomena found in connexion with a serous condition of the urine, as well as the arrangements produced from them, are, in a *practical* point of view, of very little importance." (p. 175.) However, he does not hesitate to divide these cases into two groups: the first connected with a condition of hæmotrophy; the second with one of anæmotrophy. In the first class of cases, a rheumatic or gouty diathesis exists; in the second, a strumous diathesis. Dr. Prout likewise admits a local exciting cause determining the diathesis to the particular organ. That the reader may be enabled to estimate the importance of the relations of these diatheses to the various forms of what has been called Bright's disease, we shall extract Dr. Prout's own explanation of the particular kinds of mal-assimilation in question.

> "With regard to the general pathological relations of lithic acid and its compounds, we may remark, that as the lactic acid developed principally during the secondary mal-assimilation of the gelatinous textures was considered as the characteristic feature in rheumatism; so the lithic acid developed principally during the mal-assimilation of the albuminous textures, may be considered as the characteristic feature in gout. Moreover, when the lactic and lithic acids are developed together, as they may be, and often are, the phenomena may be supposed to show that the mal-assimilation involves both the gelatinous and albuminous textures; and that the accompanying disease partakes of a mixed character; or in fact constitutes what is not improperly termed *rheumatic gout*—a form of disease, which every one

knows, is usually of a much more deep-seated and obstinate character than either gout or rheumatism alone. According, therefore, to these views, the lactic and lithic acids, considered with reference to rheumatism and gout, may be regarded somewhat in the light of *materies morborum;* or, strictly speaking, the undue presence of these acids in the urine or elsewhere, under certain circumstances, may be viewed as indices of the existence of certain diseased actions going on in the primary tissues of the body; and which are known by the names of rheumatism and gout." (p. 209.)

The more we reflect on the views contained in the foregoing extract, the more we are inclined to wonder how they could ever have obtained admission into Dr. Prout's ingenious and highly-cultivated mind. What reason is there to suppose that the waste of the bones, tendons, ligaments, &c., is greater than that of the muscles? yet if the urea proceeds from the former, and the uric acid from the latter, the difference of their disintegration must be as 30 to 1. Amongst the mammalia, we usually find but very little uric acid excreted; and the restless feline tribe, who spend the entire night in prowling for their prey, and in whom the muscles are so powerfully developed, do not secrete a tithe of the uric acid eliminated by man. On the other hand, the ovipara secrete very little urea; superurate of ammonia being the chief constituent of their urine. Even admitting the vast muscular development of birds, still, the proportion of gelatinous waste ought to yield a very appreciable quantity of urea in their urine, if the hypothesis were well founded; but Coindet could not detect a trace of urea in the urine of the eagle. Again, the hypothesis supposes that there exists a direct relation between the production of urea and lactic acid, as they are both supposed to proceed from waste of the gelatinous tissues; now we are not aware of there being any evidence in support of the existence of such a relation. Lastly, we are required to believe that it is excessive waste of the gelatinous tissues which constitutes rheumatism; of the albuminous, which constitutes gout; or if this be not the meaning of the passage quoted, we acknowledge it to be too profound for our comprehension. If it be Dr. Prout's meaning, we must absolutely dissent from it, and for a very simple reason, namely, because we never heard of a lion having the rheumatism, or an ostrich the gout; although the former, it must be admitted, is liable to an excess of urea in his urine, and the latter decidedly labours under a lithic-acid diathesis.*

Let us now examine in what it is that Dr. Prout considers the strumous diathesis to consist.

"Struma, in its established form, may be said to consist in a depraved condition of the whole animal economy. In the manufactory of the constituents of the frame, the enfeebled vital energies fall short, as it were, of their purpose, and produce results exceedingly prone to throw off vital allegiance altogether. Hence in the abnormal deposits and growths we term *scrofulous*, material properties are ever ready to triumph over the vital, and emancipate the incarcerated elements from the trammels of organization..................Though all the assimilating processes, like their products, are more or less affected in strumous habits, the primary processes are chiefly in fault, and particularly those we have termed the *completing* processes—those processes taking place between the duodenum and circulating system, and by which the chyle is principally converted into blood." (p. 240.)

---

* It is not meant to assert, that gout and rheumatism never attack the animals mentioned in the text. On the contrary, there has been a case lately in the periodicals, in which a number of domestic fowls, lodged in a damp, cold hen-house, suffered from gouty deposits in their feet. We believe, also, that rheumatism, or at least arthritic rheumatism, is not very uncommon among *lions* in our menageries, although probably rare enough in their natural condition. But these exceptions prove the rule; for, if Dr. Prout's views were correct, a condition of continual gout ought to be normal amongst birds; and one of continual rheumatism among the carnivora. They would in that case present conformity to the law advocated by Serres, that disease in man is always the repetition of the normal condition in some of the lower animals.—ED.

Before we permit Dr. Prout to proceed any further in the elucidation of his views of the nature of struma, we must again protest against this system of assumption. In the passage we quoted, he assumes that the blood of scrofulous individuals is different from the blood of the healthy. We do not say that it is not so; but, before we admit it, we require some proof. It is possible that the albumen, fibrine, colouring matter, &c., of strumous blood may differ in some respects from their normal composition or properties; but this difference is the very thing that requires to be ascertained, which it never has distinctly been. On the contrary, chemists of eminence have asserted that scrofulous growths have the same composition as the organs in which they are deposited; and, at all events, until the facts are determined, no one has a right to assume them for the convenience of an hypothesis.

"Now, from the constant admixture of a deteriorated pabulum, there can be no difficulty in conceiving that, in time, the whole mass of the blood must itself become deteriorated; and that everything formed by subsequent processes from such deteriorated blood must be likewise more or less imperfect." (p. 241.)

And in this manner Dr. Prout plausibly explains the production of tubercle,

"Unhealthy tumours, cancer, &c., in all their varieties; the most inveterate skin diseases; the frightful class of malignant affections, which, without possessing any tendency to form diseased masses, gradually corrode and eat away the living body; also fatty tumours of various kinds, and even obesity itself—diseases, many of which, as already pointed out in this volume, are indicated by the condition of the urine and its deposits, the oxalate of lime, the cystic oxide, &c.; and even sugar, in certain diabetic complications." (p. 246.)

It cannot be denied that this hypothesis is very comprehensive; pity it is, that, in the present state of science, it has no real foundation.

But struma is, it appears, nearly related to gout:

"Gout and struma we hold to be both affections, chiefly involving the albuminous elements of the body. In both diseases the albuminous principles, though to a certain extent organised, are not raised to the healthy standard, so as to be fitted for appropriation by the living body. In some instances, also, particularly in struma, the imperfect albuminous principles perhaps possess a low independent vitality. Instead, therefore, of becoming component parts of the living body, they either form separate growths, or are deposited as extraneous bodies; and thus, by interfering with organic operations, give occasion to the diseases we call struma and gout. So far, in general terms, struma and gout may be supposed to resemble each other in their character and origin; but gout may be considered to differ from struma in the following respects.

"We have elsewhere observed, that living organized matters are rarely separated from a healthy body as excrements; that a distinct function exists, which we have called the *disorganizing function;* and that this function operates by denuding the matters to be excreted of their vitality, which is retained; and separating the excrementitious matters in the form of common chemical compounds. Now, in gout, this function is more or less perfect; the albuminous matters being separated in the form of lithic acid, or of some of its compounds; while, in struma, it is much impaired, or altogether ceases to be efficient; and, instead of disorganized suffered previously from any affection of the hearing, but, on the contrary, possessed an exceedingly fine ear, so that from youth upward he has been able instantly to distinguish, whether in vocal or instrumental music, the slightest deviation from the correct tone. In 1824 he became the subject of a nasal polypus on the right side, which was successfully removed by operation. From 1828 to 1834, in consequence of great exertion of the sight, he suffered from amblyopia of a rheumatic character. In 1834 he became for a short time slightly deaf, and had in both ears a feeling of stoppage, which lasted for hours, and from which he could obtain some relief only by frequent pulling at the external ear. This complaint, however, entirely vanished after the extraction of a quantity of hardened wax from both ears, and the employment of sea-bathing. The first trace of the morbid acuteness of hearing he perceived, in 1835, in the right ear. At that time he resided in London, giving instruction in music, and was occupied the whole day, either in giving lessons, attended with great exertion, or in perambulating the streets to go from pupil to pupil. His complaint manifested itself first in the following manner: every foreign sound that occurred while he accompanied the voice upon his guitar, as the jingling of cups and glasses, the striking of bricklayers' trowels upon the bricks, the rumbling of a carriage, disturbed him so greatly that he could not continue his instruction. At a later period, the scream of a parrot, the creaking of a door, the yelping of a dog that had been trodden on, and in general, as he asserted, all indefinite tones, gave him an unpleasant painful sensation, and a feeling of stoppage in the ears. Gradually these symptoms became so aggravated, that not only all indefinite tones caused him pain in the ears, but this was also the case with every false note produced in playing or singing. For this reason he ceased to give instruction in London, and returned to Vienna, where he has taken a very quiet house on the Boulevard, lives very moderately, and gives but few lessons. Since this, his irritability is considerably diminished, but by no means completely allayed. The rattling of a carriage in the street, and the other above-mentioned more violent noises, still cause unpleasant sensations in the ears. Still every false musical note, and the tuning of instruments previously to the commencement of a concert, distract him. Moreover, he is extremely annoyed by shrill screaming voices, especially those of women, the notes of a flageolet, and so forth. He can, on the other hand, tune his own guitar, and bear powerful voices, and even the noise of cannons, very well. His ear, ten years after the commencement of his malady, is still very fine, and he derives from a perfectly accurate performance upon an instrument, or the correct execution of an air by the human voice, the highest enjoyment, so that he could often weep for pleasure. He has never had noises in the ear. The weather has no influence on his hearing. He can hear my cylinder watch on both sides at a distance of five ells (German), while others, on an average, can only hear it at the distance of one ell. He also understands words spoken in the lowest tone very easily. Meatus natural; wax somewhat dry. Throat somewhat irritated. For seven years, in consequence of much speaking while giving lessons, he has had an irritation of the throat, which even now renders him very sensitive to a draught of air. On closing his mouth and nostrils, and attempting to expire, he feels the air strike very sensibly on both ears, so that the eustachian tubes are unmistakeably open. He is very subject to determination of blood to the head, especially after exposure to great heat and exertion in playing. He also suffers from a very exalted irritability of sight, and cannot bear the light of a lamp, especially when provided with a glass chimney; while he is not inconvenienced by the light of a taper. He is often constipated, and sometimes suffers from hemorrhoids. I gave him the following advice: to avoid, as far as possible, going out in the wind and bad weather; but when this is not practicable, to protect himself from the influence of the weather, and while he remains in the street, to put in his ears charpie, dipped in oil, in order to moderate the sounds which distress him. From time to time to pour some mild almond oil into his ears. To take care to keep the bowels always open. During the milder seasons of the year, to live in the country, and take malt-baths. If possible, to use next summer, for six weeks, the baths of Ems, or Salzbrunnen, or at least Gleichenberg. Having complied with most of these counsels, the patient greatly improved in the course of a few months."

Of the second nervous affection (parecho) Dr. Schmalze gives four instances. He has only met with it six times. In the first case, the patient, æt. 36, previously in perfect health, had taken a sleigh-drive, and immediately after violently cracking the whip, perceived a loud rushing noise in the ears. This increased in intensity during four weeks, at the expiration of which time he presented himself for treatment. The sound in his ears was sometimes accompanied with confusion of the head, and frequently caused in speaking a peculiar echo, which disturbed him exceedingly. It was variable, and appeared somewhat dependent upon the state of the weather. In other respects he was quite well. Laxatives were prescribed, and in three days the symptoms disappeared.—In the second case, the widow of an officer, æt. 44, suffered from otitis externa, and subsequently from otorrhœa of the left ear. She was completely deaf in the left ear, was not troubled with tinnitus, but experienced in speaking a peculiar echo in the left ear. Astringent injections were employed, and in 14 days the symptoms disappeared, and the hearing of the left side was completely restored.

Of false hearing (paracusis), five cases are related. We select the first as an example:

"The patient, a musician, æt. 40, has suffered for two months from a whistling noise in the right ear, and particularly from a peculiar falseness of hearing. This manifests itself in a change of all the tones of his instrument, so that he has become unable to play on it. Moreover, he cannot listen to harpsichord playing, and all music sounds unpleasantly to him. He is especially annoyed by deep tones. He ascribes his complaint to overstraining the organ of hearing in his exercises, and especially to his habit of giving instruction in music, during which he always accompanies his pupils, who very often play falsely, upon the violin. On both sides he hears the cylinder watch at the distance of one ell. He understands a single person speaking very well, but when several speak together, he is annoyed by it, but less so than by music. A waggon rumbling near him upon a paved road affects him so strongly, that he is in such cases often afraid of falling. Condition of ear and throat natural. He has formerly suffered much from inflammation of the throat, with elongated uvula, rendering an operation necessary, and has frequently had catarrh, from which he still suffers occasionally. Headache little. On the other hand, frequent giddiness, in consequence of the tinnitus. Some determination of blood to the head. A few years ago he recovered from an inflammation of the ear. Some weeks ago, having violently heated himself in dancing, he completely lost the tinnitus, but after 2 days it returned. In other respects healthy. He was forbidden to give instruction, and especially to accompany on his instrument, and was advised not to visit concerts. Flor. sulph., crem. tart., and kerm. min. After some days the tinnitus, and the sensitiveness of the hearing diminished. On the other hand, a catarrh, with pricking pain in the bottom of the neck, supervened. Gargles with cold water. Russian vapour-baths; continue the medicines. After some weeks only a certain irritability remained, which gradually disappeared, so that five weeks after every trace of his malady had vanished."

Six cases illustrate the subject of diplacusis, or double hearing. Of these the following affords a fair specimen:

"Professor Dr. F., in H., æt. 60, enjoyed good hearing in his youth, yet he remembers that occasionally the warbling of a canary sounded unpleasantly to him, and that, while at school, he had from time to time a feeling as though something were fluttering in his ears. Thirty years ago he first remarked, while employed in instruction, that his right ear heard a note deeper than his left ear, which circumstance he believed to originate in a cold. Since then he experienced a singing on both sides, and heard worse on the left side than on the right, which imperfection has since increased, and passed over to the right ear. He can hear the cylinder watch with neither ear. He understands with difficulty words spoken, but hears much better as soon as he holds his hollow hands behind the ears. His difficulty of hearing and tinnitus are variable, without depending upon the weather. They are increased by every excitement. Meatus on both sides free and dry. Throat sound. The impulse of air upon the membrana tympani is sensibly appreciated, when, on closing the mouth and nostrils, he attempts to make an expiration, as also after the air-douche. Colds frequent. Otherwise healthy. His complaint was considered to consist in an old affection of the auditory nerve, and therefore to be incurable. On account of the dryness of the meatus, he was recommended to rub in a little almond oil. To assist his hearing, I caused him to procure a pair of my hearing-trumpets, which he wears with advantage."

The annexed case is recorded as an inferior grade of otalgia, manifested merely by noises in the ear, and dependent upon an excited state of certain twigs of the fifth pair of nerves.

"Schomek, bookbinder, æt. 27, has suffered for 14 days from intermittent pains in the left side of the forehead, and has been 9 days in the hospital at Prag. He has already been afflicted ten times (the first time in the year 1837, in consequence of taking cold) with the malady about to be described, which has reappeared in different years, and in different places. The attack commences commonly between 7 and 8 o'clock in the morning, increases till 11 o'clock, when it reaches its greatest height, and then between 12 and 2 o'clock in the afternoon gradually abates. Every such affection has, on an average, endured 14 days. The paroxysm begins with stabbing pains in the left temporal region, an inch from the eyebrows, which afterwards extend to the middle of the forehead. When the paroxysm becomes more violent, there is toothache on the left side of the upper jaw, lachrymation from the left eye, intolerance of light, and mucous discharge from the left nostril. Only at the period of its greatest violence are noises in the ear present, which the patient describes as though something were running round the membrana tympani. This symptom arises suddenly, and vanishes as suddenly, after having lasted about 15 minutes, without leaving behind any affection of the hearing. Only in the greatest violence of the disease was any feverish excitement present; but the patient was always rendered somewhat stupid by the attack. Sometimes during the above-described attacks, his taste and smell have failed, but only on the left side. Hearing was good on both sides, and the patient has never had any affection of the ears. Meatus healthy. Cerumen present. In other respects Schomek was tolerably healthy. The disease was diagnosticated as neuralgia frontalis typica, and treated with quin. sulph., gr. ij pro dosi three times a day. By this remedy the complaint was nearly removed, and when I saw the patient there remained only a dull pain in the left side of the forehead, and stupefaction of the head, both of which symptoms, however, appeared only at the hours above indicated. Curiously enough, Dr. Oppolzer (the physician who treated the case) has just now observed in private practice several cases of periodic neuralgia frontalis (ten cases in 9 days), most of which affected the left side, so that he considers the above malady epidemic."

Let us now pass to the consideration of an instrument, which our author proposes to employ as a diagnostic means in certain diseases of the ear. We allude to the tuning-fork. Dr. Ernst Heinrich Weber had recorded the following experiment. He stopped up one ear with his little finger, and brought into contact with the hard portion of the head of the same side the handle of a tuning-fork, which had previously been set in vibration; he then brought the tuning-fork into contact with the head on the opposite side —on the side where the ear remained free. He observed that he heard much more distinctly on the side of the stopped-up ear, than on that of the ear which remained free. Reflecting upon this experiment, Dr. Schmalze was led to adopt the following propositions, which he afterwards confirmed by repeated experiment.

"When both ears are healthy and open, the vibrating tuning-fork is heard equally well on both sides. But when the meatus of one side is stopped up by the little finger or other object, the tuning-fork is always heard much more distinctly by the stopped-up ear, so that it appears to many persons that they hear only with that ear. When the meatus of one ear is morbidly closed, as by accumulated membranes and inspissated wax, the tuning-fork is heard by this ear, provided the integrity of the auditory nerve be uninjured, more distinctly than by the other. If the other ear be stopped up also, then the tuning-fork is heard equally on both sides. The same relation also obtains, when in a healthy condition of the nerve the eustachian tube or the tympanum of one ear is closed, as by effused blood, depositions of catarrhal or rheumatic matter, whether the meatus be closed or not. Certain cases that have come before me, in which, notwithstanding that the meatus, the eustachian tube, and the tympanum were free from obstruction, the tuning-fork was heard more strongly upon the diseased side, and in which I obtained a complete cure by the use of resolvent remedies, render it, in my opinion, not improbable that even single portions of the labyrinth, as for example, the vestibule, may become occluded by collections of lymph, by effused blood, &c., the nerve (at the commencement) not participating in the disease. On the other hand, when the auditory nerve of one ear is affected, the vibrating tuning-fork is always heard less distinctly by the diseased ear than by the sound ear; and the occlusion of the sound or unsound ear makes, in this respect, not the slightest difference. These observations I have endeavoured to utilise for practical purposes in the following manner: the tuning-fork appears to me to be particularly appropriate to the examination of the hearing, when only one ear suffers, or at least the one more than the other. Thus, when the patient hears the vibration of the tuning-fork through the diseased or more diseased ear much more strongly than through the sound or less diseased ear, we may conclude that obstruction of the meatus, tympanum, or labyrinth, or of several of these parts at once, is the cause of the disease. Although mere obstruction of the meatus may, as a rule, be easily confirmed by investigation with the speculum, yet this is not always possible, and in such cases the tuning-fork becomes a convenient auxiliary. But if in the cases specified we find the meatus free from obstruction, or the patient after the removal of the obstruction hear little or no better, and the results obtained by the tuning-fork remain unchanged, then we may conclude that the obstruction is seated beyond the membrana tympani, in the tympanum or labyrinth. Finally, if the patient hears the vibration of the tuning-fork less distinctly on the diseased or more diseased ear than on the other, be this latter obstructed or not, we may, with tolerable security, predicate disease of the auditory nerve itself." (pp. 101-4.)

"Concerning that peculiar inflammation and swelling which not unfrequently attacks the auricle of the insane," says Dr. Schmalze, "I shall make a few remarks, partly derived from observations made during my visits to several lunatic asylums, partly from communications on the subject kindly made to me by the superintendents and physicians of the same; since this form of disease is still altogether but little understood, and has been hitherto formally treated of by no other aural surgeon. Essentially it consists in a chronic atonic inflammation of the cartilage of the ear, and of the cellular tissue lying between the cartilage and the skin, with an effusion of blood or bloody serum. There arises generally slowly, but sometimes very rapidly, a congested condition of the cellular tissue of the auricle, with inflammation of the cartilage.

"The auricle is reddened (from a deep red colour it passes gradually into a bluish red), hot, tense, and elastic. All its hollows are gradually obliterated, and it assumes an appearance as if it were stopped with wool. At the height of the disease, instead of the depression in the auricle, a convex swelling, projecting like a longitudinally divided egg, is present, which sometimes even attains the size of half a Borsdorf apple, and is circularly embraced by the helix. The redness and swelling appear particularly on the anterior surface of the auricle, whilst its posterior wall is but little implicated. The ear is only at the commencement somewhat sensitive to the touch, and then more particularly at the circumference; but in other respects the patient suffers not the slightest inconvenience. Only at its greatest development is the meatus closed by the swelling; for the most part it remains free, and does not partake in the disease. Seldom or never does the patient complain of difficulty of hearing, or noises in the ear. It never gives rise to otorrhœa. If an incision be made in the swelling, albumen and bloody serum are evacuated. At a later period the cellular tissue of the auricle contracts and shrivels, giving rise to deformity and diminution of bulk. But at the same time, as a rule, the auricle is thickened as though it had been attacked by nodes. Desquamation of the cuticle never occurs. The thickening frequently remains a long time, sometimes even during the whole life of the individual. After the disease has passed away, the shrivelled and thickened ear may be severely squeezed without giving the patient pain. In those who have died at this period, the cellular tissue of the auricle has generally been found to have disappeared, but, on the other hand, the cartilage of the ear has been observed, as regards its convexity, too much bent and drawn together, but otherwise only slightly thickened or altered in other respects. The disease commonly lasts from four to eight weeks. Cases terminating earlier (in fourteen days), or continuing for a longer period (several months), are unfrequent. It attacks pretty equally both sexes. We may assume, that among 100 insane it occurs on an average once a year. The individuals attacked labour almost always under the higher grades of mania, melancholia, stupor, or idiocy. It appears to have no prognostic significance as respects the mental affection. Concerning the causes of this aural swelling, physicians attached to establishments for the insane are not agreed. Some, as Bird, believe that the malady in question results from that congestion of the head which so frequently occurs in mental disease, which congestion is also indicated by the heat in the ear so common in the insane. Under these conditions, the generally depressed vital powers give rise to an inflammation of an atonic character. Several, as Fleming, are convinced that it is always the consequence of some external injury to the ear, which most frequently the patient has himself inflicted; for these patients, tormented by violent and painful sensations in the head, very often beat their heads against some hard angular body. Accompanying also that swelling on the ear, other contusions on the head are commonly observed. But, on account of its cartilaginous structure, the external ear gives occasion to a peculiar inflammation. Others, as Riedel, maintain that the ear-swelling is merely the consequence of neglect, and, therefore, in proportion as a patient is ill taken care of, and lies upon a bad bed, this disease is of more or less frequent occurrence. Cleanliness and employment of the insane in the open air is, according to this view, the best means to ward off the complaint." (pp. 165-7.)

From all these views, regarded exclusively, Dr. Schmalze dissents. He is of opinion, however, that each of these alleged causes operates partially in the production of the disease; but that none of them, taken singly, is sufficient to account for the phenomena. He argues that, if a mere mechanical injury to the ear could give rise to this disease, we should frequently find it attacking young children, who are so frequently exposed to falls and other injuries. Yet it never appears in young children. Many adults also receive violent blows and other injuries on the head; yet we do not find that these blows or injuries lead to the peculiar inflammation in question. It is necessary, therefore, to assume some predisposing cause; and this he finds in that congestion of the head, which prevails among the insane. But neither will this congestion, of itself, produce the disease under consideration. It is only after violence inflicted, that the complaint makes its appearance. In accordance with this opinion, Dr. Schmalze recommends the following treatment. At the commencement, when the symptoms are of an inflammatory nature, cold-water bandages, or bandages dipped in lead solution, ought to be employed; but when the disease has become chronic, exciting remedies, as bandages dipped in warm infusion of arnica, with wine and the like, are useful. Under this treatment, the swelling may sometimes be resolved within from two to four weeks. At Sonnenstein, he saw a patient, in whom this treatment had been so successful, that he could scarcely perceive a trace of the disease remaining.

It will be perceived, that, in the preceding pages, we have not attempted a general review or analysis of this interesting work, but have rather satisfied ourselves with noticing its more salient points. In most cases we have preferred to allow the author to speak for himself in his own words, and thus avoid all suspicion of having mutilated or misinterpreted his opinions. We now proceed to take a more comprehensive glance at the whole treatise.

Commencing with a classification of the diseases incident to the ear, the author passes on, at page 16, to consider the causes, peculiarities, and treatment of tinnitus aurium. At page 27, he begins to discuss questions connected with hardness of hearing. At page 49, the causes of diseases of the ears are investigated at some length, the nine following pages being devoted to a disquisition on the inconveniences to which the partially and completely deaf are exposed. Among all this we find but little sufficiently novel to merit particular notice. The next chapter treats of dietetics or prophylaxis. It is a judicious *résumé* of the measures best calculated to preserve health, having but little especial reference to the ear. The third chapter teaches the proper manner of conducting an aural examination. We are glad to perceive that Dr. Schmalze coincides with most British surgeons as to the impropriety, as a general rule, of catheterization of the eustachian tube. It is an exceedingly hazardous proceeding. When we consider the very delicate structure of the internal ear and of the eustachian tube, which latter is frequently separated from the internal carotid only by the thinnest possible lamina of bone, we cannot fail to perceive the extreme danger of meddlesome interference, a danger which we are only warranted in incurring in a few very rare and exceptional cases. The author next discusses pretty fully prognosis and therapeutics; but his experience does not appear to have elicited anything new on these points. Having made a few remarks on hearing-trumpets, and other means devised for the assistance of the partially deaf, the author passes on to describe the various inflammations which attack the ear—inflammation of the auricle, of the meatus, of the membrana tympani, and of the middle and internal ear. Of simple uncomplicated myringitis (inflammation of the membrana tympani), the author has never seen but one case.

"Magistratsrath Urban, of Prag, æt. 49, above a year ago, became, in consequence of immoderate indulgence in Russian vapour-baths, somewhat deaf in both ears. From this he afterwards very nearly recovered. A year ago he took cold while travelling to Carlsbad, and soon after his arrival took the baths. Immediately afterwards, otorrhœa of the right ear and deafness appeared. This discharge continued violent for four weeks, with considerable inflammation of the ear; it then somewhat diminished, but without ceasing. On the right (left?) side he hears the cylinder-watch at twelve, on the left (right?) at two inches, but, after injection, at ten inches. He understands spoken words very well. On the right side, the membrana tympani was coated with thin pus, and, after the pus had been removed, appeared violently inflamed. The meatus was not inflamed, but duly provided with wax. He has now no pain. On the left side the ear is healthy. Throat irritated. Eustachian tubes open on both sides. Voice somewhat hoarse."

Dr. Schmalze next proceeds to give us an extended account of otorrhœa, its causes, its consequences, its complications. A brief notice of polypi follows.

The next section treats of mechanical obstructions, whether from foreign bodies, inspissated mucus, altered wax, or other objects. We then come to nervous diseases, those of irritation, and those of paralysis. Of the former variety we have given several instances at the commencement of this article. The work terminates with a description of the methods of performing the various operations of aural surgery, and an account of the instruments required. The following operations are enumerated: injection of the meatus—catheterization of the eustachian tube—extraction of foreign bodies—extirpation of polypi—puncture and trepanning of the membrana tympani—perforation of the mastoid process of the temporal bone—reopening of the narrowed and closed meatus—perforation of the lobule of the ear—plastic operations—and construction of an artificial ear. These operations, regulated by the general principles of surgery, admit of little variation. When performed judiciously, they will be performed in very nearly the same manner, whether London, Paris, or Dresden be the scene of operation; it is therefore no reproach to Dr. Schmalze, to assert that this portion of the work contains nothing with which English surgeons have not been long familiar.

We have now brought our task to a conclusion, and will only add, as a parting word, that we believe Dr. Schmalze to have succeeded in producing a very complete and practical compendium of Diseases of the Ear.

Art. V.

Clinical Observations on the Pathology and Treatment of Continued Fever, from Cases occurring in the Medical Practice of St. Bartholomew's Hospital. By Edward Latham Ormerod, m.b., Demonstrator of Morbid Anatomy at St. Bartholomew's Hospital.—*London*, 1848. 8vo, pp. 244.

I. The office which Dr. Ormerod holds at St. Bartholomew's Hospital affords him unusual opportunities for the observation and contemplation of disease. Possessing the most unlimited access to the medical wards of the hospital, and having the duty of examining into the morbid changes visible at death in those cases he had observed and contemplated during life, Dr. Ormerod is in possession of all that is objectively essential to correct and instructive clinical observation. For this reason these cases, and the running commentary which connects them, deserve a more than passing notice.

It is not, however, to be understood that the best opportunity for contemplation and observation is all that is necessary to the advancement of the pathology and treatment of any disease, and of fever in particular. The opportunities and the instruments of clinical research may abound, and yet the head and hand be wanting that should rightly use them. The cultivated intellect, the mind well furnished with stores of thought, and able to distinguish the points of an inquiry most requiring research, the practised tact and judgment—these are no unimportant requisites to productive observation. As regards the therapeutics of fever, it cannot be doubted that Dr. Ormerod's work is of a high order, and will afford to the inexperienced and hasty practitioner some useful lessons; but as regards the pathology, it leaves us nearly in the same position as we were in previously to its publication, or rather it adds to our doubts on some points, while it corroborates the general statements of its predecessors on others.

We do not wish these remarks to be understood as depreciating Dr. Ormerod's labours; the fact is, that these imperfect results are necessarily consequent on the method he has adopted; for as that method differs in no essential points from those plans of observation adopted by his predecessors, it is a necessary result that the general results of his labours should present a great similarity to those of previous inquirers. So long as observers follow the beaten track, and do not venture into new fields of research, bearing in their hand the compass of a well-digested and philosophically constructed theory, and freed from the trammels of mere conventionalism, for just so long shall we move in a circle, continually improving perhaps in the *minutiæ* of treatment, but making no advance into the untrodden regions of a more comprehensive pathology. We have been led to this train of thought by the following passage in the work before us:

> "As one or other complication may be absent in different cases—speaking now in relation to the present subject of investigation,—continued fever—we come naturally to think of fever itself, as something possibly unaccompanied by any or all of them, as a something acting throughout the body, not requiring for its continuance that that action should always produce organic changes, even though it be strong enough to destroy life, itself merely an influence, however powerful, a tendency to disease, or, by analogy of its effects, a morbid poison." (p. 13.)

In this, the first paragraph, we think we observe that want of a clear perception of what is to be observed, which itself arises from a want of a comprehensive contemplation of phenomena. We should be sorry to be thought hypercritical; but if Dr. Ormerod will read over the passage carefully, he will find that he has, by the time he has arrived at the close of the sentence, substituted cause for effect. By the term fever is generally understood a certain group of deviations from health, and not "a something acting throughout the body," or "merely an influence," or, "by analogy of its effects, a morbid poison." This something so acting—this morbid poison—is the cause or antecedent of the phenomena termed fever; without it there is no fever. Now we think if Dr. Ormerod had well considered his subject generally, he would hardly have fallen into this error of composition; nor would he have failed in a precise expression of his meaning.

Let us go on with the passage:

> "Chemistry, to which men in most ages have looked for a solution of the phenomena of fever, has supplied many a fanciful explanation on the hypothesis of a

fermentation of the blood or other causes, which explanations it will be time enough to adopt in detail, when chemistry in its own way has presented us with an antidote to this subtle agent, concerning which we know nothing, and of which nothing but the convenience of the expression justifies us in habitually assuming the existence. And, so long as we consider the term morbid poison only as a convenient mode of expression, it may safely be employed; but it is very unsafe to argue from any general laws of morbid poisons, as to any other disease than the one from which they were drawn, or to consider these laws as more than a very limited statement of facts of what has been, which must be observed many times before we have any grounds for inferring what will be." (p. 13.)

The doctrine of a *zymosis*, or fermentation of the blood in fever generally, has certainly prevailed under one form or another from a remote antiquity; but we are not aware that this doctrine has been applied in especial to continued fever, or rather to that form of continued fever ordinarily termed typhus, or typhoid. The doctrine has a more particular reference to exanthematous and paludal fevers, and as such is undoubtedly of importance. That there are subtle agents which, when introduced into the system, excite these various forms of fever, each that form peculiar to itself, and no other, we are certain. We know that the subtle agent inducing smallpox will induce that fever, and none other; we know exactly the series of phenomena it will induce when introduced into the system of a person susceptible of its action; and to call that subtle agent a "morbid poison," can only be considered "as a convenient mode of expression," in the same sense that we call a covering for the head, a hat.

It is to be gathered from the text, that by the term fever in the quoted paragraph Dr. Ormerod means fever in general; and how he can assert that of febrific poisons in general we know nothing, we cannot comprehend. Pyretology has of late years made immense progress, the laws of action of numerous morbid poisons have been elucidated, and their origin traced. We would refer Dr. Ormerod to Dr. Copland's article on Fever in his Dictionary, for evidence that we know much as to the subtle agents in question, and that there is much at least to justify us in assuming their existence.

It has, in truth, become too much the fashion to profess a determined scepticism as to the current doctrines of the day, however well founded; too much the fashion to begin, as it were, with a *tabula rasa*, and slighting the accumulated experience of ages, add to medical science and art an imperfect doctrine, an old half-developed doctrine, mayhap a false doctrine; or, under mere novelties of expression, to conceal established modes of treatment, and then hope to receive the applause due to the discoverer. We do not rank Dr. Ormerod with this class of authors; the scepticism he indulges is rather verbal than real, and expressed perhaps in homage to the prevailing *mode*. Dr. Ormerod, in fact, not only recognises the existence of a morbid poison in continued fever, but points out some of its modes of action, and its analogy to other morbid poisons, as that of smallpox. The scepticism, has, in short, no influence on the narrator's plan of observation and comment.

"This unseen influence working in the body, presents very striking analogies to the modes of operation of different poisons. Its modes of action are various; sometimes the bodily functions are at once overwhelmed, just as from some severe mechanical injury; at others, the body slowly submits to its influence, but under it the patient lies for many days between life and death—no organ affected,—nothing tangible to the removal of which the physician's efforts may be directed,—simply a tendency to death. And when death does come, as too often happens, there is nothing that the eye can detect to explain this disturbance of all the functions of the body; the blood is found dark, and mostly fluid, and this is all that is left for either morbid anatomy or chemistry to infer from as to the cause of death." (p. 14.)

Here we have a full illustration of our preceding remarks, and an illustration also of another error into which writers are apt to fall, namely, to magnify the difficulties of their subject by plausible generalities. So far from there being nothing tangible in the two forms described above, the whole series of phenomena show how desirable is the aid of the physician. The action of the poison on the blood, and through it on the nervous system of both vegetative and animal life, is an action calling for resistance, although no special organ manifest derangement more than the rest. Nor are the powers of the pathological anatomist so circumscribed and imperfect as would appear from the statement; for although the naked eye, the common scalpel, or the ordinary chemical test, may discover nothing, yet that morbid condition of the blood alluded to is an all-important fact in the pathological anatomy of fevers, and demands the eye of the microscopist to detect those changes in the form, colour, and other characteristics of the molecular constituents of the blood, which may fairly be expected to accompany so decided a morbid condition. We are sure Dr. Ormerod will agree with us in thinking that it is much better to avail ourselves of every new means afforded to us for overcoming the difficulties of pyretological anatomy, rather than to tread in the accustomed track, and lament over the unconquerable difficulties of the subject.

II. The best point of departure for an investigation of the pathology and therapeutics of fevers in general, and of continued fever, or typhus in particular, is the general fact (for we venture to call it a fact) that all fevers depend primarily upon a poisoning of the blood. We apprehend that it is hardly necessary to adduce any arguments to establish so much of this doctrine as refers to exanthematous and paludal fevers; as regards these at least, the doctrine is established.

Poisons generally may be divided into three classes, the chemical or inorganic, the vegetable, and the organic. The organic poisons arise from the decomposition of vegetable or animal remains, or consist of excretions, which, if retained in the system, act as poisons. The paludal poisons belong to the former class, and the morbific agent in gout and rheumatism to the latter. Another important class of organic poisons are those which, when introduced in almost inappreciable quantities into the system, seem to increase in quantity; and which, when communicated in the same inappreciable quantity from the individual poisoned to one who is healthy, excite the same series of febrile phenomena and local inflammation, and the same increase in the quantity of the poisonous agent.

This arrangement of the febrific poisons excludes two very common forms, one of which, namely, inflammatory fever, is characterised by an excess of fibrine in the blood, the other by the presence of pus, exciting the phenomena of hectic fever. The latter agent may be very fairly classed with the morbid poisons; and the former would appear to act in proportion to its quantity on the nervous and vascular systems, and to excite either chronic irritative fever (often confounded with hectic), or else the true phenomena of pyrexia, or symptomatic, or inflammatory fever. In this case, then, it is the inflammation of some structure (whether acute or chronic, it matters not) which is the cause of the poisoning of the blood.

Now there are certain phenomena which are common to all these febrific poisons, and which bring the results on the economy of their introduction into the system, under the general denomination of fever. But, although common to all, it is not to be understood that all are manifest in every form of febrific poisoning. Certain phenomena are taken as a basis, and, like letters of the alphabet, admit of an almost infinite variety of groupings, and an equally numerous variety of expressions. This multifariousness of expression depends less upon any difference of nature or action of the poison, than upon variations in the pathological affectability of families, groups, or classes of men. The paludal poison manifests itself by a series of phenomena, varying in each latitude, and for each degree of longitude. The destructive yellow fever arising from the paludal poison of the tropics, or the paludal fever of the southern portions of the temperate zone, have scarcely any perceptible affinity with the simple ague of northern latitudes. Yet well-founded deductions from a most extensive series of observations assure us, that the febrific agent is the same in all, and that the differences in its manifestations arise either from differences in the quality or quantity of the poison, or in the pathological affectability of the subjects of it; this latter varying, of course, *ad infinitum,* according to the race, climate, habits of living, state of special organs, and the numerous other circumstances by which large groups of mankind or individuals are affected. What in a temperate zone is a simple and transient congestion of the liver, becomes inflammation in a hotter region; the affection of the cerebrum, which in one individual amounts to headache, or at most to delirium, is, in another, coma or convulsions. Thus, from the same poison multitudinous groups of febrile phenomena arise, which appearing in individuals only are termed varieties, but which appearing in many individuals become genera, and are considered as distinct fevers; nevertheless, according to the views laid down, these are susceptible of such a sufficiently exact classification and arrangement, that their pathology and therapeutics shall be freely elucidated.

The second step, then, in the investigation of febrile phenomena, consists in a determination of the various morbid poisons, an analysis of their modes of action, and a classification of the phenomena to which they give rise, according to the varying conditions of the individuals submitted to their influence. With respect to the determination of the various poisons, we are in a position to point out the majority, although much obscurity hangs over the subject, inasmuch as little light has yet been thrown by pyretologists on the results of the action of two or more poisons on the system, when circulating at the same time in the blood. Perhaps the erroneous dogma, that two morbid processes cannot go on in the system at the same time, has led to this result; but, whatever the cause, the point is an important one in every respect. Thus, for example, we know not what morbid phenomena would result from the union of the paludal and typhoid poisons according to climate, race, civilization, &c.; neither have we any records of experience as to the influence of small doses of febrific poisons on the blood, as preparatory to the action of a large dose of another poison. For example, we cannot say what influence the presence of small or latent doses of the paludal or typhoid poisons has upon the progress and fatality of the poison of Asiatic cholera or variola.

As the existence of febrific poisons is only known by their effects, a knowledge of their modes of action is necessary to their classification. In determining these we would take the widest scope of investigation, and not only apply the laws of action of one poison to the determination of the laws of action of another, but turn to the laws of action of poisons in general, and draw from thence such aids as we can in the way of trustworthy analogy. For example, inasmuch as the action of these poisons is (as proved by experiment) through the blood, we may be thus confirmed in our fundamental proposition, that such also is the mode of action of febrific poisons. Or, again, since the action of poisons in general varies according to the quantity, we may apply this principle to our investigation of the action of the febrific agents. Of this we know something already, for we know that some are instantaneously fatal at one time, and at another their existence in the organism is hardly manifest. Still we have almost everything to learn under this head; we cannot even say what a large dose of any of the exanthematous poisons will effect, as of variola, rubeola, or scarlatina, for example. Observations have been made, and the suspicion expressed from time to time, that some of these cause death before their specific characteristics are developed, exciting rapidly fatal inflammation of the brain, heart, lungs, and other organs; but the proposition has never been expressed as a general law common to all febrific poisons, that a large dose acting upon an unpredisposed individual, or a small dose acting upon one highly predisposed, will develope phenomena widely different from the ordinary results of the febrific agent, and, in fact, *altogether dissimilar.*

In poisonings in general, we are able to distinguish between the immediate and the consecutive effects. This principle has been applied to fevers; but how imperfectly and impartially, is manifest on a slight consideration of the subject. We now know that chronic enlargement of the spleen is consequent on poisoning by paludal poison; but there were not wanting able inquirers who attributed ague to this disease, thus inverting the order of the phenomena. To what extent this is *now* done we cannot say; but it is to be suspected that, in some instances at least, modern pyretologists commit the same error. An example of this kind is Bretonneau's Dothinenterite; by this investigator the local disease resulting from the typhoid poison, namely, ulcerations of the intestines, is set forth as the cause of the febrile action. Broussais' doctrines are another example. A third example is that error of Bouillaud, who placed the seat of fever in the circulatory system, and although he includes the tubes themselves, their nerves, and their contents—the blood—in that system, yet he finally resolves fever into an *angio-cardite,* more or less intense.*

Another point in toxicology, which may aid us in the investigation of the pathology and therapeutics of fevers, is, that poisons have a specific determination to some organ in especial, as well as a general influence. And even of the class narcotic or acro-narcotic poisons, whose action is principally on the nervous system, we find a local determination to specific

* Traité Clinique et Expérimental des Fièvres dites essentielles. Paris, 1826.

portions of that system, and the action of each characterised by symptoms or pathological changes, directly referable to specific localities. This general principle largely applied to pyretology cannot fail to have excellent results, and may certainly be extended from the exanthemata proper, to influenza, cholera, dysentery, and typhus.

Another principle in toxicology, which has been very much overlooked in considering the action of febrific agents and its modifications, is the cumulative action of many poisons. We find no perceptible change induced, although dose after dose be given, until suddenly the system shows itself as much under the influence of the poison, as if a large dose had been taken at once. That this law is in operation in the case of febrific poisons is, we think, certain.

Another analogy to be considered between poisons in general, especially the cumulative and the febrific poisons, is, that an effort is made by the organism to expel the poison from the blood through some of the excreting organs. The principal emunctories seem to be the skin, lungs, intestinal mucous membranes and glands, and the salivary glands. The poison of mercury, for example, passes off by the salivary glands and the buccal mucous membrane, and induces also certain pathological changes, as inflammation and ulceration of these structures. We find arsenic acting in the same manner on the skin and mucous membranes generally; belladonna, like the poison of scarlatina, excites a scarlet inflammatory condition of the surface, in passing off; certain poisonous articles of diet induce another form of cutaneous inflammation, namely, urticaria. In these and other points, febrific poisons present striking and instructive analogies.

We do not know whether it be necessary to defend our proposed method of investigating the pathology and therapeutics of fevers, by arguments *ad hoc;* undoubtedly, the physiological pathologist will agree with us, that the method is that which a natural system of pathology would point out; certain agents produce changes in the system injurious to its structures, and to the functions of its various organs; and there seems no reason that we know of, why we should withdraw febrific poisons from the method adopted of investigating the action of poisons in general on the system.

III. Modern research has contributed so much to our knowledge of febrific poisons, that although much yet remains for investigation, they may be now catalogued. At the head of the list stands malaria, the agent in intermittent and remittent fevers, and long recognised under different names as a febrific poison, of widely varying force and manifestation. Writers have not, we think, drawn a sufficient distinction between the emanations from marshes and those arising from animal matter, especially animal excreta, or effluvia from the living bodies of animals; all these kinds of gaseous poisons have been classed under the general term miasm, thus leading to a false idea of relationship as to their mode of action and effects on the system. We should prefer to separate these from each other, and place them in two groups. According to this arrangement, we should designate as malarious all fevers arising from the poisonous emanations of marshes, or from those consequent on the action of water upon the earth itself, charged with vegetable *débris,* under whatever form it may exist, whether on the shores of tidal rivers or ditches, in badly-drained tillage land, wet meadows, &c.; thus restricting the term to its original use, namely, as signifying the *bad air* (*mal 'aria'*) arising from the meadows of the Campagna. Miasmatic fevers would be the other group, and would include all those forms of fever arising from decomposing animal *débris*, or from mixed animal and vegetable remains, but especially from animal excreta. The term miasm, being derived from the Greek verb *miaino* (μιαινω), signifying to pollute, but especially to pollute with excrement, well expresses the origin and cause of this group of fevers, which may be termed civic, in contradistinction to the preceding; inasmuch as their causes haunt the habitations of man, especially where no provision is made for the removal of the polluting matters, as in unsewered villages and streets, camps, &c.

It should not be forgotten, however, that the fæcal excreta are not the only source from which miasm may be developed. It is extremely probable that the excretions from the lungs and skin are also deleterious if concentrated, and that these, in conjunction with such circumstances as will aggravate their noxiousness, are sufficient to induce miasmatic fevers. Of this kind are the Irish fever, the jail fever, &c.*

A large and well-defined group of specific febrific poisons come next, the chief characteristic of which is, that they are propagated from person to person, and seem to originate—or, if not originated, to be reproduced —within the organism of animals, and to be given off from the skin, lungs, or other excreting organs. To this group belongs the poison of hydrophobia, which, given off by the salivary glands of the dog, or other canine or feline animal affected with the fever, impregnates the saliva and renders it poisonous. To this group, we need hardly say, belong the exanthemata, and probably the febrific agent in pertussis, and possibly even in influenza and in many cases of what appears to be common catarrh. To this class should be referred the group of morbid phenomena known as syphilitic diseases, for these eruptive fevers always run a definite course; and, although they recur from time to time, and so may appear to differ widely from the other exanthemata, yet a very simple hypothesis will suffice to reconcile this objection. In the other exanthemata, the febrile paroxysm seems to clear the blood of the poison circulating with it, whereas the syphilitic febrile paroxysm fails to do this,—in numerous examples, at least,—although, in a large proportion, there is reason to believe that an absolute depuration is effected by one or two paroxysms. But in those cases in which this does not happen, the poison (continuing dormant, perhaps) is ever ready to be excited into action by the causes which are known to arouse other latent febrific poisons into activity, as exposure to cold, and other depressing agencies.

Another class of febrific poisons are those which are secreted by animals, and used by them as a means of defence. And perhaps in this class we should also include the poison of putrid animal fluids merely, unimpregnated with such a poison as that of plague or puerperal fever—or perhaps this might be indicated amongst the miasmata, although not gaseous.

---

* Under the term Ochlesis, Dr. Gregory has recently described the effects produced by these emanations, in a paper, read before the Medico-Chirurgical Society. We are inclined to think that the author has attributed effects to them which really result from a specific poison; but the paper is interesting, and led to a discussion which strikingly illustrated the influence of a moist atmosphere in the communication of gaseous febrile poisons.

It yet remains to refer to those numerous forms of fever which depend upon the commingled action of two or more febrific poisons, such as, probably, cholera and typhus are. Few writers have dwelt much upon this point, and fewer still have distinctly recognised the dependence of the febrile phenomena upon such twofold action. Yet perhaps the fevers which depend upon two or more febrific agents are more numerous, and more varied in their phenomena, than those dependent on a single agent. Few of the exanthemata run their course unmodified by the miasmatic poisons, and few miasmatic fevers occur amongst a population occupying a locality in which malaria may be generated in greater or less quantity, without being modified by the malarious agent. And the established fact, that influenza, scarlatina, cholera, and, indeed, all epidemics, are most fatal in districts in which miasmata or malaria abound, is sufficient evidence of this law of compound action of febrific poisons, and of the great need there is for a more scientific investigation of the pathology and therapeutics of fever in general, and of malarious and miasmatic fevers in particular.

IV. Now, with reference to the modes of action of these various febrific agents, it may be stated generally, that the first manifestation of their presence takes place in the nervous system. The influence thus exerted may either irritate or depress the system; but, in general, depression is first observed; then excitement follows. Dr. Copland advances the theory, that the nervous system of the lungs is first attacked, in consequence of the poison entering the system through the respiratory surface; but if it could be shown that such was the fact in the case of gaseous febrific poisons, there still remains those instances to account for, in which the poison is introduced by a wound or abrasion of the surface. There can be little question that the ganglionic portion of the nervous system is most directly affected by the poison, and in many instances the cerebro-spinal is implicated in a less degree; but in by far the majority of cases, the cerebro-spinal is fully involved in the morbid change. Hence the lassitude, the muscular pains, the convulsions, which accompany, from time to time, every form of fever. The circulation of poisoned blood through every part of the nervous centres will necessarily be followed by morbid phenomena, corresponding thereto. How these phenomena can arise is now easily comprehended,—the great advances made in neurology having thrown a flood of light upon this difficult part of the pathology of fevers.

Nevertheless, the action of the poison will be modified and determined by those various circumstances which we have enumerated as modifying the action of poisons in general. If we take the malarious poison as an example, we find that every variety of phenomena is produced, according to the quantity and other modifying circumstances. In many instances the effects on the ganglionic system are scarcely, if at all, perceptible; there are no rigors, little or no febrile action; in short, the only important symptom is the periodic irritation of some portion of the cerebro-spinal or ganglionic system. To this class of cases belong brow-ague, periodic gastralgia, yawning, laughter, convulsions, &c. In other cases, the action of the poison is not perceptible for some time after its reception in the system; or, in other words, is latent or hidden, until some exciting cause places the organism in such a condition as will allow the poison to take effect. In a third class of cases, the malarious poison never excites a febrile paroxysm; its influence is of a chronic kind, and induces a chronic morbid condition of the blood and of the organs subjected to its influence, which seem to be the stomach, liver, and spleen. Hence chronic gastrodynia, pleurodynia, hepatic and splenic congestion, and chlorosis, from the morbid condition of the spleen. In a fourth class of cases, the same poison, administered in large quantities, will induce almost immediate coma, convulsions, or death; and these are termed pernicious fevers,—*febres perniciosi*—and have been well described by Torti. In other fevers the poison will act with the same fatal rapidity. In the history of cholera asphyxia, for example, there are not wanting numerous instances of this kind. Thus, also, the poison of some of the exanthemata has acted; but in such cases (if we except the poison of the plague), the immediate fatal action has depended less upon the action of the poison, than upon the condition of the individual subjected to its agency; a highly morbid condition of the blood perhaps already existing, or the tone of the nervous system being already deeply depressed.*

The immediate and consecutive effects of febrific poisons are well worthy investigation and discrimination. In the exanthemata, while the immediate effects are the usual febrile phenomena, the consecutive are those by which the exanthem is distinguished. Within the tropics the action of the miasmatic and malarious poisons is on the gastro-hepatic system, and we therefore find acute gastritis and hepatitis arising as consecutive effects in both classes. Add to these so much congestion of those organs, and such a dyscrasis of the vascular system, that the capillaries readily give way, and blood is effused; and we have the "black vomit" and yellowness of the skin, which have led observers so much astray, and which, so far from being peculiar to one form of solar-tropical fever, seem to be symptoms common to every form of malarious and miasmatic fever; and probably to some forms of the exanthemata, as, for example, the exanthematous typhus.

But, while certain structures may be affected by all fevers, it is of importance to remember, that there is a specific determination in many to certain structures in particular. Thus, while in variola the nervous system suffers from the first, and there are vomiting, gastralgia, and other symptoms, indicating gastric neuralgia, after the lapse of a certain period inflammatory action is set up on the skin and mucous membranes. So also with scarlatina; so with rubeola and the other exanthemata. We have no clue to an explanation of this general fact, except in the general law of excretion; and of these we can only say, that there is some kind of affinity between the membranous tissue which is the organ, and the element which is present in the blood, and eliminated from it or by it. Why there should be this affinity between the kidneys and urea, we cannot say. And so with these specific poisons; all we can say is, that the tissue or viscus affected seems to be that which has an affinity for the poison, and which has to be eliminated from the blood; and it would appear that, in the process of elimination, inflammation and its sequelæ are excited.

---

* Salvatore de Renzi, a Neapolitan writer, enumerates the following varieties of the tertian ague: Cholerica, dysenterica, subcruenta, atrabilasis, cardiaca, diaphoretica (the perspiration commencing with the hot stage), syncopalis, algida (the cold stage never ending in the hot or sweating), lethargica, catarrhalis, colica, arthritica, pleuritica. In the first two or three, the miasmatic element is in operation; in some of the others, the variety is owing to modifications and peculiarities in the individual. (Miasmi Paludosi, Contagi ed Epidemie, del Dottor S. de Renzi. Napoli, 1826. Tom. i, p. 114.)

That this morbid affinity is something analogous to the normal, cannot be doubted, inasmuch as all morbid processes are but modifications of healthy processes. We therefore may expect to find those excreta that are given off from the body seeking the same outlets, when reintroduced into the body from without, as poisons. And in this way we may obtain a clue to the morbid affinities of the malarious and miasmatic poisons, especially the latter, for certain structures. Those miasmatic fevers that arise from a concentration of animal emanations from the skin and lungs, will be characterised by disturbance of function of the skin and lungs; that is to say, by capillary congestion, inflammation, &c. Thus we may account for the pulmonary and cutaneous phenomena of some forms of typhoid fever. Again, with regard to those fevers which arise from the emanations given off by the fæcal or intestinal excreta of animals, we might be guided by this theory, to look to the intestinal mucous membrane as the surface which naturally excretes some of the morbid products which arise during the decomposition of intestinal excreta, and are received into the blood.

One great difficulty has to be overcome, however, before we can carry out this principle satisfactorily to its legitimate results; and that is, our ignorance of the true functions of those numerous glandular structures which cover both the external and internal surfaces. Although much has been done of late years to elucidate their structure, of the functions of the Peyerian glands absolutely nothing is known, except from inferential evidence (see Vol. II, p. 136); and as to the cutaneous glands, although their structure and functions have been in some degree determined, and their relations to the exanthematous fevers demonstrated, we have much to learn respecting the affinities of the various poisons for various structures; we cannot say why the poison of variola should produce phenomena so widely differing from those of miliary fever, scarlatina, or erysipelas; or why in the former there should be a series of abscesses,—in the latter serous effusion only. Having, then, no sound physiological basis, the relations of fevers can only be investigated with reference to what may be termed unknown quantities.

V. The miasmatic fevers, in which the alimentary canal is involved, and which have a clear and well-defined connexion with the fæcal miasmata, are summer diarrhœa, or English cholera, dysentery, Asiatic cholera, and typhoid fevers. In each of these, different portions of the alimentary mucous membrane are the seat of the local phenomena, although it is of importance to remember that the one disease will run into the other; thus, dysentery will commence with diarrhœa, be accompanied by typhus, and be communicated. Typhus, again, will be ushered in by diarrhœa, dysentery, or the Asiatic cholera. Any allocation, then, of the poison in each of these is subject to such modifications; and, indeed, it is probable that, with the exception of Asiatic cholera, the poison in all is identical, and that the part of the alimentary canal to which the poison is determined, receives the preference in consequence of modifying circumstances of race, climate, season, habits of living, especially as to cleanliness, food, and ventilation; or of local diseases, as of the liver, stomach, &c. In dysentery, the poison attacks the large intestine; in continued fever, the lower third of the small intestine, and commencement of the large; in English cholera, the liver and small intestines; in Asiatic cholera, the stomach, liver, and small intestines.

The action of the poison of continued or typhoid fever on the alimentary mucous membrane has been often demonstrated; and the fever itself has received names, indicative of the surface principally affected, as abdominal typhus, gastro-adynamic fever of Pinel, entero-mesenteric of Petit and Serres, Broussais's adynamic form of gastro-enteritis (with this Asiatic cholera is classed), follicular enteritis of Scoultetten, dothinenteritis of Bretonneau, ileo-dycliditis of Bally, gastric fever, gastric-nervous fever, intestinal-nervous fever, mucous fever of the German writers. Although the dothinenteritis of Bretonneau, and the entero-mesenteric fever of Petit and Serres may be more properly referred to exanthematous typhus, inasmuch as one attack is stated to afford immunity from a second, yet still we have the great general fact of a whole class of fevers attacking civic populations, in which the gastro-intestinal mucous membrane is affected to such an extent, as to determine the name of the fever. How is it, then, that this portion of the mucous surface is so constantly affected; and whence is the poison derived?

The position of civic populations, with respect to the miasmata derived from animal excreta is this,—a daily and hourly accumulation of the fecal evacuations of the population and of the domestic animals they employ, takes place with the most unerring continuousness, unvaried by any circumstances whatever. On the other hand, the removal of these excreta is uncertain, and depends upon various circumstances, as the proper construction of water-closets and of sewers, a due supply of water, a due diligence on the part of scavengers, &c. Recent inquiries have shown that all cities, towns, and villages are more or less defective as to their arrangements for the prompt removal of these noxious agents; whilst in numberless instances there are scarcely any arrangements whatever, and they are left to be completed as chance or necessity may dictate. The consequence is, that great numbers of people are exposed to these fæcal miasmata, and especially in the summer months, when decomposition is accelerated by the augmented temperature of the season.

There is no large civic population, whose condition, with reference to these miasmata, we can investigate so well as that of London. The weekly returns of the Registrar-General of the deaths in London and the causes of death, are accompanied by a table of meteorological observations; so that the deaths from typhus, diarrhœa, dysentery, and cholera can be considered in relation to those changes in the temperature and moisture, which influence the degrees of activity of chemical decomposition, and consequently the amount of miasmata evolved. Add to this, that the observations are made over a population of nearly two millions, and we have all the elements for satisfactory and instructive deductions.

Recent inquiries have shown that the sewerage of the metropolis is by no means so perfect as it was presumed to be; for, although the sewers are numerous, and, as regards the brickwork, sufficiently well constructed, yet their form is faulty, inasmuch as it allows of the accumulation of their contents. A greater evil is in the fact, that the fall of whole lines of sewerage is so defective, that there is no current through them; and this, again, is a cause of a vast accumulation of fæcal matters, derived daily and hourly from the myriads of human beings and domestic animals that people the vast metropolis of the British empire. The sewers are therefore rather elongated cesspools than ever-active channels for the prompt removal of these noxious debris, and are only emptied by the floods of a heavy fall of rain or by artificial flushing.* But, in addition to these faulty sewers, it appears from a communication, addressed to the 'Builder,' that there is scarcely a house in the metropolis without a cesspool under it, and that very many old houses have two, three, and more under them. The writer—Mr. Phillips, Surveyor to the Metropolitan Sewers' Commission,—estimates that their number is 300,000, and that each contains 58½ cubic feet of filth. Their exhaling surface is equal to 62 acres nearly, and the total quantity of foul matter contained in them amounts to 17½ millions of cubic feet. They are, in fact, equivalent to a cesspool ten miles long, fifty feet wide, and six feet six inches deep, which would extend through London, from the Broadway at Hammersmith, to Bow Bridge over the river Lea. Mr. Phillips adds, that, if such a gigantic cesspool were to be seen, it would fill the mind with horror. As it is, the ground in old districts is honeycombed with these cesspools, and perhaps matters are worse than if the whole were open.

Such being the condition of the sewers and cesspools of the metropolis, what effect have the miasmata thence evolved on the intestinal or defecating mucous membrane? In the spring of 1842, up to the end of June, the deaths from diarrhœa, dysentery, and cholera averaged 10 or fewer weekly; and from typhus, from 15 to 30. From the beginning of July, the deaths from the former gradually increased from 10 weekly to 100, in the week ending August 20th, this maximum being coincident with a maximum temperature of 80°. The increase of temperature commenced from the week ending August 6th, during which the highest temperature was 72° (being below the average of the previous weeks), and the deaths from diarrhœa, &c., were 60. *When the deaths from diarrhœa were at the maximum, those from typhus were at a minimum, falling from* 30 *to* 15. The decrease in temperature and in mortality from diarrhœa was coincident with a heavy fall of rain,—3⅓ inches falling in the month of August. While the mortality from diarrhœa diminished, that from typhus increased, so that, during the week ending October 29th, the latter being 45, the former was only 13. During the following month of November heavy rains fell,—the total quantity for the month being 4·048 inches,—and in the week ending December 10th, the mortality from diarrhœa and typhus had fallen to minima of 15 and 10.

During this summer, then, we find the mortality from diarrhœa, &c., at its height at the very time when the amount of miasmata in the atmosphere of London would be the greatest; that is to say, when, by the want of previous flooding rains, the accumulation in the cesspools and sewers was the greatest; and when, in consequence of the high temperature, the chemical decomposition would be the most active. Concurring with a heavy rain, the effect of which would be to scour the sewers, and with a fall of temperature, from 80° during the week ending August 20th, to 58° during the week ending October 1st, we have a fall in the mortality from diarrhœa, &c., from 100 in a week to 25.*

---

* A stranger, accustomed to a pure atmosphere, can readily detect the gully-holes in the metropolis at a distance of several yards; and sometimes the stench that arises from them is overpowering.

There are, however, so many modifying circumstances to be taken into account in numerical estimates of this kind, that the results of one year's investigation is hardly a sufficient basis for an argument. We will therefore take another year or two.

The spring and summer of 1843 were wet and cold. Large quantities of rain fell in every miasmatic month except September, which was a dry month until the middle. The temperature, especially in April, May, and June, was low, never rising above 69°—its highest point during the first week of July,—until the second week of July, when the thermometer rose to 79°. It fell, however, immediately, until the end of the month, to 69° again, and only rose again in the middle of August, when the highest temperature during the fortnight ending August 19th, was 75°. It was from this period that the deaths from diarrhœa rose,—from 20 during the week ending August 5th,—to 75 during the last week of the month. Concurrently with a fall in the temperature, there was a diminution of mortality, but it rose again in September to 100, the weather being hot and dry. From this maximum the mortality from diarrhœa and the temperature declined together, in almost parallel lines, into autumn. It is worthy of notice, that, during the cold, wet spring, when the mortality from diarrhœa was at a minimum below 10, that from typhus was never below 45; and, in the week ending June 3d, rose to 73. This was after a fall of rain during the previous month, amounting to 4½ inches. Thus the analogy with the previous year is maintained throughout.

The miasmatic season of 1844 was marked by fewer deaths than usual. The temperature was moderate until the second week in June, and the deaths from diarrhœa and typhus were few, the former rising little above the cold months. But the high temperature in June was quickly followed by a change in these respects; the mortality from typhus rising to 40 during the week ending June 29th, against 25 during the week ending June 15th. The mortality from diarrhœa also increased gradually from 8 during the week ending June 8th, to 75 (the maximum) during the week ending August 10th. A slight check in the increase of the mortality occurred concurrently with a fall of rain during the last week in June; and this and further rains in July seem to have kept the general mortality from diarrhœa, cholera, and dysentery in check. The deaths from typhus fell from 40 during the week ending June 29th, to 25 in the following week; and this decrease was coincident with a fall of rain, amounting to 2·83 inches in the two weeks.

---

* The less number of deaths from typhus when the mortality from diarrhœa, &c., was greatest, may be readily explained on two grounds. First, that the lower classes would be less in their houses and more in the open air, and thus less exposed to the bad effects of confinement in an atmosphere loaded with animal emanations; and secondly, that, of the persons attacked with and dying of diarrhœa when the mortality was at the height, many were cut off, in reality, in the first stage of typhus. In short, if the dose of the poison had been less, its effects would have been less immediate, and they might have struggled on, to die of typhus, with ulceration of the intestinal mucous membrane.

The miasmatic season of 1845, although hot during the months of June and July, was cool both before and after. Abundance of rain also fell, and the sewers were therefore frequently flushed. In May there were 2·2 inches, and during June, July, and August, 8·83 inches. The consequence of this state of things would be, a much less evolution of fæcal miasmata, and we might expect fewer deaths from typhus, diarrhœa, and the other miasmatic diseases. This was in fact the case. The weekly mortality from typhus was only once above 20, and that from diarrhœa oscillated between 50 and 35.

The miasmatic season of 1846 differed widely from that of the preceding year. The temperature gradually rose until it attained a tropical height in June; and, although it fell again in the week ending July 11th, concurrently with a fall of rain of ·96 inch, it immediately steadily rose again, until it attained the extraordinary height of 88° during the week ending August 1st. The deaths registered in the following week from diarrhœa, dysentery, and cholera, were 238! During the month from May 20th to June 21st, there was absolutely no rain; the sewers of the metropolis were stagnant cesspools, acted upon by a temperature in the shade often above 80°, and on one day (June 20th) 87°. And the rain which fell on June 22d was only 0·37 of an inch, a quantity totally inadequate to flush away the accumulations of the previous month. From that day until the 1st of August, the weather remained remarkably dry, rain only falling in small quantities; and during the last week in July there was no rain whatever. Thus, all the requisites were in operation for a copious evolution of that miasmatic poison which acts on the defecating organs, and with the result of increasing the mortality caused by diarrhœa, dysentery, and typhus, from 9 during the first week of June, to 238 during the second week in August. Fortunately, during that week a thunder-storm burst over the metropolis; 2 inches of rain fell in three days; the sewers became the channels of foaming torrents; and the number of deaths gradually diminished. But, as if the poison had accumulated in the blood from long exposure to its influence, the deaths were still comparatively numerous, being 160 during the week ending August 29th. At this time another hot and dry period commenced, and continued uninterruptedly from August 29th to September 23d, when 1·2 inch of rain fell. By October 24th, the deaths from diarrhœa, &c., had fallen to 35; but, during the same week, the mortality from typhus had risen from a minimum of 23 during the week, when that of diarrhœa was at its maximum, to a maximum of 60; and, as if the unusually concentrated miasmatic poison of the previous summer prolonged its action into winter, the mortality from typhus remained unusually high until the week ending December 19th, when it fell to 35.

To contrast these two years more strongly as to their varying meteorological conditions, and the influence of these on the evolution of miasmata, we subjoin a table, showing the highest mean temperature, the fall of rain in inches, and the deaths from diarrhœa, dysentery, and cholera in each week, commencing with the first week in June. The table is rather to the disadvantage of 1845, as the weeks in 1846 each end a day earlier.

Week ending	1845.			1846.		
	Highest temperature.	Fall of rain in inches.	Deaths from diarrhœa, dysentery, and cholera.	Highest temperature.	Fall of rain in inches.	Deaths from diarrhœa, dysentery, and cholera.
June 7	75·4	0·75	5	83·9	0·00	8
14	83·2	0·01	6	82·4	0·04	12
21	73·5	0·55	6	87·0	0·00	31
28	69·5	0·62	13	85·2	0·52	40
July 5	79·5	0·28	15	86·8	0·12	76
12	80·5	0·64	30	68·7	0·96	98
19	73·0	0·29	30	82·5	0·31	149
26	72·3	0·52	50	76·5	0·45	187
Aug. 2	66·5	1·15	41	88·4	0·82	218
9	69·8	0·59	50	83·7	2·12	238
16	66·0	0·43	48	74·6	0·54	180
23	67·7	0·81	42	68·1	0·76	169
30	69·4	0·04	38	76·9	0·33	148
Sept. 6	67·0	0·07	38	78·1	0·00	126
13	70·8	„	46	77·2	0·02	87
20	64·1	1·37	38	76·3	0·00	83
27	60·1	0·81	52	69·5	1·60	62

In reading this table it must be remembered that the figures in the third column show the number of deaths *registered*, and not the number *occurring* during the week. Perhaps this circumstance, as regards the summer months, need not be taken into account so much as in reading the numbers for the winter months, when a corpse will be kept longer previous to interment. It would be a great improvement, however, in the returns, if the deaths *occurring* during the week were returned, in place of the deaths *registered*. The table shows, nevertheless, very clearly the double influence of fall of rain and of temperature, and warns us not to take them singly.

Although the amount of rain and the temperature during the miasmatic seasons will determine the amount of matter subjected to chemical decomposition, and the intensity of that process, yet an incessant renewal of the atmosphere would have an influence in diminishing the mortality caused by the poison. During the dry, hot season of 1846, the air was remarkably stagnant as compared with 1845, especially at the time when no rain fell. The total amount (in miles) of horizontal movement in the air, and the dothinenterite for each month and during the miasmatic season of the two years, were as follows:

Month of	Horizontal movement of the air, in miles.		Deaths from diarrhœa, dysentery, and cholera.	
	1845.	1846.	1845.	1846.
June	3040	2620	30	91
July	3740	4460	125	490
August	4545	2850	219	953
September ...	3630	2050	174	358
Totals.........	14955	11980	548	1892

During July and August, 1845, the increased horizontal movement was coincident with a decrease in the mortality from diarrhœa, &c.; but at the same time, the temperature was low, never attaining a higher point than from 66° to 70°, and much rain fell; whereas the greater horizontal movement of July 1846 was accompanied by less rain, and an unusually high and continually increasing temperature. There was, therefore, a gradual increase in the number of deaths from diarrhœa, &c. It is probable, however, that the prevalence of brisk breezes during this month had a beneficial effect, and prevented a still greater mortality.

The solubility of a gaseous emanation in the atmosphere will depend upon the amount of vapour contained in the latter; and in proportion as it is soluble, will be the quantity received into the system, and probably, in some degree at least, its activity as a poison. It is now well ascertained that gaseous febrific poisons are not communicable through a perfectly dry atmosphere; the harmattan destroys even the infection of smallpox. Such being the facts, the activity of a febrific poison will depend much upon the dampness of the atmosphere, and we shall find fevers most prevalent in situations where heat and moisture co-operate. Such is the undoubted fact with regard to cholera and plague; is it so with reference to the activity of civic miasm? The weekly meteorological tables that accompany the tables of mortality do not (unfortunately for our purpose) give an estimate of the mean amount of water in a cubic foot of air, until the notes of Mr. Glaisher were begun in December 1847. But we have his estimates up to July 15th, (the date of our writing), and we find that, during the six weeks of 1846, previous to that date, the mean amount of water in the atmosphere was fully equal to that of 1845 for a similar period, although there was much less wind and rain during the former than the latter.

The Third Report of the Metropolitan Sanitary Commission contains some interesting illustrations of the general fact which the preceding statistics set forth. During April last, several cases of fever occurred among the scholars of the Westminster School, and the inhabitants of several houses in the Cloisters and Precincts of the Abbey Church; rumour ascribed the origin of the fever to the emptying and cleansing of a number of cesspools in the immediate neighbourhood. Upon inquiry being instituted by the Metropolitan Sanitary Commission, it was found that the cesspools in question had been emptied nine weeks previously, and then in such a manner as to prevent the escape of noxious gases. But it was also found that the cesspools and sewerage of the locality were in a most imperfect state. The track of the fever was almost exactly in the course of a foul private sewer; and, in Little Dean's Yard, a small space, about 45 yards long by 35 yards wide, there were no fewer than seven gratings, being direct openings into this sewer. Several boys had fever, who did not reside in the houses situated near this sewer or its branches. On examination, it was found that these were only apparent exceptions; for, between the entrance door of the school, adjoining the school-privy, and the entrance to the dormitory, there is a space having two gratings communicating with the foul private sewer; this space was chosen by the boys alluded to as their play-ground, and consequently they were directly exposed to the noxious emanations. An additional and corroborative fact is found in the circumstance, that nearly if not all the Queen's scholars attacked were juniors, whose duty it was to act by turns as janitors at the same place.

The nature of the morbid changes produced by civic miasm, in doses varying as to the amount, is illustrated by several other facts recorded in this Report. Thus, Mr. Cooper, the surgeon to the family of the Dean of Westminster, states that, after passing a drain grating, from which a most fætid and disagreeable smell arose, he was immediately attacked with nausea and an indescribable sensation of illness, and was confined to the house for some days with feverish symptoms. Mr. Cooper mentions two other instances of sickness arising in a similar way: one of these was the case of a stout, healthy maid-servant, who, after passing one of these horrible "gully-holes," was affected with retching and vomiting, which were followed with all the usual symptoms of fever for three days. The other case was that of a gentleman who had violent headache, followed by vomiting, diarrhœa, and febrile disturbance, which kept him to bed for several days.

A more striking and remarkable illustration is one quoted in the Report from Dr. Christison; who relates that, in August, 1831, twenty-two boys at a boarding-school at Clapham, were seized in the course of three or four hours with an alarming irritation of the stomach and bowels, subsultus, and excessive prostration. Two died in about twenty-four hours; and, on examination after death, the Peyerian glands were found to be enlarged in the one case (a child), whilst in the other there were ulcers of the mucous coat of the small intestines, and softening of that coat in the colon. The only circumstance that could be found, after a most diligent investigation, to explain the accident, was, that two days before the first case was attacked, a foul cesspool had been opened, and its contents spread over an adjoining garden.

That much of the increased mortality from diseases of the intestinal mucous membrane, during the summer of 1846, is to be attributed to the greater amount of miasmata present in the London atmosphere, cannot, with these facts before us, be doubted; but it is equally certain, that the miasm would have been less fatal if the population had been less predisposed to its influence. This predisposition consisted in an impoverished and morbid condition of the blood, in consequence partly of the defective supply of food, and partly from the want of fresh vegetable food during the previous winter and spring. That this cachectic condition predisposes to the action of febrific poisons, is a matter of universal experience, and has often been seen in camps, besieged towns, devastated countries, &c.; although it has not always been rightly interpreted. The greater predisposition thus arising has been termed the *genius epidemicus*, and attributed to some unknown constituent or condition of the air. Several able observers have applied this doctrine to the febrile phenomena of the last two or three years, and have pointed to the potato rot as a proof of some great meteoric change going on in the atmosphere. But as no change in the atmosphere has been detected, or demonstrated as a sufficient antecedent, and as we have an amply sufficient reason for this greater susceptibility of the people to the influence of miasmatic and specific poisons in the causes stated, the rules of sound philosophy require us to adopt this theory rather than the other, and to trace the *genius epidemicus*, so far as it regards febrile disorders at least, to a general morbid condition of the population, of an asthenic type. The prevalence of land-scurvy (a novelty in modern pathology) is a striking proof that this general morbid state existed in 1846; and thousands presented that cachectic state which ushers in the disease, in whom purpura vera or land-scurvy was never developed.

Reverting to the pathology of continued or typhus fever—it is now we think manifest that the direct action of this civic miasm on the intestinal mucous membrane must be considered, and its indirect action on the blood and the system at large, in connexion with specific poisons, estimated. That its presence in the blood would predispose to the more ready action of other poisons cannot be doubted, and particularly of those which act on the same tissues, or have a community of origin. Thus the poison of cholera, it would appear, almost requires this miasmatic predisposition—this preparation of the blood for its reception—before it can produce morbid changes in the organism. Where the materials for civic miasmata abound, and when the temperature is favorable for chemical decomposition, there and then the Asiatic cholera is most prevalent; and so necessary do the fecal miasmata appear for its propagation, that the simple removal of a camp to a dry clean locality has been sufficient to arrest its progress in a regiment. And in fact such removal should be considered, *à priori*, as equivalent to the fall of rain, lowering of temperature, and drying of the atmosphere, which, as we have shown, checks the mortality of summer diarrhœa and English cholera. The contradictory statements as regards the contagiousness of cholera, will (we are sure) be reconciled in most instances by a due consideration of those circumstances which predispose to the action of the poison, either by inducing a morbid condition of the blood, *analogous* at least to that which is produced by the choleraic poison itself, or by facilitating its communication to, and reception by, healthy individuals.

The researches of French pathologists, especially those of the capital (where fæcal miasmata, in proportion to space and population, are infinitely more abundant than in London), have shown that inflammation and ulceration of the intestinal mucous membrane, and of the glands situate therein, are a very frequent complication of fever in Paris; and from the descriptions they have given of the latter, it may be fairly suspected that two or three species of typhus have been observed by them. Dr. Ormerod devotes two or three chapters to the subject, and looks upon the affection of the bowels as something extraneous to the essential nature of the fever—as a mere complication like bronchitis. On this point we differ with Dr. Ormerod, as it appears to us that fæcal miasm is sufficient of itself to develope continued fever and this morbid state of the bowels; but, nevertheless, we readily grant that the action of the miasm may be connected with, and modified by, the action of another febrific poison, as by malaria, or—as probably occurred in the fever prevalent in London—by the specific poison of exanthematous typhus; both being modified by the condition of the blood dependent on the failure of the potato-crop. In the cases detailed by Dr. Ormerod, we find diarrhœa ushering-in the disease.

The seat of the intestinal irritation in typhus is pretty constant:

> "But whatever be the form of ulceration, its special seat, and the point from which it begins, is the same; the Peyer's glands of the lowest part of the ileum, whether the solitary or the aggravated, are attacked first, and thence the disease

spreads upwards and downwards as from a centre. It is only in severe cases, and in particular seasons, that any great tendency of the ulceration to spread far along the colon is noticed; it rarely passes downwards beyond the cæcum, but upwards it extends so regularly, that the whole history of an ulcer can be clearly traced in most cases of fever fatal during the second week; a regular gradation of lesions may be found from the first formation of the sub-mucous deposit to its final elimination, or from the first abrasion of the epithelium to the perforation of the mucous, the muscular, or may be the serous, coat of the intestines, commencing in the ileum, reaching its highest point of intensity at the ileo-cæcal valve, and declining thence along the course of the colon." (p. 121.)

Having traced the connexion between diarrhœa and fæcal or civic miasm, we have now to consider dysentery and cholera. The term dysentery has been applied to various distinct diseases. In the first place, articles of diet, as bad food or impure water, have been known to bring on a chronic irritation of the colon, and this uncomplicated with the action of any febrific poison. But there are other forms of dysentery which are true fevers, and which (it must be carefully remembered) *correspond most closely with the various forms of continued fever.* Thus Dr. Copland mentions the sporadic, the endemic, the epidemic, the remittent and continued, the inflammatory, bilious, typhoid, putrid, scorbutic, complicated dysentery; all which terms have been and are applied to continued fevers. In true dysentery there may be three, or even more poisons in action at the same time: firstly, there may be the fæcal miasm; secondly, a specific febrific poison, as that of exanthematous typhus, rendering it contagious; and, thirdly, the paludal poison, rendering it remittent. It may be that there is a specific poison in dysenteric fever, whose special point of irritation is the colon, but a vast experience renders it probable that the cause of the dysenteric phenomena is none other than the fæcal miasm, determined to the large intestines by raw, bad food. Such was its mode of origin in Ireland, in which country it took the place of the diarrhœa prevalent in England, and still more prevalent in France.

The explanation of the symptoms connected with the upper portion of the alimentary tube is rather more complex. In the English and Asiatic cholera, there is something more than mere irritation of the intestinal mucous membrane; the cramps, tormina, violent vomiting, and great depression of the powers, show that the cerebro-spinal system is involved directly. In typhoid fevers, the morbid changes in the blood are sufficient to account for the neurotic phenomena; in cholera, we have the direct action of a poison that, like malaria, acts on the nervous system at once. And indeed we perhaps ought to look to some combination of this poison with fæcal miasm and a specific poison, for an elucidation of the choleraic phenomena. There are several forms of *intermittents* closely allied to dysentery, typhus, and cholera. Thus the "terzana colerica" of De Renzi is so called by him because the accession begins with violent purging and vomiting, hoarse voice, facial sweats, small pulse, coldness of the extremities, and, in short, all the symptoms of cholera; it is seen at Monopoli.* In like manner, the "terzana disenterica" is characterised at the accession of the fit by dysenteric symptoms; it is observed in the vicinity of Pozzuoli. The "terzana algida," observed also at Pozzuoli and the neighbourhood, and at Capua, is singularly similar to Asiatic cholera in the symptoms, from which it derives its name. There is the greatest coldness from the beginning to the end of the paroxysm; the pulse never rises, nor does heat come on; the thirst is intense, the breathing difficult, the countenance cadaveric. It would appear, too, from the fact of cholera haunting low damp grounds, that this view of the compound nature of the poison is correct, in some degree, at least. But just as dysentery and abdominal typhus may be contagious, so also may the cholera; and it is far from improbable that a specific poison of the nature of exanthematous typhus enters into the composition of the choleraic poison of Asia, just as it enters into the composition of the poison of the yellow fever of America.* It is indeed on this hypothesis only, that we can explain the otherwise anomalous facts in the history of typhus, yellow fever, and Asiatic cholera; as, for example, one attack being prophylactic against a second, &c. &c. This theory of the compound nature of the commoner febrific poisons will, we think, be applicable to the explanation of other febrile phenomena also, and is applicable, indeed, to the whole range of fevers. It will help us to correct views as to their contagiousness, and the circumstances that modify it.

---

* Op. cit., p. 114.

There is one other febrific miasm which requires a passing notice; namely, that arising from the effluvia of living beings. There is ample evidence to show that if a number of human beings, or even brutes, be confined for a time in an atmosphere loaded with their own proper effluvia, a fever having a typhoid type will be developed amongst them, and even in healthy individuals breathing the poisoned atmosphere. Little is known definitely about this poison, but it is not improbable that it enters into combination with other febrific poisons of its class, and that it is communicable by crowded cities when besieged, in the baccaroons in which the slaves are crowded previously to embarkation, in the crowded huts of the Irish peasant; and, indeed, amongst all numbers of persons crowded in a space in which ventilation is defective, this form of typhus may be expected to arise, and the poison thus generated may act in smaller doses as a cachectic agent, and a predisponent to other forms of fever, and so modify their phenomena. But although certain of the fact of its existence, it is not possible, we think, to separate its effects on the system, so as to analyse them, and trace them to their source. In the essay on Ochlesis by Dr. Gregory already referred to, we have as much definite information perhaps as can be obtained. We can say generally, that the blood is *corrupted* by it (to use an old pathological phrase), and thus rendered a nidus of the most suitable kind for the specific action of febrific poisons generally; while the circumstances in which the individuals affected by it are placed, render them peculiarly liable to receive the specific febrile poisons. Thus hospital gangrene, puerperal fever, typhoid, dysenteric, choleraic, miliary, and erysipelatous fevers become endemic in hospitals, prisons, camps, besieged towns, &c.; and from some general facts connected with the exanthemata, it would appear probable that it determines to the cutaneous and pulmonary surfaces (as the fæcal miasm determines to the intestinal), and so renders the exanthemata in particular more malignant and fatal. How far this form of miasm is the cause of the bronchitis, which is so often a complication of continued fever, remains yet to be shown; that the usual causes of bronchitis, as exposure to cold, a rapid fall in temperature, or a predisposition to pulmonary congestion, are in operation in fever with this complication, cannot be doubted; but there are cases which render it probable that a specific determination to the pulmonary mucous membrane occasionally takes place, and that the cause of it is a poison analogous to that of rubeola, influenza, and the epizootic pleuro-pneumonia—the last two, however, being perhaps as identical as variola and vaccinia.

---

* It is altogether improbable that the inhabitants of the localities whence cholera breaks forth are free from some form of typhus, or even from the exanthematous form; yet we never or very rarely hear of that disease being in India.

VI. It now remains to determine the mode of action of the febrific poisons on the blood, and especially of those which belong to the typhoid class. The condition of the blood is so remarkable in the latter, that it has continually attracted attention, and of late years many analyses have been made. These are, however, most contradictory in their results; and it is manifest, that unless the blood analysed be taken from persons identical in every respect, and in the same stage of the fever, the results must always be diverse. Le Canu found the globules below even the minimum proportion of healthy blood; Buzorini found a diminution of fibrin and albumen, as well as of colouring matter. Under the microscope, the globules are seen to have lost their normal form, to have increased in size, and to be without the proper sharpness of their edges. In the Sydenham Edition of Simon's 'Animal Chemistry,' there is a good summary of all that is known as to the condition of the blood in typhus, including an estimate of the more recent analyses. We learn generally, that the fibrin never rises above the normal standard, but is frequently diminished, and that the albumen and salts are also in diminished proportions. The hæmato-globulin, although relatively to the other constituents in larger proportions, is also absolutely less in quantity. The blood coagulates imperfectly, the clot is soft, friable, and of a very dark, almost black-red colour, and has no buffy coat. In malignant typhus, the blood is very fluid, coagulates imperfectly, or not at all, and the serum is of various shades, from a bright yellow to a red tint. The hæmato-globulin is dissolved in the liquor sanguinis, and then a red or yellow transudation takes place.

These changes are not, however, the primary effects of the poison; we are not aware, indeed, that the latter have been investigated at all. Yet perhaps the changes that supervene during the premonitory or precursory stage of fever have the most important bearing on therapeutics. We can infer something, however, as to the condition of the blood and its constituents. The first action of the poison is apparently upon the functions of the blood; these are impaired, or, in peracute cases, cease altogether. The functions of the blood are, first, to maintain the activity of the nervous and muscular systems, and, secondly, to supply the materials for the molecular changes constantly going on in the tissues. It is essential to these being properly performed, that the blood-corpuscles be in a fit condition to carry oxygen; and it would appear, from the symptoms which mark every stage of fever, that this function of the corpuscles is impaired from the first. There is the *besoin à respirer* developed even in the premonitory stage; the sensations and condition of the respiratory organs are precisely the same as if their capacity had been diminished, and the due aëration of the blood thus prevented. The patient makes many forced inspirations, sighs or gapes, and the breathing is quickened on the least exertion. The hæmato-globulin is changed also, for the skin assumes a peculiarly pale, sallow, and unhealthy look. The functions of the nervous system are impaired, in consequence of these changes in the blood, namely, the changes operated by the poison, and the defective oxygenation: hence lassitude and weariness, disturbed functions or congestion of some or of all the viscera, and a lower temperature.

The duration of this stage may be either a definite or an indefinite period; in the majority of fevers, perhaps in all, with a specific poison, the period is definite; in the miasmatic and malarious classes it is indefinite, and in both may never reach to be a true fever, but constitute a chronic cachectic condition, and thus there is a predisposition to the action of the specific febrile poisons established; this we apprehend is the condition of the blood that appears requisite to the development of certain miasmatic fevers. In hydrophobia we have apparently an exception to this rule, but in that disease the local or specific action of the poison is concentrated directly upon the respiratory system, and not so directly upon the blood in the first instance; so that defective oxygenation takes place rather from defective respiration, than from an impaired function of the blood-corpuscles.

Such being the predisponent condition of the system, it is not difficult to understand how many causes of fever, as hitherto stated, are not causes in the true sense of the term, but only influences which facilitate and quicken the morbid changes already going on. The term exciting cause may be permitted, if its meaning be thus limited, but if we say that fear, or exposure to cold, or a debauch, or starvation, has brought on an attack of fever, the expression is incorrect, as that is induced by a poison already existing in the blood, and the operation of which has only been facilitated. It still remains, however, to determine in what the premonitory stage differs from the actual accession, and how these depressing or disturbing influences operate in exciting the true febrile phenomena.

In the first place, the difference between the premonitory stage and the actual accession may be one of degree only; the oxygenation of the blood may gradually be less and less; the congestion of all organs becomes as gradually greater and greater, until at last all the higher phenomena exist, without its being possible to determine the day when the symptoms were aggravated. In such a case the dusky hue of the countenance, the cachectic sallowness, and the general debility dependent on the morbid condition of the blood-corpuscles, steals on gradually, and there is no violent febrile paroxysm. Dr. Ormerod thus remarks with regard to this point:

> "Of 115 cases in which the mode of aggression of fever has been noticed, occurring during the last few years, this was sudden in sixty-four, and gradual in fifty-one cases. The fifty-one cases numbered sixteen deaths, the sixty-four twelve, and this difference is even greater, taking the smaller number of facts which the practice of 1846 supplies. Yet this does not differ from what one would expect; the suddenness of the attack, though it may denote greater violence of the fever, and causes, indeed, sometimes fatal collapse, yet in most cases denotes, in the mode of succession of the early phenomena of disease, greater power of resistance." (p. 39.)

We believe that Dr. Ormerod is substantially right in his explanation of this general fact. It is an undoubted fundamental principle in both pathology and physiology, that living organisms possess within themselves a power—*a vis insita*—which acts in a conservative sense, and protects them from injurious agents. This *vis insita* is active and efficacious in proportion to the perfection of the organism itself, and may be altogether impotent when the vital powers are depressed. It is almost impossible to avoid giving full assent to the doctrine which looks upon fever as the result of an effort of this *vis insita* under the form of a "*vis medicatrix*," instituted for the purpose of eliminating or expelling a poison from the system.

The period during which the system tolerates a morbific poison, is the latent or dormant period, or precursory stage; and this period of tolerance will depend upon the sensibility of the system to the presence of the morbific agent, which will necessarily vary according to the condition of the individual, and the circumstances under which he is placed. There are examples in which, from long habituation, a perfect tolerance is the normal state, and the poison is apparently innoxious; in other cases it would appear as if the poison was eliminated as soon as received, without any reaction being established, and as if it were one of the excreta; these are examples of exemption by idiosyncrasy. In by far the majority of instances of typhus, the moment of reaction is determined by extrinsic circumstances of a depressing kind, as the use of a violent purgative, a fit of indigestion, exposure to cold, &c., and its initiatory stage is ushered in by rigor, or the "cold fit," more or less marked. The influence of external circumstances in determining the accession of fever, is well shown in those cases in which an ague fit appears at distant intervals. At a first glance these fits appear desultory and irregular; but in calculating the periods, it has been found that they recurred on such days as they would have happened on, if a fit had taken place at every regular period; so that, in fact, there is an omission of a number of fits, the organism resisting the poison until a depressing circumstance occurs.

The febrile condition having, however, been established, it has to run its course, until the poison is eliminated or counteracted. This course is marked by the supervention of new phenomena, or by the subsidence of old; and at regular intervals in some classes of fever, as the intermittent and exanthematous. The days on which the symptoms are aggravated, modified, or alleviated, were termed critical days by the ancient physicians; and the term crisis was used to designate the totality of morbid phenomena then accruing. These morbid phenomena were made subservient to prognosis and therapeutics, by a long course of observation of their law of sequence, and enabled the physician to *judge* as to the termination of the fever by death or otherwise, as to the most dangerous period of the fever, as to the symptoms that would arise in the course of the fever, and as to the treatment of the case in each successive stage. This use of the periods of fevers in establishing a *judgment*, led to their being termed *crises* (the Greek word for judgment) and critical. In intermittents, and in smallpox, measles, and other exanthemata, the same use is even now made of them; but as to remittent and continued fevers, much discussion has been raised whether they exhibit critical days at all, or if so, whether with sufficient distinctness for prognosis. A reference to principles is necessary for the elucidation of this point. Dr. Laycock has shown that one of the circumstances which determines the outbreak of a fever, is the menstrual period; and, by a series of facts, he maintains the proposition that this period is a marked example of a periodic revolution which occurs in both sexes, and, indeed, in all animals, made up of lesser cycles or periods. To this principle he refers the determination of the periods of incubation and utero-gestation in the vertebrata and invertebrata, the periods of moulting, the stages of development of organs in the embryo and the young, &c.; and maintains that, in fact, it is this great law of periodicity which regulates the course of fevers. As an example, he points out the latent period of the specific febrile poisons as being limited within the monthly period,* and illustrates his doctrine with examples drawn from the exanthemata, the pathology of gout, and the general facts of pyretology.

To understand the action of these physiological oscillations, it is necessary to remember that they are of a disturbing and even depressing character; and with this fact in recollection, we can readily understand how in the course of a fever there would be alterations in the phenomena at the physiological intervals. By it we can as readily predict the convalescence from variola on the fourteenth day, as the return of the menstrual flow on the twenty-first or twenty-eighth. Indeed, the only difficulty in the application of the law, will be in those fevers in which the period of accession cannot be determined, or in which the periodic oscillations are masked by complications. In simple agues, there is no local inflammation to keep up a constant irritation; consequently, when the febrile reaction terminates, the system is restored to comparative health, and so the periods of reaction are well marked; but if within the tropics the action of the malaria be not checked in the premonitory stage, or if a full dose of the antidote—quinine—do not modify the first accession, and render the disease an intermittent fever, the poison excites inflammation, and not a temporary congestion of the liver, spleen, and stomach; and thus the fever becomes complicated with a local source of febrile phenomena, the powers of the system are further reduced, and the accompanying fever is remittent, continued, or malignant.

The determination of critical days in typhus fever must necessarily be beset with difficulties. In the first place, the numerous examples in which the development of the disease is gradual, renders it impossible to fix a day from which to enumerate; secondly, the compound action of two or more poisons will complicate each other; thirdly, the local inflammations arising from that action, whether of the brain, lungs, skin, or Peyerian glands, will disorder the sequence of morbid phenomena; fourthly, the ordinary inflammations, to which the morbid condition of the blood and system generally in typhus so effectually predisposes, and which so frequently complicate it, constitute another disturbing element; and, fifthly, the febrific poison itself seems less under the control of the vital powers than the malarious or specific poisons, and more continuous in its action. At the same time it cannot be doubted but that in all those cases in which the typhus is exanthematous, the critical days may be determined by a careful observer; and in many cases of ordinary typhus, although the day of commencement cannot be determined, the experienced eye will detect a critical day in the progress of the case; and from that calculate the subsequent crises.

---

* Lancet, vol. i, 1842-3, p. 128.

VI. From all these considerations it must, we think, be evident that the progress of pyretology depends upon an extension of the field of observation and treatment. An hospital is not the place for either. The morbid poison in every recorded case should be traced, as far as possible, to its source; if there be more than one, their compound action should be in like manner analysed, and their results separated from those of intercurrent inflammations, or existing chronic disease. In every recorded case, the phenomena of the latent or premonitory period should be as diligently investigated and noted as those of the disease itself, and the essential phenomena carefully separated from the accidental or complicating. Formerly the exanthemata were confounded with each other; our present knowledge of typhoid fevers is in just the same state.

An hospital, we repeat, is not the place for the treatment of fever, if the considerations we have advanced be allowed to have any weight. The purest atmosphere is necessary to success, seeing that it is the impaired functions of the oxygen-carriers—the blood-corpuscles—which is at the root of the morbid phenomena; nay, it is by no means improbable, that an atmosphere with a slight excess of oxygen would be highly curative. But how can the necessary degree of atmospheric purity and ventilation be attained in the wards of a civic hospital, where the miasm from civic drains and the effluvia of its numerous sick inhabitants are diffused through them? Before all things a pure atmosphere is necessary around fever patients, and a *total change* in their management in this respect is absolutely necessary to ensure the best curative means.

Science can afford us little information as to the sort of medication requisite, except this,—that the instinctive desires excited during an attack of fever will often indicate the best line of treatment. Thus, the hot skin requires cool air; the ardent thirst points out the necessity for diluents *ad libitum;* and the desire, not only for water, but acidulated water and fruits, indicates the necessity of supplying the blood freely with vegetable acids or vinegar, in the primary stages, and with the mineral acids in the more advanced. The great diminution of the appetite for food, and the general want of *natural desires*, point out the need of repose—so delicious to the fevered patient—from all the active instincts.

Medication may do much in the earlier stages, especially in the premonitory. The poison may have its antidote, or its elimination may be facilitated by diaphoresis, diuresis, or purgation. Much experience has been accumulated on this head. The so-called water-cure, by the wet sheet and copious draughts of water, is a combination of diuresis and diaphoresis of a most efficient kind, capable of cutting short a fever in its premonitory stage or at its first accession. Our own experience is decisive upon this point. The diarrhœa of summer and autumn is eliminatory in its effects, inasmuch as the poison is carried off by the intestinal mucous membrane. Sydenham, who thoroughly comprehended the general principles to be observed in the treatment of fevers, although hampered by the prejudices of the age and a want of knowledge of details, on various occasions prescribed a preliminary gentle aperient in diarrhœa and dysentery, and the fevers which accompanied them. He did this that he might imitate and assist Nature. He considered them as all dependent alike upon hot and acrid humours contained in the mass of the blood, directed to the intestines, and expelled from it through the arteries of the abdomen: "Humores calidi atque acres in massa sanguinea contenti, atque eandem exagitantes, per arterias mesaraicas in dictas partes deponuntur." The aperient he administered consisted of tamarinds, senna, manna, and rhubarb, and it was followed up by an opiate. In treating cholera morbus, Sydenham avoided even the slightest purgative, the irritation being already too great:

"Sedula mentis applicatione et multiplici etiam experientia edoctus, quod, si hinc acres istos humores, fomitem morbi, catharticis expellere conarer, idem agerem atque is qui ignem oleo extinguere satagit, cum cathartici vel lenissimi operatio omnia magis perturbaret, et novos insuper excitaret tumultus, &c.

"I have been taught by sedulous meditation and also by extensive experience, that if I should attempt to expel these acrid humours, the *fomites* of the disease, by cathartics, I should be doing the same thing as the man who wearies himself to extinguish fire with oil, seeing that the operation, even of the mildest cathartic, tends rather to increase the disturbance, and moreover excites new commotions."

Dr. Ormerod's cases, illustrative of affections of the bowels in fever, are instructive illustrations of the curative doctrines of Sydenham, and comprise every form of consecutive intestinal disease. In those cases, in which the ordinary sedatives and astringents fail to allay the intestinal diarrhœa and irritation, we have found enemata of di-acetate of lead and acetate of morphia, together with small doses of the nitrate of silver, to be of great service.

We need not notice the special points of treatment detailed by Dr. Ormerod. Every case will present its peculiarities, and these must be treated on general principles. We cannot close this article without a quotation from Sydenham's works on these points, for we are sure our readers, like ourselves, will appreciate the far-seeing sagacity and practical wisdom of that great man.

"Quo pacto indicationes curativas ab hoc vel illo phænomeno minus insigni mutuabitur, naturalem morbi historiam ignorans, (quæ sola veram medendi rationem indigitare poterit,) cum nequeat dijudicare an hoc vel aliud phænomenon a curandi quam adhibet methodo, an ab ipso nascatur morbo? Dies deficeret enumerantem varia et subtiliora illa minutioraque quæ in hoc atque in aliis morbis curandis observanda occurrunt; quæ cum tot sint et tanti momenti ad liberandos a morte homines, semper licebit posteris et seris nepotibus aliquid adjicere iis observationibus, quibus traditur varietas pene infinita operationum Naturæ in morborum productione, et curativæ indicationes ex eo fonte depromendæ enumerantur. (*Epistolæ, Responsoriæ duæ.* Sydenham Society's edition, p. 297.)

"By what means will one, ignorant of the natural history of the disease, (which alone can indicate the correct mode of treatment,) collect the curative indications from this or that less prominent symptom, seeing that he is unable to determine whether these symptoms arise from the remedies which he is administering, or from the disease itself? Time would fail to enumerate the various and more subtile and minute particulars which call for observation in this and other diseases; and, inasmuch as they are so numerous and of such great importance towards the conservation of patients, there will always be room for our successors and remote posterity to add something to these observations. By these the almost infinite variety of the operations of Nature in the production of diseases is ascertained, and from this source the curative indications must be drawn."

Art. VI.

A Treatise on Diet and Regimen. By William Henry Robertson, m.d., Physician to the Buxton Bath Charity. Fourth Edition, rewritten and much enlarged. Two Volumes.—*London,* 1847-8. Post 8vo, pp. 718.

That the sources of information, from which valuable data may be gleaned for the construction of a definite and satisfactory code of dietetic rules, have become far more numerous and trustworthy of late years, than they have been at any former period, is a fact of which our readers must be well aware. It will not surprise them, therefore, to be told, that Dr. Robertson has found himself obliged to rewrite his work, and to more than double the quantity of matter contained in it, since its first appearance twelve years ago. Its character, too, has been changed. It was originally styled a "Popular Treatise;" the "popular" is now dropped, and it evidently aspires to a higher scientific character, without, however, being raised above the comprehension of "the Invalid and the Dyspeptic," for whose particular use it was originally designed. We shall accordingly treat it as a new work, and bring its contents *seriatim* under the notice of our readers.

The work opens with an Introductory Chapter on the Organic Laws, and on the causes of disease arising from violations of those laws. These are classified under three heads:—1st, as they arise from the acts of the individual, entailing upon him, and perhaps on his posterity, a certain set of consequences, in which others are not directly involved; as when intemperance produces disease of the liver;—2d, those which are produced by the acts or neglects of a combination of individuals, involving injury to the health, and life-probability, of the body politic; as when fever is produced by the imperfect drainage of a town or district;—and 3d, those which are still more general, chiefly depending upon natural phenomena, as atmospheric conditions and the like; to which the non-contagious epidemic diseases must be referred. But these three classes of causes, as Dr. Robertson justly remarks, are continually acting together; and, although the separation is well adapted for the formal discussion of the subject, especially when considered in its practical bearings, it must be admitted to be purely artificial as regards the *modus operandi* of morbific causes upon the bodily system. As *individual* hygiene is the principal subject of the treatise, and as the agency of atmospheric causes does not properly fall within its scope, the greater part of the introductory chapter is occupied with a general sketch of *social* hygiene, which is clearly and judiciously written, and may be studied with profit even by those who have themselves given much attention to the subjects it embraces. The following is an interesting extract from the portion relating to those endemic causes of disease, which are connected with the condition of the surface-soil and of the superincumbent atmosphere:

"The expectation of life in America is becoming greater, and its population more vigorous and healthy, in the same ratio as its surface is becoming more extensively inhabited, its soil more thoroughly drained, and its air cleared of the impurities that exhale from a wet and corrupt surface-soil, charged with organic remains, which are exposed and not immediately wanted by the existing vegetation. It thus happens that the climate of a newly-peopled country, hitherto in great degree covered with wood, or rankly-grassed prairie, or undrained and in a great degree submerged morass, becomes, in the first instance, so much less healthy as the forest-land is cleared, the morasses partially drained, and all the miasmatous exhalations multiplied; becomes less healthy in the same proportion as the woodland is robbed of its trees, and dense brushwood and grasses,—and the morasses are less entirely covered with water; and the miasms are rendered by so much more hurtful to animal existence, and by consequence to the health of man, as they are less changed and purified by the agency of a redundant vegetable life. Thus, the land may have been originally by no means, or to no great extent, injurious to the health of its few and widely-scattered savage population; and yet speedily become greatly prejudicial to the health of its first civilized settlers, who clear its forests, and begin to cultivate its soil. This, however, will soon cease to be the case, as its surface gets to be better drained, as the land is brought more under tillage and under grass crops; and the probability will be, that, from having been at first so greatly deteriorated as a place of human residence, it will become eventually more healthy, and have a longer-lived population, than when it was an uncultivated hunting-ground for barbarous denizens. The medical histories of different districts of America have proved, and are now proving, the truth of these statements." (p. 7.)

Without denying the propagation of continued fever of the typhoid type by contagion or infection, Dr. Robertson unhesitatingly adopts the now fashionable creed, that it may *originate* in bad drainage, insufficient ventilation, &c. We shall not here enter upon the discussion of this question; indeed, we doubt if any discussion of it can lead to its final settlement, or need have much bearing on the practical measures of which the importance is admitted on all hands. For, with the knowledge we possess regarding the possible prolongation of the latent period of morbid poisons, during weeks, months, or years, it seems impossible to prove that any case of fever, however apparently isolated, *may* not have had a contagious origin. On the other hand, the most ultra-contagionist must freely admit the powerful influence of bad drainage, insufficient ventilation, &c., in promoting the spread of fever, by predisposing the system to be acted on by the morbific agent; and may even go so far as to allow that, if these predisposing causes could be altogether removed, the disease would no longer be propagated, and would die a natural death. There is much truth, then, in Dr. Robertson's remark, that "fever hospitals are, in themselves, public demonstrations of a neglect of those means by which the disease might be in great degree, if not altogether, put an end to." He urgently calls upon medical men to busy themselves more in enlightening the public upon the proved and admitted causes of disease; and we agree with him in the conviction that they cannot be employed more honorably, nor with a greater certainty of beneficial results.

"The people in general do not and cannot know, that the occurrence of a case of fever, not obviously dependent on some localised inflammation of adequate severity, is *primâ facie* proof that there is in the district some removable source of atmospherical impurity, which,—although possibly producing only one case of fever, or only a few cases of fever,—and although perhaps attracting little notice, and not affecting under the name of fever the tables of mortality, or even adding directly to the local rates under the guise of widowhood or orphanage,—gives rise, necessarily, to numberless cases of lesser disease, amounting to fever only in those who have been predisposed to suffer largely from the operation of such causes, owing to debility or functional derangement, however induced, or in those who have been most exposed to the miasmatous matter, which has so far poisoned the air. Such minor disordered conditions may be visceral derangement, dyspepsia, irregularities of the circulation, or any of the countless forms of *malaise*, that render man less fitted for the duties of his calling, if not temporarily incapacitated from following it at all. People do not and cannot know such facts as these; or nothing less than a hue and cry would be raised on the occurrence of fever; the sources of impurity would be anxiously investigated; and every matter for suspicion would be promptly and carefully removed. It cannot be believed that the public mind is aroused to the vast and vital consequences of ignorance and inattention to these questions; and it is only to the members of the medical profession, that attention can be turned with any hope of witnessing speedily a more enlightened state of things." (p. 26.)

If a tithe of the attention that has been bestowed by the profession upon the disease itself, had been devoted to the investigation and exposition of its causes, we should not be where we are now,—at the very commencement of sanitary legislation.

In Chapter II, our author enters upon the subject of Diet; commencing with an inquiry into the nature of the digestive process. He abstains from discussing the merits of the more recent investigations into its nature; contenting himself with the mixed view of the solvent action of the gastric fluid, which has always appeared most satisfactory to ourselves, and which none of these investigations seem to us to have disproved,—viz. that the acid is the real solvent, the use of the peculiar animal principle, which seems to act as a *ferment*, being to predispose to the solution of the alimentary matter, by the incipient change which it excites in the particles of the latter. The nature of the acid, however, must still be regarded as doubtful; the difficulties in the way of an exact determination of the question being such, as to have hitherto prevented the desiderated accordance amongst those who have investigated it.

"A much more practically interesting result, derived from the numerous and anxious investigations into these questions, and one that seems to be proved, is, that an animal matter contained in the saliva, and separable from it, when mixed with alkali, proves to be the solvent and digestive medium of starch, upon which acidulated animal matter has no action,—and that either this same animal matter, or some similarly constituted animal matter, when mixed with the acid secretion of the stomach, proves to be the proper solvent for the azotised articles of food, such as fibrin, gelatin, and albumen, having no power to dissolve or assimilate the non-azotised aliments." (p. 35.)

We are much surprised to find Dr. Robertson espousing Dr. Prout's theory of the "vital principle" or "organizing agent" as one of the instruments in the digestive operation. "In all the views and reasonings," he says, "which greater researches and further information may seem to justify, the true vital character of the digestive functions must be always set forth, or truth and usefulness must be equally forfeited." We cannot see the least ground for the assumption, that any change which the food undergoes in the alimentary canal, can be regarded as a "vital" process. The alimentary materials enter the stomach in the condition of dead matter, and they leave it in the same condition, their form only being changed. No properties other than those of a purely chemical solution—none, therefore, that can be characterised as vital,—can be discovered in the chyme; there is no reason to suppose, therefore, that vital powers have been at work in preparing this solution, in any other way than as they are concerned in the secretion of the solvent from the blood. The food whilst merely contained within the digestive cavity, is as much *outside* the living body, as it would be if lying in contact with the cutaneous surface, of which the mucous membrane of the alimentary canal is an inversion: it is in the same relative position with the nutrient materials lying around the roots of plants. It is only when absorbed by the blood-vessels or lacteals, and subjected to the agency of living tissues, that the selected fluid begins to manifest, in its incipient coagulability, the direct influence of the vitality of the system in doing that which no mere physical agency could effect. The assertion (p. 41) that "the peptic functions of the stomach require, for their performance, a large amount of vital or nervous power," appears to us equally gratuitous, although we are aware that it is in accordance with current belief. But since the experiments of Dr. John Reid have shown, that the digestive process will go on pretty nearly or quite as well *after* section of the par vagum as *before*, when the first shock of the operation has passed away, we cannot see how it can be any longer sustained upon a valid foundation.

We feel called upon also to notice an error of fact, into which we are surprised that a writer of Dr. Robertson's intelligence should have fallen. He says (p. 39), that "liquids, when taken alone into the stomach, unmixed with solid food, are in great degree absorbed by the veins and absorbents, which open on the lining membrane of the viscus." Surely he cannot be ignorant that neither veins nor absorbents have mouths opening on the lining membrane of the alimentary canal; and we know of no ground for the assertion, that the absorbents of the stomach, which are purely interstitial and do not commence in villi,—being lymphatics, not lacteals,—have any active participation in the absorption of liquids from its cavity.

Dr. Robertson's practical rules appear to us to be everywhere more valuable than his scientific expositions. In the following extract he urges the importance of attending to indigestion in every form and degree, as one of the most important of the connecting links by which external causes of disease are enabled to influence the system.

> "A man in the full vigour of mind and body, with his stomach's functions unimpaired and underanged, may probably be exposed with impunity to circumstances that would lead to inflammatory and other morbid actions, under less healthy conditions. The great morbific agents, cold and wet, may repeatedly find him securely fortified against their influence, until a breach has been made by stomach derangement,—the energies of the system, diverted by local irritation from the main duty of keeping up the temperature of the surface, may prove to be unequal to resist the external depressing influence,—the surface may become chilled,—the equal distribution of blood be interfered with,—internal congestions of blood established,—a febrile state of the heart's action, and a general irritation of the nervous powers, be produced, to combat with, and if possible remove, the local determination and congestion,—and, unable to accomplish this, or being greater than is needed for the restoration of health, become, either in the one way or the other, causes of increased disturbance to the economy,—ending, possibly, in inflammatory action of some of the great and vital organs of the body." (p. 52.)

That, when a morbid condition of the system has been once established, the regulation of the diet is a matter of still greater importance, our readers need scarcely be told; yet we feel confident that this is a point on which medical practitioners in general are far from laying sufficient stress.—Dr. Robertson very justly remarks, that this subject must occupy a more important position in any system of hygiene or of medicine, the more artificial, the farther removed from a state of barbarous labour, the state of mankind may be.

> "The more fully the physical conditions of life and health are fulfilled, the less needful must be the regulation of the diet, either as to its quantity or quality. If a man is to breathe the fresh and unvitiated open air during the greater part of his working hours,—if he is to labour daily and severely for his subsistence,—if the body is used so much, and the mind comparatively little,—attention to diet may be of little moment, even if any variety or abundance of appetising viands were by possibility attainable, which, under such circumstances, would be most unlikely. But if a man is to spend his life very differently,—shut up in the closer air of a town, passing the great majority of his hours within doors, and possibly in an apartment where the direct rays of the sun seldom or never enter, and which may be very imperfectly ventilated,—engaged in an occupation that confines him to his chair, or limits his bodily exertion to an occasional and uniform change of position, or to a series of slight muscular movements, and calling comparatively few muscles into exercise,—and if to this be added much wear and tear of mind, whether in thought or anxiety,—the question of diet must become of primary importance, even although, notwithstanding any amount of attention to it, the most that can be hoped for in the case, may be to diminish the amount and severity of the dyspeptic symptoms, and to maintain the man's system in a state nearer to that of health than it otherwise could be." (p. 54.)

The chief alimentary principles are then noticed, in their chemical relations to the tissues of the body; and here again we feel obliged to notice another extraordinary oversight of our author's, which we can scarcely set down to the account of ignorance. After mentioning that the fat and water of the system are destitute of nitrogen, he continues,—"The other organs and textures, according to Professor Liebig, and most other modern chemists, are all, with one exception, reducible to a substance having a very similar composition and properties, whencesoever obtained; and this substance, from presenting a primitive or simple type of a compound, from which all the various tissues and organs may be supposed to be formed, has been called protein. To this general rule, gelatin furnishes the important exception." (p. 57.) To what category, then, does Dr. Robertson refer the nervous tissue? Surely he does not mean it to be included in the designation of "fat and water?" On the other hand, the assertion, that it is derived from any protein-compound, is purely hypothetical. It is easy enough to make this conversion on paper, by taking away a certain number of atoms of one element, and adding a certain number of the other. But what proof have we that protein is ever metamorphosed into neurine? It is even doubtful whether the matter contained within the cells and tubes of the nervous tissue really contains any nitrogen; the small proportion which it was formerly considered to include being now supposed, with much probability, to be a constituent of the *walls* of the cells and tubes, not of their contents. Our present knowledge of the composition of neurine would lead us to regard it as a peculiar phosphorised fat, and therefore to refer its origin to the fatty matters taken in as food, or produced by the metamorphosis of other constituents. Assuredly it can never be reconverted into protein, whether or not it was originally derived from it.—In other respects, also, our author's statement is too general and imperfect. When adopting the views of Liebig, that the gelatinous aliments cannot be applied to the nutrition of the albuminous or protein-tissues, he does not mention the fact, of which there is ample evidence, that the gelatinous tissues may be generated from albumen; so that gelatin, however valuable, can never be regarded as an indispensable article of diet. Nor does he tell us that there are many other tissues,—for instance, the yellow elastic fibre,—which, though first elaborated from protein-compounds, cannot be reconverted to that form by any chemical processes with which we are acquainted; standing in this respect upon precisely the same footing with gelatin.

In commenting upon the well-known experiments of Dr. Beaumont, our author makes some very judicious remarks upon the fallacy of any inferences drawn from the mere digestibility or solubility of an alimentary substance, and its aptness for nutrition; and also upon the very little light which is afforded on the first of these points by a knowledge of the chemical composition of the substance in question. Thus, as a general rule, farinaceous substances are much more easily and quickly converted into chyme than any other articles of ordinary aliment; rice being dissolved at least twice as rapidly as meat; but the relative nutritious powers of the two are in an inverse proportion. On the other hand, turnips require a much longer time for their solution than rice; but their nutritive power, as indicated by the amount of azotised matter they contain, is far lower than that of rice. Further, chemistry finds very little difference in composition between gum and sugar, their relative digestibility being very different; or between fat and alcohol, although the comparative readiness with which they are taken into the system is still more different. Hence, then,—

> "In the question of the comparative digestibility of the different articles of food, we are reduced to the necessity of finding general rules for our guidance in individual cases, from wide and careful observations of the digestibility of different kinds of food, in health and disease; by no means conceiving such observations, however carefully made, to be infallible; but bearing ever in mind the probability, or at least the possibility, of frequent exceptions occurring, to any one of the generalizations at which we may have arrived. In fact, all attempts to frame such general rules are liable to strictures, on the ground of so many and signal exceptions to the rule being frequently met with. But, notwithstanding such strictures, dietetic observations are of the greatest use as guides; although they may not be found worthy of implicit trust, in every detail of every case that may occur. To prove that such observations must have much value, it is enough to show from general experience and universal admission, that some articles of food are almost invariably found to be less likely to agree with the debilitated stomach than others are." (p. 80.)

The true bearing of our present knowledge of chemistry upon dietetics is very well indicated further on.

> "It is only in the general principles, and not in the minuter details, that chemical experiments have assisted dietetic inquiry. If an article of food does not contain azote, it may be inferred that it can only minister to the respiratory process; and in many instances, as in the case of fat, and in that of milk and its different products, this serves a very important purpose in estimating the dietetic uses and value; but such experimental results do not enable us to arrive at any definite conclusions, as to the amount of respiratory or nutrient matter which the digestive organs may extract and assimilate from any dietetic substances; although, if all other circumstances are the same, it may assist us in arriving at a probable conclusion. For instance, it is certainly true that, in man, and *a fortiori* in strictly carnivorous animals, animal food is capable of affording a larger amount of nutrimentary matter to the system than vegetable food. No statement that bean-meal or linseed-meal contains more albuminous matter than beef or mutton, would justify

an inference that they are therefore more nutritive articles for the food of man. The character and peculiarities of the organs of assimilation must be taken into the account; and the results of chemical experiments, the peculiar nature of the digestive organs, and general experience and observation must serve to confirm and strengthen one another; or, in a practical point of view, the researches of the chemist are of little value in dietetic details." (p. 142.)

The greater part of the first volume is occupied with a statement of the results of scientific investigations and practical observations on the value of different articles of diet, and their suitableness to the wants of the system in the various conditions of health and disease. Our author's conclusions upon these subjects appear to us on the whole correct and judicious, though perhaps liable in some instances to the charge of over-refinement. We shall not follow him through the details of this part of his treatise; but shall extract some of the remarks which we have noted as most original or discriminative. After noticing the indigestibility of pork as compared with other meats, Dr. Robertson goes on to say that,—

> "Much, however, as to the degree of digestibility of pork depends upon the feeding of the animal, and the other circumstances of its life. If pigs are fed on tainted refuse, or fish, or animal food of any kind, even very partially,—if they are not duly supplied with vegetable and farinaceous food from the first,—it is vain to expect that the flesh will be well developed and tender, and as easily digested as it is capable of being rendered. To this may be added, that the health of all animals, and perhaps especially of the pig, depends very much on the cleanly condition of the skin,—the due ventilation, drainage, and purity of the sty,—and the having a sufficient degree of exercise. There is no doubt that the system of shutting up pigs, in small, ill-ventilated, imperfectly-cleaned styes, which is so prevalent in England, makes pork much less wholesome and less digestible than it might be;—or that the flesh of the wild animal is more easily digested than that of the tame animal, and probably for this reason chiefly. Until the ventilation and drainage of stalls and styes are properly and sedulously attended to, and considered to be of first importance,—until due exercise of the animal be permitted, and, if need be, enforced,—enough has not been done to render the flesh as perfect and digestible as it may be; and these matters are of an importance only secondary to the due, careful, and wholesome feeding of the animal throughout life, and putting it, as far as circumstances permit, into the natural conditions of existence, or those which would obtain in the wild state." (p. 86.)

There is ample evidence of the justice of these statements. It is well known that the pork and bacon of those counties in England, in which circumstances favour the rearing of swine in habits of comparative activity and cleanliness,—e. g. Hampshire and Wiltshire,—are more esteemed than those of other localities, in which stye-feeding prevails. And we have been assured by those who have tried the experiment, that, where the latter system is practised, attention to cleanliness is always accompanied with all the improvement that could be expected in the general condition of the animals. We have ourselves eaten pork which was almost as delicate in flavour and as free from grossness as veal; and there is no reason why this improvement should not be general, save the prevalence of the notion that filth is as natural and healthy to the porcine race, as cleanliness is to most other animals.

When speaking of the diet of the young, it is justly remarked by Dr. Robertson, that an acquaintance with chemical facts is of great importance in reference to this question; and that proof is continually being afforded how great is the danger, and how serious are the consequences, of ignorance on the part of those who have the charge of the physical training of children.

> "It is not to be doubted that many children have been atrophied, and probably lost, from their food being confined to such articles of diet, as starch, sugar, butter, and perhaps it may be added, gelatin: substances incapable of doing more than minister to the respiratory functions, or renew the gelatinous tissues, and not fitted to repair the general waste and expenditure of the body. It should be impressed on the minds of parents and nurses, that, whereas one great danger arises from overloading the systems of children, and so deranging their susceptible and easily-disturbed economy, there is a great danger of possible occurrence,—that of not giving them such food as contains a sufficiency of nitrogen to meet the wants of the system, and maintain its health and power. Children are often found to do well upon a minimum amount of azotised food, until the periods of the first or the second dentition, when the extraordinary call upon the system for the development of the roots of the teeth, and the consequent protrusion of the teeth through the gums, is not adequately met, or only ministered to at the expense of the rest of the body,—and dentition becomes slow and defective, and the health evidently impaired. The possibility of an extra quantity of fibrinous aliment,—or such aliment as is reducible to protein out of the body, and by consequence convertible into organ and tissue within it,—being required at this period of life, should be borne in mind. There is, moreover, as great a risk of children being supplied with an undue quantity of simply carbonaceous food, as of their being furnished with too scanty a proportion of azotised food. The consequence of this error, whether in the years of growth, or in after life, must be to surcharge the system with carbon,—to diminish unwisely the instinct and necessity for muscular exercise, by supplying without its intervention the whole of the carbon wanted for respiration,—to render the system torpid,—to overwork the great emunctory of carbon, the liver,—to derange its great processes,—to render the bile less fitted for its important duties,—and, finally, to disturb the digestion, and thus affect the healthiness of the whole economy." (p. 113.)

There can be little question as to the *facts* thus set forth, although we may not altogether coincide with the theoretical explanation of them given by Dr. Robertson. Thus we cannot see that any large amount of fibrinous nutriment can be required for the development of the roots of the teeth, which are tissues that do not undergo continual renovation, but are sufficiently durable when once formed; the substance peculiarly required for this purpose will be phosphate of lime, since a single meal would supply all the fibrin that the whole set of teeth can require. We should refer the demand, of which our author speaks, to the requirements of the general system at the periods in question. Thus the time of the first dentition is that at which the infant is passing from a state of bodily inactivity into one of energetic and restless movement; from lying in its cot or its nurse's arms, or sprawling upon the floor, it begins first to crawl, then to walk, and lastly to run; the consequence must necessarily be, that the *waste* of the muscular tissue will then take place with greatly increased activity, and a much larger supply of fibrinous aliment will be requisite to repair it. The period of the second dentition is that at which childhood is passing into youth; the body is rapidly growing; the muscular system, in particular, is undergoing a great increase of development; there is a disposition to exertion of a much more violent kind; the mind, too, is much more vigorously exercised; and if all this be not adequately sustained by appropriate nutriment, serious mischief must ensue.

The remarks we have just quoted are followed by some very appropriate and philosophical reflections on the advantages which civilized man has gained from the due adjustment of the proportions of azotised and non-azotised articles in his diet. Starting from the principle enunciated by Liebig, that where the food contains an insufficiency of hydro-carbon for the sustenance of the animal heat, the maintenance of the temperature of the body is dependent upon the disintegration or waste of the azotised tissues, and that this can only be brought about by continual exercise, to which there is under these circumstances an instinctive impulse, he continues:—

> "Thus, step by step, has man, in his successive generations, earned for himself the power of maintaining his elevated temperature, with longer intervals of repose and quiet,—the power of making his body subserve more and more the purposes of its great tenant,—the power of devoting life to intellectual pursuits, of expanding and elevating the nobler capabilities of his nature. It is indeed curious, and an abundant cause for wonder and gratitude, to contrast the barbarous inhabitant of the colder latitudes, living chiefly on the animals he might slaughter in the chase, half clothed, and coldly housed,—passing all his waking hours in constant exertion, to procure the precarious means of sustenance; and, even if the chance slaughter of some larger animal set him temporarily above the immediate pressure of want for some days or weeks, nevertheless compelling him, by his instincts, to continue his days of toil, for the maintenance of his bodily heat, by the adequate expenditure of tissue to furnish the carbon requisite for the consumption of the respired oxygen;—to contrast such a slave to the body's necessities, with civilized man, raised above the pressure of want by agriculture,—sated with variety by commerce,—the temperature of his body largely preserved by abundant clothing and well-warmed houses,—his respiratory carbon supplied adequately by vegetable food,—his need of muscular exercise reduced to the question of expending what is necessary for the tissues, to make way for their timely repair and renewal,—and the large balance of his days of life left at his disposal, for the cultivation of his intellect, and the enlargement of his mental and physical resources, and the placing himself and his fellow-men on still higher vantage-ground, for the elevation and improvement of the moral and intellectual nature." (p. 115.)

Dr. Robertson lays much and deserved stress on the principle, that the digestion and assimilation of pure and concentrated alimentary principles, whether animal or vegetable, are much more difficult than that of the mixtures in which these principles are naturally combined. It may be difficult to assign a reason for this; but the principle is not the less certain, not merely as resting upon the well-known results of experiments made for the purpose, but also as being in accordance with universal every-day experience. Thus in the case of starch, simple and little irritating as it usually is, and readily enough digested, the pure starch obtained from wheat-flour is not by any means so easily assimilated as arrow-root, in which it is mixed with a small quantity of albuminous matter; and even arrow-root is often found to be less easily digested, by people of very weak stomachs, than the farina of many of the grains in which the starch is more divided, and combined with a larger proportion of other vegetable principles. So again, casein and butter, which are generally sufficiently digestible when in the natural, weak, and mixed state of milk, are with much more difficulty assimilated when separated from it. The separated vegetable acids, moreover, are much more likely to derange the digestive organs, than the same acids when in combination with other matters in the fruits, &c. from which they are obtained. And the same principle is applied by Dr. Robertson to explain the long observed difference in the effects of their long-continued use upon the body, between the mixed and less pure forms of alcohol as existing in wine and beer, and the purer and stronger alcohol obtained by distillation.

There can be no doubt that a great modification is required, to bring our practice in regard to the use of amylaceous articles of food, especially in the case of invalids, into accordance with theory; and we have strong reason to believe that this modification would be productive of very beneficial results, the apparently innocuous qualities of starchy compounds having led to a very erroneous estimate of their value. On this subject our author justly remarks:—

> "The different forms of starch made use of as food, although useful from proving so little irritating to the organs of assimilation during their passage through the stomach and bowels,—and even so little irritating in some cases, where they prove to be in great degree indigestible, passing through the bowels and out of the system without having undergone any appreciable degree of change,—are not to be considered as being digestible in the same proportion as they prove to be little irritating; nor is their occasioning little or no sensible inconvenience or dyspeptic symptoms, to be considered as in any degree marking their suitableness to the assimilating powers of the digestive organs, or their capability of being converted into respiratory carbon, or into the adipose tissue of the system. I cannot help believing that this has been a frequent source of error. There is no doubt, that all the forms of the amylaceous principle, if capable of being digested, are largely useful to the economy, in supplying the carbon required for the purposes of respiration, —and in supplying the adipose matter of the body, which is essential for so many of its wants and processes, and is capable besides of proving a reservoir for the respiratory carbon, into which it may at any time be converted, to meet the wants of the economy. The whole question, of course, rests on the non-azotised articles of food being digested; and the probabilities of this are less, in the same proportion as they are pure,—unmixed with the azotised articles of diet. To this, in the case of the saccharine variety of the amylaceous principle, has to be added their directly irritating effect on the *primæ viæ*, if the assimilating functions are not equal to their conversion into chyle." (p. 229.)

We are glad to find that, contrary to the doctrines of some modern diet-doctors, our author approves of the moderate use of tea and coffee; that he considers a cup of hot coffee preferable to spirits for "keeping out the cold;" and that he believes in the beneficial effect of tea as a promoter of the later stages of the digestive process, when taken three or four hours after the principal meal of the day. But, as he justly remarks, tea is injurious to the system, not merely when taken in positive excess, but when taken in excess relatively to the more substantial kinds of aliment, as it very commonly is by the lower classes. There are doubtless idiosyncrasies, however, in respect to tea, as to other articles of diet. We have met with a gentleman upon whom *green* tea, taken in very small quantities, has a peculiar *calmative* and *soporific* effect; tranquillizing his mind when harassed, and procuring sleep when he would otherwise be wakeful. And although he has sometimes had recourse to it for these purposes three or four times in a day, besides making it a part of his regular morning and evening meal, he has never found himself obliged to increase the dose; the same very moderate allowance answering his purpose as well now as it did more than thirty years ago. This gentleman, we may mention, is the master of a public school, enjoys excellent health, has a particularly comfortable placid aspect, and never touches fermented liquors. On the other hand, some cases have been lately published in an American Journal, in which there appeared to be pretty clear proof that to the use of tea may occasionally be laid many anomalous symptoms of nervous disorder, as well as gastric derangement. The following remarks may be appositely introduced, as supplementary to those which we have already quoted (p. 109) in reference to the modifications in diet that progressively become appropriate to the changed conditions of civilized life:

> "As civilization advances, the man of wealth and rank uses personal exercise less, whether in walking or on horseback, and prefers the luxurious carriage as a means of transporting himself from place to place;—keeping pace with the progress of civilization, is the number of thinking and the studious increased; a class of men which is proverbially, and with few exceptions, sedentary;—tantamount to the increased number and importance of our commercial relations, is a larger number of men drawn from the fields and the health-fraught toils of agriculture, into the pent-up and close atmosphere of a town, and have their time occupied in sedentary, or almost sedentary employment; and in these ways there has arisen a daily increasing number of all classes, who, taking less exercise, could bear less food, could assimilate, consistently with health, a less amount of nutriment; who could not eat, with impunity, the meat and beer breakfasts, the heavy substantial food, to which their fathers have been accustomed; and, as if to meet this, tea and coffee have been introduced, and supply the desideratum; a diet which is palatable, only moderately nutritious, and, if not abused, quite harmless." (p. 251.)

We shall now make a few remarks upon the portion of Dr. Robertson's treatise which relates to the use of alcoholic liquors; a question on which we deem it of vital importance that the public should be rightly instructed. The first notice on the subject which we meet with, is under the head of coffee, and is as follows:

> "The practice—probably still a common one—of 'taking a drachm' of some kind of spirits before exposure to cold, is at once foolish and dangerous. The stimulating effect of the spirits soon goes off, and is followed by a degree of languor proportioned to the amount of stimulation. This is the state in which the body is most easily chilled, and the secretion of the skin most easily checked,—in which an individual is most liable to 'take cold;' and if exposed to the depressing influence of cold under such circumstances—after the stimulating effects have subsided,—the chances are very strongly in favour of the system suffering from it. Spirits ought not to be taken before such exposure; unless the person is to be so exposed only for a very short time, or unless the dose is to be repeated as often as the effects of the previous dose begin to subside." (p. 249.)

To the truth of the first portion of this statement we most unhesitatingly subscribe; but we cannot sanction the reservation with which it concludes, because this seems to intimate that if the dose of alcohol *is* repeated, as often as the effects of the previous dose begin to subside, it may exert a beneficial influence in assisting the body to resist cold,—a proposition which is pointedly and completely contradicted by the teachings of the most trying experience. We have before us a letter written by Sir John Richardson (whose authority on this subject must be admitted to be of the greatest weight) not long before his departure on his present gallant and, we trust, successful search for the lost expedition; in which letter he expresses himself as follows:

> "I am quite satisfied that spirituous liquors, though they give a temporary stimulus, diminish the power of resisting cold. Plenty of food and sound digestion are the best sources of heat; and a Canadian with seven or eight pounds of good

> beef or venison in his stomach will resist the greatest degree of natural cold, in the open air, and thinly clad, if there be not a strong wind. We found on our northern journey that tea was much more refreshing than wine or spirits, which we soon ceased to care for, while the craving for the tea increased. Liebig, I believe, considers that spirits are necessary to northern nations, to diminish the waste of the solids of the body, and that tea is less useful; but my experience leads me to a contrary conclusion. The Hudson's Bay Company have for many years entirely excluded spirits from the fur countries to the north, over which they had exclusive control; to the great improvement of the health and morals of their Canadian servants and the Indian tribes."

We have had personal assurance to the same effect from Dr. Richard King, who accompanied Sir George Back in his overland expedition in search of Sir John Ross; and he further mentioned that whereas he had been previously accustomed to reject every particle of fat, owing to the dislike he felt for it, he found himself able, during his Arctic journey, to eat any amount of it with relish, and even experienced a positive craving for it. Everything seems to point to the oleaginous articles of diet as those best adapted to sustain the animal temperature; the disposition to employ them, and the power of assimilating them, increasing with the demand for the generation of heat.—We shall add one more testimony to the same effect. Not long since, we fell in with an intelligent man of above seventy years of age, who had spent more than fifty winters as a fowler on the southern coast of England. It is well known that this pursuit is more successful in proportion to the severity of the weather; a larger number of birds being driven to the south, the more intense the cold of winter. Our informant had frequently been out for a fortnight at a time, without lying down, save in his boat, and scarcely ever obtaining warmth from a fire; and notwithstanding this severe exposure, he is a remarkably hale and vigorous man for his years. Being himself the proprietor of a public-house, he cannot be supposed to have any indisposition to the use of fermented liquors, in which he indulges in moderation; but his very explicit testimony to us was, that although the use of ale or brandy might seem beneficial in causing the cold to be less felt at first, so that, when out for a day or two, he did not think it necessary to abstain from it, the case was quite reversed when the duration of the exposure was prolonged, the cold being then more severely felt, the larger was the proportion of fermented liquors taken. And he further stated, that all the fowlers who had been accustomed to employ brandy with any freedom, when out on prolonged expeditions, had died early.—It is surely impossible, in the face of such testimonies, to maintain the position that alcoholic libations, however frequently renewed, can be of any real efficacy in aiding the body to resist cold.

Further on, we meet with the following passage:

> "The quantity of nourishment obtainable from wine and malt-liquor, is a curious, intricate, and interesting subject of inquiry. There can be no doubt, that the amount of nutriment these several liquids afford to the economy, bears no necessary proportion to the per centage of alcohol they contain; nor even, as far as we can judge, to that of the saccharine, azotised, or carbonaceous ingredients, that are contained in them. Whether or not this may be explicable, on the principle of the nutrimentary and the stimulating matters being so essentially combined, that they are taken more readily into the system, and with less expense to the assimilating organs, than other forms of alimentary matter,—there can be no dispute, that malt-

liquor, and wine do afford an amount of nutriment to the system, that is greater than a knowledge of their composition could have led us to infer. But, this having been granted, it must, at the same time, be allowed, that the chief use of any or all of the products of fermentation is, to rouse the circulation into more energetic action," &c. &c. (p. 266)—

In other words, to operate as *stimulants* to the various nutritive operations.

Now we cannot but wish that Dr. Robertson, instead of taking the *nutritive* powers of wine and malt-liquor so completely for granted, as facts beyond dispute, had favoured us with some evidence in support of his position. He cannot but remember the old story of Dr. Franklin, who, when working as a journeyman in a London printing-office, so far anticipated the doctrines of modern teetotallers, as to arrive at the conclusion that a slice of bread must contain more nutriment than a pint of porter; and by acting upon that conclusion, not only saved his money, but kept his system in a degree of vigour which rendered the "American aquatic," as he was nicknamed, the envy of his fellow-workmen, and which induced many of them to follow his example. The experience of tens of thousands is every day confirming the assertion, that where there is a demand for active nutrition, arising out of vigorous bodily exertion, such demand is much better met by a well-adjusted supply of wholesome alimentary materials, than by any combination of alcoholic stimulants with these. The healthy body, in all ordinary and most extraordinary conditions, is well able to assimilate all the nutriment it requires; and it is only when there is some considerable departure from the natural state, either in congenital predisposition, or in habit of life, that recourse can be had to alcoholic beverages with even a show of justification. For we have seen that even in supplying combustible matter for the heat-producing process, they are inferior to solid aliment; and we cannot understand what claim can possibly be set up in their behalf, as supplying the place of azotised nutriment for the regeneration of the tissues. The amount of solid matter contained in wine is extremely small, and consists chiefly of the vegetable acids, either free or in combination with potash, sugar, and colouring matter, sometimes with a minute proportion of tannin and resino-mucilaginous extractive; we cannot understand how this can by any possibility afford any nutriment to the system, that will not be much better supplied by solid aliment. And although the proportion of solid matter contained in malt-liquors is generally greater, we can see no reason why it should not be just as efficient as a material for tissue, when directly derived from the unaltered grain. The *stimulant* effects of these liquors, whereby the system is urged to more energetic action, and to more rapid consumption of the aliments taken in conjunction with them, are continually mistaken, as it seems to us, for the proper *nutritive* action.

We are glad to find that Dr. Robertson urges abstinence from fermented liquors on young people; believing that they are more likely to grow up into healthy men and women, if these stimulants are not habitually employed. He makes an exception, however, in favour of "some cases of scrofulous or feeble children." Now we do not say that there should be no exceptions of this kind; but we think that, in general, more permanent advantage will be derived from attention to the general regimen, from fresh air, exercise, and nutritious diet,—in other words, from putting the system into the most favorable *natural* conditions possible,—than from recourse to any *artificial* provocatives whatever. And at a time when the constitution is being formed, we deem it to be especially important that no such aid should be had recourse to, which it will not be wholesome and proper to rely on through life. Much of that supposed dependence of health and comfort upon the moderate habitual use of fermented liquors, which Dr. Robertson seems to consider an unquestionable fact as regards a considerable proportion of the higher and middle ranks of society, especially among those who dwell in towns, and who exercise their minds rather than their bodies, arises, we are satisfied, from the employment of them during the periods of childhood and youth; which renders the nutritive system less able to do its work without them in adult age. Now it appears to us just as repugnant to common sense to induce such a state of dependence upon artificial aid, in order to do that which the natural powers are of themselves quite sufficient to effect if fair play be given to them, as it would be to render a child habitually dependent upon a narcotic (which might be very easily brought about) for its natural quantum of sleep.

Throughout this section of his work, Dr. Robertson seems to us to be hampered by a fear of being supposed to espouse a movement that is not very popular amongst the class to which he chiefly addresses himself. We should have liked him to say unreservedly, what we are pretty sure that he really feels,—namely, that the habitual use of alcoholic stimulants is neither requisite nor desirable in the natural healthy state; and that, in correcting departures from that state, a fair trial should be first given to every natural means of restoring health and promoting vigour. It is surely impossible that alcohol can take the place of repose to the man whose nervous system is unduly tasked; of fresh air, to the man who is pent up in a close vitiated atmosphere; of muscular exercise, to the man of sedentary habits. The use of it may enable each to *feel less need* of that which he really wants; but he does not in reality want it the less. In fact, the real need is increased, in proportion to the greater functional activity induced by the operation of the stimulant. And though by its use a greater quantity of work may be undoubtedly got through in a given time, yet few will be found to affirm that the powers of the system are not thereby impaired. The case of the ballasters, draymen, and others, who consume several quarts of malt-liquor a day, is appositely cited by Dr. Robertson in proof of the necessity of looking onwards, before the real effects of habitual over-indulgence can be estimated. These men undergo a great amount of severe muscular exertion in the open air,—circumstances in which the injurious influence of the large quantity of liquor they daily imbibe will be longest in making itself evident; and although they may continue for many years in apparent health and vigour, yet every London practitioner knows that even the slightest accident, or the most trivial ailment, will produce a degree of constitutional disturbance and depression which it is very difficult to combat. We remember a case in which a drayman was affected with an attack of phlegmonous erysipelas, which proved fatal, merely from having bruised the skin between the base of the thumb and forefinger between two casks.—As Dr. Robertson justly remarks:

> "That man only is in good health, who recovers rapidly from the simple accidents incidental to his occupation, and from the simple disorders incidental to his humanity and to the climate he lives in, and who can bear the treatment that those

accidents or those disorders demand; and such a man is not he who uses in excess alcohol in any of its forms; but, far more probably, the man who is, from hereditary or acquired strength of constitution, able to maintain himself in health and strength, without making use of alcohol at all, or who makes use of no more than just suffices for these purposes." (p. 271.)

The doctrine here inculcated would be, in our apprehension, more accordant with truth, if unaccompanied by the qualification with which the sentence closes. For if the temporary vigour of the drayman or ballaster, derived from the habitual use of a large quantity of fermented liquor, be gained at the expense of the power of resisting disease or repairing injury, is it not reasonable to believe that the "health and strength" supposed to be derived from the habitual use of a smaller proportion of the same stimulant, is more apparent than real, temporary rather than permanent? And are not the cases in which positive, indubitable, permanent good is derived from its moderate habitual employment, the *exception*, instead of being (as we should be led to imagine from Dr. Robertson's treatment of the subject) the *ordinary rule*, at least among the class of readers to which he addresses himself?

We have dwelt longer upon this subject than our readers may perhaps consider necessary, after the full discussion which it received not long since in the pages of one of our predecessors.* But in justice to those who have thought much, and who have formed definite convictions, in regard to it, we could not feel satisfied in allowing the somewhat vague and inconclusive views put forth by our author to pass without comment. Having gone as far as he has done, we cannot but think that consistency requires that he should go still further; and we do not despair of finding him, in his next edition, a convert to our own views.

In the latter portion of the first volume, sundry other questions relating to diet are discussed; such as the total amount of food required by the human system in its different conditions of activity, and under varieties of temperature; the proper time for meals; and various subjects connected with these. A large quantity of valuable and recent information is brought together under the first of these heads, for which we should in vain search in any other volume known to us; and the whole matter is treated with a degree of judgment and good feeling, of which the following is a fair sample.

"Under any circumstances, however, as to climate, season, habits, occupation, age, or sex, it may be taken as a general rule, admitting of few exceptions, that the system must be generously dealt with in regard to food, and not restricted to the mere quantity that appears to meet the absolute wants of the economy; or such a state of system will be induced, as forms the most common predisposing cause of disease,—rendering the body peculiarly liable to suffer from exposure to any of its immediate or exciting causes, whether cold, or miasms, or changes of weather, season, or climate. Man was destined to work for his food; and was accordingly formed to require such an amount of food, as would support the expenditure of a life of labour. People may indeed go on existing for indefinite and long periods of time, upon much less food than that now referred to, and therefore need less enforced expenditure of the waste and redundancies of the system; but they will not be in vigorous health, if their state of system deserves the name of health at all; and their state of body is exactly that, in which the seeds of cachexia are most readily laid,—or in which inflammatory affections are the most readily set up,

---

* British and Foreign Medical Review, Vol. XXIV, p. 515.

—or in which malaria and contagion find their readiest victims. And, moreover, their condition of system is exactly that, in which the powers succumb most readily to the effects of morbid action; in which, if inflammation be not so high, nor so active, as when it attacks the man in full and vigorous habit, it yields less readily to treatment, and leaves behind it consequences the most injurious and least remediable." (p. 341.)

Dr. Robertson is deservedly severe upon the attempt to substitute weak soup, however savoury to the taste, for an adequate supply of solid nutriment, in the maintenance of a starving population;—a substitution which could be sanctioned by no one who possesses the slightest knowledge of the real wants of the system. We wonder that he has not discovered that the vaunted discoveries of Liebig, in regard to kreatine, kreatinine, &c., are just as fallacious; these substances, so far from having the least nutritive power, being the results of the disintegration of the muscular fibre from which they are extracted; and being in the living body purely excrementitious, as is shown by their presence in the urine. He seems to have imbibed from Dr. Prout a belief in the converting powers of the system, under the "stimulus of necessity;" as we find him hazarding the assertion that, "under a special exigency, muscle may be formed from gelatine;" whence he infers that, under an exigency still more special we presume, "muscle may be formed from its elements by the powers of the system." (p. 337.) We can scarcely imagine that by the term "elements" our author really means oxygen, hydrogen, carbon, and nitrogen; or would have us to suppose that the human system can generate muscle by the direct combination of these. We infer from the context that he rather implies other albuminous compounds, such as casein, vegetable albumen, &c., under this term; and merely intends to affirm that any of these may be converted into muscular tissue, although the fibrin of flesh is the pabulum from which this tissue may be most readily and easily generated, —a position which nobody denies. If this really be his meaning, the use of the term "elements" in order to convey it, is obviously erroneous.

The concluding summary, on the importance of dietetic knowledge to the medical man, and on the advantageous use which he may make of that knowledge, is sufficiently valuable to deserve being quoted in full.

"The adaptation of the general principles of dietetics, and of all we know or have to learn of the chemistry of food, and the chemistry of the human system, to the treatment of different diseased conditions, must be regarded as no secondary duty of the medical man, and no inconsiderable and unimportant means of restoring health and preserving life. The importance and needfulness of the antiphlogistic regimen,—including a diet that is principally aqueous, and containing exclusively farinaceous diet, and that in a minimum quantity,—have always been admitted and enforced; but more than this assistance in the treatment of disease and the fulfilment of its indications, may be more and more looked for from dietetic researches. The management of the scorbutic, purpuric, anæmic, cachectic, and arthritic conditions of the system, by corresponding modifications of diet, may be already adduced as significant illustrations of the practical good that may be looked for in this direction, by further researches and a more zealous attention to this important department. Indeed, without a rational adaptation of dietetics to chronic as well as acute diseased conditions, their treatment must be rendered by so much more difficult, tedious, and uncertain,—even if the chances of restoration to health be not otherwise wholly done away with. A form of modern empiricism unquestionably owes the proportion of success which has attended it to such dietetic management,—and not to the infinitesimal doses of silica, and the like; and this may be fairly adduced as an illustration of how much dietetic management may effect in the cure of certain morbid states, without the assistance of any strictly medicinal treatment whatever. By simple attention to the quantity and quality of the daily rations, the number of the blood-globules may be diminished or increased,—and febrile or inflammatory states moderated or even removed,—or an anæmic condition cured; by a moderate vegetable diet, the scorbutic condition; by a mixed diet, the purpuric condition; by a fibrinous diet, the cachectic condition; by a less azotised diet, the arthritic condition; may be respectively relieved, in an important degree, without the aid of drugs; and, in many cases, it cannot be doubted, be more effectually and permanently influenced by such means." (p. 353.)

It will not be requisite for us to dwell so long on the contents of the Second Volume; seeing that this is for the most part devoted to topics, on which there can scarcely be two opinions amongst intelligent and unprejudiced thinkers, and on which medical men, in particular, are now pretty generally informed. The first subject discussed is Exercise, on which it would be rather difficult to say anything new; but all that we meet with here is sensible and discriminating, and a probable rationale is given for many truths which experience has long since practically demonstrated. We may allude, for example, to the remarks on the necessity for adapting not only the *degree*, but the *kind* of exercise to the period of life. (pp. 43-5.)—In the next Chapter, Ventilation, Climate, and Change of Air are considered; and to this the character which we have just given of the preceding is equally applicable. We must take exception to one passage, however, which seems to us to treat too lightly the connexion between the health of the inhabitants of a district, and the character of the water with which it is supplied for their domestic uses.

"The degree of this influence has been much exaggerated; and much has been referred to this, which is more justly referable to the stagnant atmosphere, or the prevailing winds, or the deficiency of the drainage, &c. It is probable that, unless in the case of the water being in a state of greater or less putridity, or of its containing a large amount of saline impregnation, or a perceptible impregnation of vegetable or animal matters, it has seldom much effect on the health of a population; and when individual systems have once become habituated to its use, the instances are rare in which it proves to be of any importance, whether the water reaches the surface by passing through sand, clay, limestone, or even shale; a small quantity of iron in water not appearing to have any bad effect on the health of those using it." (p. 133.)

Now we, on the contrary, are of opinion, that too much attention cannot be paid to the purity of the water which is employed by us in various ways as a beverage, and in the preparation of our food. We do not go so far as to assert that our supply of water should be drawn from the still or the rain-water cistern, rather than from the spring. A very minute quantity of saline or earthy impregnation may be innocuous, or even positively beneficial; but a very small addition to that quantity, on the other hand, may be extremely injurious.

A very remarkable case of this kind has recently been brought before the public by Mr. Corfe, the resident medical officer at the Middlesex Hospital.* The principal facts are as follows.—The town of Wolverton, called into existence by the wants of the London and Birmingham Railway Company, is partly supplied with water from wells sunk on the spot, partly from the neighbouring canal, and partly with a mixture of these, or "half and half." The well-water contains so much solid matter, that, although a well was sunk at the station for the supply of the engines, it was found necessary to abandon this source and revert to the canal, in consequence of the copious deposit which took place in the boilers. There have been continual complaints on the part of the town supplied exclusively with this well-water, of the morbid effects resulting from its use; the men suffering from eruptions over the face, hands, and neck; whilst the women and children were the subjects of a much more distressing train of symptoms. About a year ago, the water became more foul than usual; and the number and severity of the complaints have increased. During the first half of the year 1848, no fewer than 110 out-patients from Wolverton presented themselves at the Middlesex Hospital, suffering under a set of symptoms, which, from their uniformity, were evidently referable to some common cause. Their disorders might be divided into two classes. The first were chiefly among the young men, boys, and children; who, soon after their arrival in the town, became affected with a scaly or desquamating surface of the face and neck, porrigo of the head, or psoriasis of the neck, hands, and arms, or urticaria; together with febrile excitement, with more or less disturbance in the bowels, loss of appetite, cough, and disturbed sleep. Amongst the second class of sufferers, of whom the greater proportion were adult females, there were the following more serious, distressing, and intractable symptoms: constant frontal headache, muscæ volitantes, dizziness of sight, temporary loss of consciousness, impaired memory, excessive prostration of the bodily and mental powers, palpitation, pulse varying from 100 to 120, sometimes intermittent, skin feverish, tongue clammy and furred, with a peculiar coppery taste in the morning, bowels costive, and constant gastralgia, with tenderness on pressure. No appetite, disturbed sleep from frightful dreams, a dry, harsh, and straining cough. The cerebral symptoms in some instances were attended with distinct epileptic fits.

---

* See Pharmaceutical Journal, July, 1848.

These symptoms were uniformly presented in the greatest intensity, in those individuals who employed the well-water exclusively; those who were supplied with the mixed water did not suffer from the foregoing disorders in the same degree, but they were frequently troubled in addition by the presence of intestinal worms; and after these had been expelled by the usual anthelmintics, their symptoms corresponded with those of the patients who partook of the undiluted well-water, but were less decided and obstinate. "So singular and so uniform," says Mr. Corfe, "were the symptoms of the whole class of patients, that I do not exceed the bounds of truth when I assert, that if a patient presented herself, and informed me of the street she lived in, I could repeat her ailments almost word for word before I interrogated her." It can scarcely be doubted for a moment, that these peculiar ailments are attributable to the impurity of the water, and to that alone. Here we have a newly-built town, in which every attention has been paid to drainage, and to the construction of the streets and dwellings, in such a manner as to ensure a free supply of air. There can be no suspicion, therefore, of poison from these sources; and, as Mr. Corfe justly remarks, the whole train of symptoms is such as by no means to favour the supposition of malaria, whether arising from vegetable or animal decomposition. The inhabitants supplied with pure water enjoy their usual health; those supplied from the principal well are affected with an aggravated series of disorders, presenting a remarkable uniformity; whilst those who imbibe the mixture suffer from the same complaints in a less intense degree, but with the additional discomforts arising from entozoa. Now we might suppose that the impurity of this pernicious water must be very decided; but such is not the case. The following is the result of Dr. Ronalds's analysis of it:

Saline matter, per gallon	40·44 grs.
Organic matter, per gallon	2·34 grs.
Total solid matter, per gallon	42·78 grs.

The saline matter consisted of a small quantity of the carbonates of lime and magnesia, with free carbonic acid; sulphate of soda and chloride of sodium in large quantity; sulphate of potass and chloride of potassium; carbonates of potass and soda; silica; oxide of iron and alumina in small quantity. The water is thus a weak saline, in which the alkaline bases are imperfectly neutralized. In the combination of its components, it is very similar to the *Ferdinandsbrunnen* of Marienbad; but its strength, as compared with this, is only as six to thirty-four. The symptoms which have been described as prevalent at Wolverton bear a very close correspondence with those which are known to result from the imprudent use of the Marienbad water; the same effects being thus induced by the continued employment of the far weaker beverage, as by a much smaller number of doses of the stronger one. Mr. Corfe seems at a loss to account for the fact, that the larger number of those who suffer from the peculiar disorders he has described, are women and children; the workmen being usually but little affected; and he supposes that the general exemption of the latter is due to "the labour and exertion of their daily occupation, whereby a vast quantity of these soluble salts are thrown off by means of perspiration." Is it not much more likely that, the chief beverage of the workmen being malt-liquor, they imbibed a considerably smaller amount of the pernicious water, than did their wives and children, whose consumption of water and tea was probably much more considerable?

We have given the details of this case, not merely because it is one of the most remarkable we have ever heard of, as well as the most recent; but also because it is probable that, under the new Sanitary Bill, measures will be taken for procuring a public supply of water to many towns which are at present destitute of it; and we would urge upon our readers that they do all that in them lies to secure the most wholesome source that can be attained, as well as the most perfect freedom from subsequent contamination by leaden pipes, which has been a source of disease in many towns whose supply of water is derived from them. That no mischief has resulted from this method of conveyance in one locality is by no means a satisfactory guarantee against its injurious consequences in another; since the action of water upon lead depends upon the proportion of its carbonic acid, saline ingredients, &c.; and the effects of any given combination can by no means be predicated with certainty.

Still, we quite agree with Dr. Robertson in the belief, that many endemic complaints, which have been attributed to the impurity of the water, are justly chargeable on other circumstances; such as the condition of the soil or of the atmosphere, or local peculiarities of diet and regimen. That atmospheric conditions, which may produce no influence on some individuals, are most potent disturbers of the health of others, is a matter of every-day observation. We know many individuals who cannot sleep for many consecutive nights in London, without being troubled, some with asthma, others with headache, others with cough, others with indigestion, —each being affected apparently according to his particular predisposition, and all finding relief by merely changing their nightly abode to one of the suburbs.—Dr. Robertson's remarks on the subject of Change of Air are highly judicious. He justly says, that a man can scarcely be in a state of such perfect health, as not to be benefited by it; and that occasional recourse to it may be considered to be almost necessary to the perfect well-being of every one. Its influence in accelerating convalescence from acute and in promoting recovery from chronic diseases, and its almost specific effect in many cases, must be familiar to every well-informed medical man.

> "It would be generally well, when a man becomes affected with a disturbed state of his digestive or biliary organs, if such state should not speedily yield to a regulated diet, and exercise, and the usual medicaments, that days and weeks should not be consumed, and indeed wasted, in trying this thing or that thing, or trusting to the beneficence of Nature, or to the *strength* of his constitution; but that an early trial should be given of a *judicious* change of air. It would be generally well, when a man becomes affected in some degree of dyspnœa, cough, and other indications of an irritable state of the respiratory organs, if, as soon as the urgent symptoms had been relieved,—or, *better still*, if they should not have as yet come on,—the effect of a wisely-selected change of air were early and timely tried. How often, in cases of this kind, have delays been proved to have been not only dangerous, but fatal! There is not a fact in the whole range of pathology, that is more pregnant with sound matter for practical reflection than this; that at no time is a man more apt to contract structural disease, than immediately after the subsidence of active disorder, during the debilitated convalescence. A *judicious* change of air, as a means of accelerating the restoration to health, as a means of lessening this risk, is invaluable; and, where it can be accomplished, should be never neglected." (p. 162.)

We are not sure whether the simple change of *air* does not get too much credit in some of these cases; and change of *scene* too little. We are satisfied that there is something in the effect of *monotony* of impressions on the sentient system, which acts very unfavorably upon the organic functions, as well as upon the mental operations; and that, in those chronic disorders of the digestive system, with which more or less of hypochondriasis is usually associated, the severance of the ordinary chain of feelings and associations, by which the patient and his disease become, as it were, bound together, is one of the most important parts of the treatment. Every practitioner must have met with cases in which the fixed belief, on the part of the individual, of his or her inability to make the least mental or bodily exertion, is the most obstinate symptom of the malady, rendering the efficacy of all other treatment hopeless; in such a condition no remedy is so certain to have a good effect as an entire change of scene and circumstances, which operates in two ways,—by giving new incentives to exertion for the gratification of the curiosity,—and in breaking through the toils in which the patient, by yielding to his unfortunate propensity, has allowed himself to be gradually and completely inclosed.

Dr. Robertson very justly urges, that as much personal exercise as possible should be taken (short of over-fatigue), to give to the system the full benefit of the change of air; and he points out how those who are not provided with the means of luxurious foreign travel, may derive an equal degree of benefit from a cheap pedestrian excursion through the Highlands, Cumberland, Westmoreland, Wales, or Derbyshire; the access to all which is now so much facilitated by railroads, that the medical man in almost any part of the kingdom, when over-tasked by the anxieties of practice, may manage to spend a week in refreshing his mind and body amidst the beauty and grandeur of Nature, by an absence of no more than eight or nine days from his home and his duties.

In Chapter V, the Hygienic Effects of Clothing are discussed; Chap. VI is devoted to the subject of Bathing and Ablution; Chap. VII to that of Mineral Waters; Chap. VIII to Sleep; and Chap. IX to the Effect of Occupations on Health. These are all written in the sensible and pleasing style, of which we have already given several specimens; and, as we do not find anything specially worthy of comment or criticism, we shall pass on to Chapter the Tenth, and last, which is devoted to the subject of Mental Culture. This presents so concise and simple, but, at the same time, so comprehensive and philosophical a view of the mental functions, their connexions with each other, and their relations to different states of the bodily system, that we would strongly recommend its perusal to all concerned in the direction of physical and mental education,—whether parents, instructors, or medical practitioners. We should gladly place before our readers some lengthened extracts from it, as samples of its merit; but the space to which our critique has already extended renders it necessary that we should content ourselves with a few briefer quotations.

> "The due use of such of the vital forces as are under the control of the individual in a greater or less degree, stimulates and strengthens *all* the vital forces, much in the same way that the due use of the muscles of the extremities strengthens the muscular system generally; whereas the undue use of any of the varieties of vital force exhausts and debilitates, not only the kind of vital force so expended, but the vital forces generally of the system, as well those that minister to the higher endowments, as those which minister to the mere vegetative functions. Hence the undue use of the mental faculties may so far rob the organs of the vital force necessary to the normal performance of their functions, as to interfere with and impede such performance; and the studious man, or the anxious man, becomes a dyspeptic; or the over-tasked child becomes enfeebled, cachectic, and the subject of glandular disease, rickets, *et id genus omne*. Hence, the due use of certain of the varieties of vital force may stimulate the vital forces generally, and promote the well-being and the healthiness of the whole economy; and social intercourse, intellectual excitement, or a moral stimulus, may remove dyspepsia, or rectify other states of organic indisposition; or the same result may follow from a long walk in the open air, or from music, or from dancing, &c.; or the digestion of easily assimilated aliment, or the due use of the motor organs, may react on the mental powers and manifestations, and be followed by greater energy of the intellect, greater powers of judgment, or greater capabilities of memory or imagination." (p. 335.)

The adaptation of the educational training to the various phases presented by the successive development of the bodily and mental powers, is a point much and justly insisted on by our author.

> "The infant, for a term that may differ somewhat in duration, according to the degree of congenital strength and development, is virtually the mere creature of a

vegetative existence, with the addition of the needful instincts that lead it to desire food, that enable it to swallow, to respire, &c. The less that this condition is interfered with, and the less that the supervention of the next gradation in this curious series is hurried,—in the more delicate it might almost be said, the more it can be retarded,—the stronger the probability of future health and vigour....... As life passes on, as the infant system becomes stronger, as the nervous organism becomes more and more developed, the same pains are to be taken to avoid the premature excitement of the opening faculties,—to keep back, rather than accelerate, the dawnings of the successive grades of automatic, organic, sentient, perceptive, and reflective faculties.......By degrees, the manifestation and development of motor power, of the sentient system, and of the perceptive faculties, are evidenced in the voluntary and uncontrollable movement of the extremities and of the trunk of the body,—in the exploring and prying eye,—in the apt and attentive ear, and the nursing should gradually follow such progressive development;......but these advances should follow the evident indications of Nature's requirements, and should never precede, and perhaps seldom go hand in hand with them. By degrees the gradually dawning faculties will show the sense of what is customary and what is unusual, will distinguish between the tones of approbation and those of disapproval; and *the moral culture of the little being begins from that moment.* Then the unwise indulgence, the needless harshness, the inconsistent kindness or severity, may lay the foundation of good or evil in the future life, may influence the temper, and confirm or modify the disposition.......All this is antecedent to the education of the intellect; although thereafter they may advance together to the common advantage of sentiments and faculties. There is no lesson so valuable, so needful to the physical strength, to the bodily health, or even to the chances of intellectual power in the after life, as this,—that intellectual education should be left, during the earlier years, to the natural efforts and investigations of the child himself, and should not be aided by parents or teachers, or promoted by books and lessons, until long after the time when it is commonly thought to be necessary or advisable to commence the business of instruction." (pp. 336-40.)

We are not quite sure that on this last point Dr. Robertson does not err a little on the side of over caution. We fully accord with him in the conviction that by far too high a value is set in the present day upon the amount of the acquirements, and too little upon the mental habits which it is the most valuable result of these acquirements to induce. It is not sufficiently borne in mind that the child, having everything to learn, is continually exercising his mind, even if left to himself, in observation and reflection upon the objects and events around him; and this of course the more valuably, in proportion to the mental cultivation of the individuals by whom he is surrounded. But we cannot go so far as to say with Dr. Robertson, that a child thus circumstanced is undergoing the best possible ordeal for future intellectual effort, even if no book be set before him until he arrive at ten or twelve years of age, and the meaning of the words "lesson" or "school" be practically unknown to him. We attach great importance to the habit of mental *definiteness*, for we are every day perceiving the injurious results of a deficiency in this quality; and we do not think that this can be acquired at a later period, if the mind be not early trained in it. Now no training is so efficient as the application of the intellect to the comprehension, and of the memory to the retention, of a series of facts, and of rules with their applications,—whether in grammar, arithmetic, or physical science; these need not constitute the *chief* part of the early education; but they should not, in our opinion, be entirely omitted from it. Again, there is not a power more important to success in life, than that of steadiness of application to the object of pursuit; now if boys be allowed to run wild and follow their own bent until they are twelve years old, we very much doubt whether a large proportion of them can be brought at that age into those habits of self-discipline, which are needful to check the desultory tendencies which they are pretty sure to have imbibed. We are advocates, then, of the *gradual* introduction, from a very early period, of habits of method and application; so that the child shall grow up, not only with vigorous powers, but with the capability of voluntarily and efficiently directing those powers to a given object, without which all that self-reliance so highly valued by Dr. Robertson is utterly fallacious.

Our author lays great and deserved stress upon the maintenance of a due balance in the employment of the three great classes of endowments by which the mind of man is characterised;—namely the propensities, the sentiments, and the intellectual faculties: and he points out very clearly the results of the excessive or deficient action of either, which are to be seen in every-day characters, as well as in those which have attracted the world's notice. And he thus appropriately concludes:

> "We are bound, then, to govern our appetites, to restrain our passions, to subjugate the animal to the intellectual nature, but to use duly and equally all the powers with which we have been gifted, not only by Divine command directly revealed to us, but by the same command indirectly manifested in the adaptation of our physical system to the ways of temperance, activity, and virtue. The man who so lives is not only acting for his happiness in another state of being, but is securing for himself the greatest possible chance of health, length of days, and real happiness, in his present state of existence; and the man who pursues the courses of intemperance, irregularity, and vice, is heaping coals of fire on his head in this world as well as in the next. An improper use of our powers, whether mental or bodily, is ever attended with a degree of retribution, even in this world, that is little dreamed of by the man who is ignorant of the mighty instrument of reward and of punishment, that the strict enforcement of the organic laws becomes in the hands of the Omnipotent." (p. 353.)

It is scarcely necessary that we should add our hearty recommendation of Dr. Robertson's treatise, not merely to our medical readers, but to the public over whom they have an influence. It is one of the few books which is legitimately adapted, both in subject and manner of treatment, to both classes.

Art. VII.

Mémoires de l'Académie Royale de Médecine. Tome XIII.—*Paris,* 1848. 4to, pp. 728.

Nearly the whole of the Thirteenth Volume of these Memoirs is occupied by a prize essay,—

On the Bile and its Diseases; by Dr. V. A. Fauconneau-Dufresne.

This elaborate essay is certainly the most complete monograph extant upon the subject to which it relates. The author, after adverting to the exaggerated ideas concerning the influence of the bile upon the economy entertained by some of the older writers, and the important part it was made to play in the genesis of disease by Stoll and his followers, observes that it is very desirable that we should avoid falling into opposite errors, by denying the importance of changes in the condition of this and other of the humours; and believes that, at the present day, we must rather look to microscopic and chemical observations, than to mere pathological anatomy, which seems to have exhausted its stock of useful indications. This work is divided into two parts, the one treating of the physiological conditions of the bile, the other of its diseased conditions.

Bile in its physiological condition. We cannot pretend to follow the author through the details of this branch of his subject, a great portion of which must be already familiar to our readers. He reports the *microscopic* observations of M. Bouisson, of Montpellier, at considerable length. The cystic bile of the ox, after having attained a certain amount of viscidity by careful evaporation, or prolonged abstinence, if examined *with its mucus,* presents, amidst the yellow matter, numerous opaque, or semi-diaphanous corpuscles, generally agglomerated into little masses, and sometimes seen in such abundance as to render the bile quite granular. Besides these and the globules of mucus, are seen small bodies, sometimes presenting a cylindrical form, and sometimes flattened, and distinguished from the colouring matter by their geometrical forms and crystalline appearance. These are portions of *cholesterine,* which substance M. Bouisson regards as being not dissolved, but merely suspended in the bile, as it is found to be in the fluid of certain dropsies, especially hydrocele. When the mucus has been removed from the bile by alcohol, the yellowish-green corpuscles are still seen, their size being less, as if their solution had commenced; while the isolated fragments of cholesterine are more distinctly visible.

The author gives a long account of the *chemical history* of the bile, furnishing the analyses which successive chemists have given; but here we need not follow him, inasmuch as the analyses in question are already well known, and many substances once supposed to form elementary constituents of the bile, are now generally acknowledged to have been the mere results of the processes employed. An especially full account is given of the analyses of Demarçay, but none of the more recent investigations of another able disciple of the Giessen school, Strœeker. The author observes with Blondlot, that all are agreed in admitting in the bile—1, water; 2, mucus; 3, neutral and alkaline salts; 4, colouring matter; and 5, a peculiar principle which has been variously designated as bilious matter, biliary resin, picromel, biliary sugar, biline, choleic acid, and resinoid. Although, when the bile is poured out into the intestine, it is decidedly alkaline, it becomes less and less so as we approach the large intestine, when it is acid. "These alkaline and acid conditions have been observed in several cases of intestinal fistulæ, and they furnish us with the means of deciding which part of the canal is the seat of the fistula."

In respect to the *depurative action of the bile,* the author observes that it is a fluid loaded with carbon, and as eliminating this substance from the blood, the liver may be regarded as auxiliary to the lungs; and, in point of fact, an inverse development of the function of these two organs is observable at different periods of life, and in the different degrees of the animal scale.

"In intra-uterine life, the bile, in the absence of respiration, purifies the blood by the removal of carbon. The meconium of the fœtus is the carbon of the blood, extracted in a liquid form, which, after birth, will be eliminated in the gaseous form. If it is objected that the biliary secretion in the fœtus is not manifested until towards the fourth month, it may be answered that, until this period, the liver is of a very considerable proportionate size, and that it retains, for the purpose of its augmentation, materials which, later, will serve for the secretion of this fluid. This function of the bile is not so applicable in extra-uterine life. In the different vertebrata, the development of the liver is generally found to be inversely to that of the lungs. This remarkable antagonism exists especially in fish, which respire by the branchi. In certain species of serpents, the bile is very abundant, as if to compensate for their imperfect respiration............

"............MM. Sandras and Bouchardat (Annuaire de Thérapeut., 1845) have established that, whatever be the nature of the aliment taken by an animal in good health, the quantity of fatty matters found in the blood is nearly the same, which gives rise to the supposition that they are eliminated as fast as they are introduced into it. According to the same authors, the fatty bodies which the liver separates from the blood, have a constant point of fusion, and consist principally of cholesterine, which the blood of carnivora always contains, and of the margaric and oleic acids united with soda. The fatty bodies of the blood are subjected to a series of successive oxidations, by which the solubility of the sodaic compound, which they form, is indefinitely increased. Cholesterine may result from the alteration of the fatty bodies; for it is a neutral fat, the point of fusion of which is very high, and which, not having been burned in the blood, must necessarily become eliminated from the economy." (t. xiii, pp. 99-100.)

Modifications of the bile. These may be brought about by various individual and hygienic circumstances. Among the first may be mentioned the effect of *age*. In the *fœtal* condition, the bile is distinguished (1) by its being a foreign body in the organism, and by its accumulation in the intestine as meconium; (2) by its not containing picromel or choleate of soda, but a large relative quantity of colouring matter; and (3) by its office being that of the removal from the blood of matters which are afterwards removed during respiration. *After birth* the change operated on the liver, both as regards its size, and the nature of its secretion, renders its depuratory action of less importance. In children the quantity secreted is considerable, as may be supposed from their liability to diarrhœa. In them it is very thin, and of a light yellow, and only gradually acquires its viscidity. It is about the 35th or 40th year that the bile is especially energetic, "this being the period when the passions to which the bilious temperament disposes, as well as the affections which have the liver as their seat, are especially developed." In more advanced age, much bile continues to be secreted, although its action on the economy is diminished; in *old age* the fluid becomes more and more viscid and deep-coloured; and in extreme old age, the liver is atrophied, and its secretion lessened, and of a dark or even black colour. Although no direct observations have been made in reference to the influence of *sex*, it is to be remarked that the bilious temperament is more common in men, and a feeble digestion, constipation, and biliary calculi in women. Although chemical and microscopical observations upon the condition of the bile in different *temperaments* are wanting, yet it is obvious that great differences exist; for while in the bilious temperaments the fluid is found darker, thicker, and more adhesive, so that it attaches itself to the walls of the different receptacles and canals, it is of a much more watery consistence in the lymphatic; so that the author is inclined to attach some importance to the influence, so often stated formerly, to be due to the acrid and irritating conditions of this fluid. He suggests that the deep colour of the bile in the bilious temperaments, indicates a greater elimination of the colouring matter of the blood, which is accompanied by a proportionate diminution in the deposition of fat in the cellular tissue. He considers the term *bilious temperament* a very legitimate one, and that the *atrabiliary*, or *melancholic* is a diseased condition of this, in which the bile takes on more determined characters, and the organ secreting it has acquired a susceptibility which often leads to disease.

In respect to the modifications of the bile dependent upon *hygienic circumstances*, various *ingesta* exert their influence. In ordinary alimentation, the presence of chyme in the duodenum excites an increased flow of bile; while in *prolonged abstinence* the quantity entering the intestine is diminished, the fluid becoming increased in amount and in viscidity in the gall-bladder, so that in persons dying of inanition, this is found much distended. By observing particular forms of diet, and other circumstances, the condition of the bile may be much influenced. Thus, ducks and geese fed on maize, and kept at rest, have the liver rendered fatty, and the bile *albuminous*. According to Tiedemann and Gmelin, vegetable substances especially produce the resinous portions of the bile, seeing that this exists much more abundantly in the ox than in the dog or in man. The experiments of many observers, however, show that animal matters excite the secretion more than vegetable; and according to the last experiments of Sandras and Bouchardat, this observation especially attaches to fatty bodies. As to the ingestion of *medicinal substances*, aqueous drinks effect a dilution of the bile, to which is due a principal part of the medicinal effects of certain mineral waters, as Vichy and Cheltenham, in old hepatic affections. The odour of ether has been detected in the bile after its introduction into the stomach, and metallic mercury is in part eliminated by this fluid; but various other substances, as arsenic, copper, &c., absorbed from the intestinal surface, are arrested and deposited in the substance of the liver. Certain substances, from their augmenting the quantity of bile, are called *cholagogues;* as various purgatives, especially rhubarb and colocynth. M. Cazenave found that the stools of cholera patients treated by carbon soon manifested bile. Acid juices and drinks, opium, &c., diminish the secretion of bile. The influence of *season* and *climate* on the secretion of bile is familiarly known; the liver eliminating a portion of the carbon of the blood in warm climates, which, in more temperate ones, would have been discharged by the lungs. The bile of the inhabitants of such is not only more abundant, but of a higher colour, to which some have referred the peculiar colour of their skin. *Inactivity* and *sleep* diminish the production of bile, and lead to its inspissation.

> "But if the influence of ordinary sleep upon the bile of man and animals is but slight, this is not the case with respect to those animals who become plunged into a *hyemal sleep*. In such, the fluid seems to resume the characters it possessed during the fœtal life, in consequence of the torpor into which the whole organism is thrown, and the especial diminution of respiration. During this singular condition, the rectum becomes filled with a substance analogous to the meconium, and the gall-bladder contains a green bile approaching to brown, and but slightly bitter. These observations, made first by MM. Mangeli and Prunelle, have been verified by MM. Tiedemann and Gmelin, who, having killed various hedgehogs, marmots, &c., during their lethargic sleep, also found in their intestinal canal a large quantity of bile." (t. xiii, p. 124.)

Passing over the author's chapter upon the variations of the bile observed in different animals, we come to the second part of his work *On the Diseases of the Bile.*

1. *Changes in the bile in respect to quantity.* This chapter is divided into three sections, treating of the excess, the diminution, and the retention of the bile. 1. *Polycholia, or an increased quantity of bile,* may occur independently of any disease of the liver, properly so called. It is sometimes merely *temporary,* occurring at the commencement of various diseases, or what seem to be the critical periods of others. When more *prolonged,* the organ seems to acquire a habit of secretion; and Cruveilhier has several times observed, without the existence of any obstruction to the flow of bile, the intra-hepatic canals acquiring the volume of the vena porta, and the gall-bladder that of the urinary bladder, the bile being at the same time very watery, and the tissue of the liver softened. In typhoid fevers, according to Louis, the gall-bladder often contains two or three times more bile than ordinary, that found in the intestines being also very abundant and watery. The *symptoms* will vary, according to the degree of the affection, from slight *embarras gastrique,* supra-orbital headache, yellow skin, &c., to pain and tenderness at the hypochondrium, colics, diarrhœa, and the vomiting of very large quantities of bile. Little *treatment* is required in the slighter cases; but, when there are much pain and vomiting, opium, leeches, ice, purgatives, or even astringents, may be required.

2. *Oligocholia,* or a *diminished quantity of bile,* may vary much in degree; some authors, as Stahl and Bordeu, even admitting of an *acholia,* or complete suppression, without, however, offering any proof of this.* It is produced under the influence of opposite causes to polycholia, viz. cold seasons and climates, sedentary life, concentrated passions, &c. It exists to a certain degree in feeble lymphatic subjects, and in ill-fed women. The abuse of acids and astringents, and too long fasting, diminish the amount of bile, as may certain diseases, as chlorosis, hypochondriasis, mania, and organic affections of the liver. As the affection is slight or severe, more or less impairment of the digestive and assimilative processes will occur, and supplementary secretions may be set up from the skin and kidneys; the engorged state of the venous abdominal system giving rise to hemorrhoids. In treating this case, the author protests against the English practice of giving calomel under the idea of its possessing a direct power of augmenting the biliary secretion; but believes that good results may sometimes flow from bitters with aloes or rhubarb, as also from the ferruginous waters, due attention being paid to hygiene.

3. *Dyscholia,* or *retention of bile.* Obstacles to the flow of the bile may occur either within or externally to the biliary canals. Among the former may be mentioned a thickened or degenerated state of the bile itself, the consequences of inflammatory action in the canals, the entrance into them of foreign bodies from the intestines, &c. Of all causes of obstruction, biliary calculi are the most common. Acting externally, we may have various altered conditions of the liver, tumours from enlarged glands and other causes, the gravid uterus, peritoneal dropsy, &c. The cystic canal is the most frequent seat of the obstacle, and if the passage of the

---

* The author seems to be unacquainted with the cases published by Dr. Alison (Edinb. Med. and Surg. Journ., vol. xliv), in which death evidently resulted from *suppression* of the secretion; the patients having been deeply jaundiced, whilst the bile-ducts and gall-bladder were found to be empty.

bile is completely prevented, this soon becomes converted into a ligamentous cord. Obstacles are rarer in the hepatic ducts than in the ductus choledochus.

The author describes the ***anatomical changes consequent on obstruction*** at great length. When this occurs at the *cystic canal*, the bile contained in the gall-bladder becomes much thickened, and so altered in appearance as rather to resemble sedimentary urine. The parietes of the vesicle may become converted into fibro-cellular membrane, the inner coat, however, not unfrequently inflaming and ulcerating, so that perforation takes place. The receptacle often atrophies, and may entirely disappear; the other biliary canals sometimes at the same time become dilated so as to compensate in some measure for its absence. When the obstruction is seated in the *hepatic duct*, the radicles of this may become distended to the size of the finger, near even to the periphery of the liver; and those of them which are not usually visible to the naked eye may now be seen on the surface of the organ larger than a quill; while the bile, passing into the lymphatic vessels arising on its canals, distends them considerably. The radicles may give way at various points, or they may form small tumours, here and there filled with bile, sometimes mixed with pus. The liver may acquire enormous dimensions, and present a fluctuating tumour, the slightest puncture of which gives rise to a continuous flow of bile. The hepatic tissue, compressed on all sides, atrophies, and may fall into the completest *ramollissement*. Obstacles compressing the *choledochus* give rise to complex results, for, besides the changes in the liver just mentioned, there is a distension of the gall-bladder that sometimes becomes enormous. Thus in No. 333 of the 'Philosophical Transactions,' a case is given in which eight pints of bile were discharged. Young, in vol. xxvii of the same work, cites a case of seven pints; while Fryer, in a case quoted in the 'Bibl. de Med. Brit.,' 1814, relates one in which 13, 15, and 13 pints were removed by successive tappings of the liver of a patient suffering from traumatic jaundice. In these cases of excessive distension, the inner coat of the gall-bladder often gives way, while the outer is thickened; but in some cases this too gives way, inducing fatal peritonitis, or evacuating the bile by a communication with the intestines. The biliary canals in these cases are much dilated, and the choledochus above the obstruction may equal the stomach or intestine in size; and when the obstruction is near the duodenum, the pancreatic duct and its radicles participate in the dilatation. The bile which is detained in the canals does not undergo exactly the same change as does that of the gall-bladder. It may become thickened, and of much deeper colour, and give rise to calculi; but at other times it loses its viscidity, and is converted into a light yellow or greenish insipid serosity. Even after all obstruction has ceased, the canals, having lost their elasticity, return very slowly to their size; which may explain the fact of great dilatations being sometimes met with in autopsies, without the existence of obstructions to explain them.

The *symptoms* are chiefly derived from the effects produced by the enlarged liver, or the distension of the gall-bladder; icterus also being an early symptom, and intense in proportion to the amount of obstruction, for this is sometimes incomplete, so that some bile still gets into the intestine. It is not rare for these cases to become complicated with inflammation of the lining membrane of the gall-bladder, which may lead to suppuration. This complication renders the diagnosis of the disease from abscess of the liver difficult. In *treating* this affection, we have not only to do what little can be done for the removal of the various obstructing causes, where these can be ascertained, but to treat the *biliary tumour* also; when this reaches a large size so as to threaten rupture, a fistulous opening should be made by means of potassa fusa, which induces adhesion of the peritoneum. Two cases are related in illustration. If inflammatory complications have preceded, as adhesions are then always present, a simple puncture suffices.

2. *Changes in the composition of the bile.* The bile may become changed in its intimate physical or chemical characters, or by its mixture with other products. Thus the fluid may undergo much change as respects *colour, consistency,* odour, &c.

> "Although the intimate causes of such modifications may be unknown to us, it is certain that they are the consequence of the nature of the disease which they accompany. In the affections in which the blood is impoverished, as dropsics, chlorosis, phthisis, when the liver becomes fatty under certain debilitant regimens, bile approaches more to the character of serous secretions. In some fevers, on the other hand, all its special characters are exaggerated: its viscosity becomes pitchiness; its bitterness, acridity; its colour and odour, more decided." (t. xiii, p. 178.)

The *chemical alterations* of the bile have been, amidst the uncertainty as to its normal composition, little investigated; but the author especially refers to the transformation of this fluid, in the case of fatty liver into an albuminous fluid, the bile being the least albuminous of any humour naturally:

> "M. Bouisson believes we may compare this remarkable condition of the bile to that of the urine in albuminuria, and thus constitute a morbid result which may be termed *albuminocholia.* He supports this view by the fact, that in Bright's disease the bile undergoes the same alteration as the urine; for having sent to M. Berard a certain quantity of the bile of a patient who had sunk under albuminuria, this chemist recognised in it a notable proportion of albumen." (t. xiii, p. 183.)

3. *Biliary calculi.* This section is so fully and indeed elaborately worked out, that it constitutes a very complete and valuable treatise upon the subject to which it refers. Our limits will only enable us to glance at some of the leading points. After furnishing a most minute description of the various *physical characters of the calculi,* the author gives some account of what he terms *biliary gravel,* consisting of very small calculous particles not possessing the regular structure of the calculi properly so called. With Bouisson, he believes that there are three varieties of this, viz., 1. *Cholesteric gravel,* consisting of little particles having an unctuous surface, devoid of consistence and form, and possessing a homogeneous and fatty composition. Mixed with colouring matter, they form the amorphous bodies found in the gall-bladder or canals, or passed by stool. 2. *Pigmentary gravel* is formed of the pigment of the bile; and bovine animals, whose calculi are chiefly composed of colouring matter, are very liable to the formation of these little bodies; but small particles, in which the pigment predominates over the cholesterine, are not unfrequently seen in man. 3. *Melanic or carbonaceous.* Several examples of this have occurred, and the author is disposed to regard it as analogous to the carbonaceous depositions observed in the lungs.

The author enters into a long history of the *chemistry of biliary calculi,* properly so called. It suffices here to observe that very variable proportions of cholesterine and colouring matter enter into their composition, these substances being sometimes promiscuously commixed, and at others disposed in regular layers. In rare instances calculi are formed solely of one of these substances; and occasionally *melanic* calculi are met with, which admit of a subdivision into carbonaceous and resinous. There are two examples on record, in which the calculi were exclusively formed of *saline* matters; and in some others small proportions of alkalies have been found. *Water* exists in calculi often in the proportion of one half; *albumen* is found, and *mucus* is present in the envelope, and plays an important part in binding the particles together.

> "Microscopic researches have taught us that the colouring matter is not entirely dissolved in the bile, a portion of it becoming precipitated. So, too, cholesterine, which, from the observations of Chevreul, was believed to exist in a state of solution, is only suspended. From this, it results that the materials of which biliary calculi are composed, are already formed, exist in a separate state in the gall-bladder, and may be regarded, according to M. Bouisson, as microscopic calculi." (t. xiii, p. 230.)

Among the *circumstances favouring the formation of calculi*, we may mention—1. The *period of life*. Thus, they are of very rare occurrence in childhood and early life, the period most favorable for their formation being, according to Walthers, first, between 30 and 40, and then between 50 and 60; and, according to our author, first between 50 and 60, and then between 60 and 70. 2. *Sex*. Most authors, except Morgagni, admit the greater liability of women. In the author's table, they are as 78 to 43 men; and calculi are much oftener found in the examination of the bodies of the old women at the Salpêtrière, than of those of the old men at Bicêtre. 3. *Temperament*. It is generally admitted that persons of the bilious temperament are the most liable; but difference of opinion exists as to how far *obesity* may predispose. It is certain that fat old women are especially liable to hepatic colic. 4. *Hereditariness*. M. Petit, of Vichy, who has a great deal of hepatic disease coming under his notice, states that he has known several families in which this affection has been distinctly hereditary, and this in spite of prolonged medical care. 5. *Causes which render the passage of the bile slower*. In this manner, sedentary and indolent habits are supposed to act. Hence, as shown by Tissot, literary characters, and, according to Soemmering, prisoners are very liable. Chlorosis, melancholia, &c., in like manner, predispose. 6. *Diet*. A too animalized diet, unless accompanied by abundance of exercise, loads the blood and bile with fatty and carbonaceous matters, which may easily become precipitated. 7. *Season*. At Bicêtre and Salpêtrière more cases of calculi are observed in winter than summer, after taking into account the greater mortality in the former season. 8. *Effects of acids*. The older physicians (and some modern ones) gave a completely local explanation of the formation of the calculi, viz. by the penetration of the normal acidity of the *primæ viæ* into the gall-bladder. Little probable as is this, the too great consumption of acid substances may favour the deposition of the colouring matter and cholesterine. Becquerel and Rodier state that the quantity of this last substance in the blood becomes doubled under the influence of phlegmasia; and this material of the bile, thus retained in the blood, may, after the inflammation is subdued, pass into that fluid in too great quantity, and give rise to concretions.

Calculi will produce different effects, according to the *portion of the biliary apparatus in which they exist.* Although rarely, they are sometimes found in the *radicles of the hepatic duct*, generally then existing as biliary gravel. Occasionally, however, they there become very large, and cause much obstruction. Cruveilhier has observed, both in adults and children, a great number of single or multilocular tumours on the surface of the liver, generally containing biliary concretions, and sometimes only a thick bile, not to be confounded with certain calcareous concretions occurring under the envelope of the liver, but not communicating with the biliary canals. Calculi of the *hepatic duct* itself are of the rarest occurrence, while those of the *gall-bladder* are of the most frequent. They may there exist in great numbers, or, if single, acquire an enormous size. Occasionally, the calculi adhere to the bladder; and sometimes the union between the two becomes so intimate, that the receptacle is described as atrophied or non-existing. In other cases, hypertrophy, ramollissement, or ulceration of the gall-bladder may take place; and long-continued retention may give rise to the development of its muscular tunic, as described by Andral and Louis. It is to the detention of the calculus in that tortuous canal, the *cystic duct*, that the severe hepatic colic in these cases is due; and if the calculus is very large, it may become immoveably fixed, giving rise to inflammation and its consequences. In the case of calculus in the *choledochus*, if this be irregular in size, even though somewhat large, a portion of the bile will sometimes pass by; while a small calculus placed at the ampulla common to the hepatic and pancreatic ducts may cause total obstruction.

The *symptoms* also vary according to the part in which the calculus is located. Those seated in the *hepatic duct or its radicles* may furnish few or none of a distinctive character; and when the calculi are small, smooth, and few, their existence even in the *gall-bladder* may give rise to little inconvenience. Under opposite circumstances, however, they may give rise to great suffering by distension, and sometimes may be felt externally, or heard when brought into collision with each other. It is, however, to the passage of the calculus along the *cystic duct*, that the chief suffering is referable; but even here nothing can be more variable than the amount and nature of the pain suffered. The period when the bile should be normally poured out in the intestine, viz. some time after a repast, is a very frequent one for the excitation of painful attempts at expulsion of the calculi; but, although the attacks are sometimes periodic, they are generally irregular. The suffering is often so intense as to be unbearable by the most determined patients, and produces various sympathetic effects in the general system. Duparcque relates, that he has observed in four cases out of thirteen, a singular *atonic spasm*, affecting first the right side of the abdomen with rapid movements of elevation and depression, and gradually implicating the rest of that side of the body in these epileptiform convulsions, during which also the brain became confused, and its functions suspended. These attacks were renewed with the colic paroxysms. Death itself (Portal) has followed the same colic; and syncope, excessive vomiting, and hallucination may accompany it. There is seldom, however, unless inflammation of the passages be present, any febrile reaction, the pulse continuing small and frequent. Icterus is far from being a constant symptom; and arising not from obstruction of the bile, but from sympathetic irritation, is not intense. Occurring at the end of the colic, it indicates that the calculus, having passed into the choledochus, now obstructs the flow of the bile into the duodenum. Intense and prolonged as may be the patient's sufferings, they are not always efficacious, the calculus falling back again into the gall-bladder. The arrival of the calculus in the *choledochus* is usually attended with relief and the production of jaundice. This last, however, owing to room being left for the passage of some bile, sometimes hardly exists during the entire progress of the case. So, too, the pain in some cases continues equally intense until the calculus has entered the duodenum, its escape thereinto being occasionally distinctly appreciable by the patient. The torrent of grateful exclamations uttered by him, and the profuse, and sometimes fetid, sweat which pours from his body, testify to the intensity of his previous suffering. Calculi reaching the *intestinal canal* are in some few cases rejected from the stomach by vomiting, although of this the author can only find eight cases on record. They may be retained in the canal, and much augmented in size by additional portions of bile or matters secreted by the intestines. It is somewhat surprising, considering the great number of biliary calculi which pass along the intestine, that there is but one case on record of these having been detained in the appendix vermiformis. Some persons pass calculi by stool, without having suffered any prior symptoms, but these are then usually small and imperfectly formed. Sometimes their agglomeration forms immense masses, only to be expelled by great effort.

Biliary fistulæ are oftenest caused by calculi. They may be *external* or *internal*, the former of which are very rare, since the author, by every care, can only collect 23 cases, some of which are very imperfectly recorded. In 19 of these cases, calculi were the cause; and in most of these, inflammation of the gall-bladder had preceded. In some cases pus alone flowed from the aperture, in others bile in vast quantities, as also great numbers of calculi. The fistula may be single and direct, or multiple and tortuous, giving rise to impaction of calculi and further inflammation. They are not usually a cause of death, several patients having completely recovered. Although we have few accurate records of such cases, *internal fistulæ*, by which a communication is opened for the discharge of large calculi, cannot be very rare,—the duodenum being in general the point selected; most examples of this, as in the case of the external, being found in old women.

Biliary calculus formed in the blood of the vena portæ. This, which is an unique case, the author observes, might well be called into question, had it not been under the observation of one so competent as M. Deray, of Lyons. The case has already been published in the 'Gazette Médicale' (April 1843). Icterus had existed for several years, and after death the biliary passages were found obliterated and the gall-bladder atrophied. The liver was small and softened, but the trunk of the vena portæ was at least double the size of that of the vena cava inferior; and in its right branch a cylindrical calculus was discovered, which, on analysis, was found to consist of cholesterine, stearine, yellow colouring matter, resinous matter of the bile, picromel, and magnesian salts.

The *diagnosis of biliary calculi* may be rendered difficult—1. By the non-manifestation of symptoms, which in some forms of the disease is not rare.—2. These symptoms may simulate those of other diseases, especially those of a *nervous* or *rheumatic* character. In all suspected cases, it becomes, then, essential for the physician himself to examine the fæces for calculi, under a stream of water.—3. Certain other diseased conditions may simulate calculi. Among these are an inspissated or altered state of the bile, intestinal worms, rheumatic or nephritic pains, neuralgia of the liver, causing jaundice, &c.

In their *treatment* four indications are to be observed—1. *To relieve the pain and other symptoms* which result from the passage of the calculi through, or their detention in, the biliary canals. When the symptoms are severe and the patient strong, bleeding to syncope is indicated; for nothing is so much to be feared in the distended state of the canal, as inflammation. We must be guided more by the amount of tenderness, than by the state of the pulse, which, in consequence of the prevalence of the spasmodic condition, is small. If fever accompanies inflammation, the pulse then indicates the amount to be drawn. Leeches, cataplasms, fomentations, anodyne liniments, prolonged and frequent tepid baths, emulsive or oily drinks, and enemata are indicated; but purgatives, given during the attacks to hasten the passage, only lead to inflammation and rupture. Narcotics and antispasmodics are also required, if the pain is very severe. In some cases, where every means have been exhausted, others, tried in mere despair, as the application of ice or the use of the vapour-bath, have entirely succeeded.—2d Indication: *the disengagement of the calculi and the facilitating their expulsion.* If, from the state of the symptoms, we have reason to believe that the calculus has reached the choledochus, we may carefully excite the peristaltic movements of the intestinal canal by Seltzer water or castor oil. The abundant use of the Vichy waters exerts a liquefying effect on the bile, and aids its flow.—3d Indication: *the solution of the calculus.* Many of the older writers occupied themselves in the search of solvents, but it was not until the time of Durande that the one so often since resorted to was contrived, viz. *sulphuric ether, 3 parts; essence of turpentine, 2 parts.* In consequence of its nauseousness, various modifications have been since made, and one quite recently by M. Duparcque (castor oil 60, ether 4); but the author, while believing that these remedies have some effect in relieving pain and facilitating expulsion, does not allow that any of them possess any solvent property whatever. He attributes some amount of such power to *alkalies,* which, too, by saponifying the fatty matters of the blood, prevent their additional deposition as cholesterine. He prefers a solution of the *bicarb. of soda;* and the Vichy waters form a very suitable preparation.—4th Indication: *the prevention of new formations,* the author believes, is best brought about by whatever tends to keep the bile in a more fluid state, as alkaline salts, the organic acids, alkaline mineral waters, a temperate diet, and the avoidance of all fatty bodies; while baths, frictions, warm clothing, exercise, and whatever increases the respiratory powers, are advisable.

4. *Icterus.* This disease also is treated of at great length; but we can only advert to some of the leading observations. In his *description* of the disease, the author observes that the *yellow colour* is always first seen in the conjunctiva, and that we shall always find it most marked at the point where this is reflected on to the inner surface of the eyelids—the

membrane being most thick and vascular here. In a Negro observed by the author, this colour was well marked, and his black skin presented a slight greenish reflection. When the jaundice is sympathetic, the colour will increase or diminish in intensity, according to the condition of the determining lesion, oscillations which are not observed in the idiopathic form. Sometimes the colour is much more intense on one side of the body than the other, and occasionally certain parts of the body are quite exempted from it. Cases, however, given by many authors as examples of the colour being confined to very limited regions, are not due to the colouring matter of the bile. M. Delongeon has observed almost all the *mucous membranes* to become coloured, except at the edge of the lips.

> "A special mark of icterus seems by its constant existence, and the invariableness of its seat and form, to deserve attention. Portal is the only author who makes any mention of it. If we examine the mouth, we shall find a yellow tinge apparent on the surface of the mucous membrane behind the lips, that of the gums, the lower surface of the tongue, the cheeks, the palatine arch, and the pillars of the visible portion of the posterior wall of the pharynx. But this colour is commonly little marked on all these points at once, or, if it be deep, it is unequally so, without affecting any special part or observing any exact limitation. At the velum palati, on the contrary, there is constantly a deep uniform yellow colour, contrasting strongly with the tinge of the surrounding parts. Behind, this loses itself gradually in the normal or changed colour of the posterior edge of the velum, the uvula, and the pillars; but in front it terminates in a straight, well-defined, transverse line, corresponding to the line of demarcation of the palatine arch, and the horizontal portion of the velum, and which extends from the last molar tooth of one side of the upper jaw to the corresponding one on the opposite side. In only one instance has M. Delongeon found this disposition absent. In all others, even of the slightest description, and in all the varieties and epochs of the disease, has he always seen it thus distinct and regularly limited. We may find in the soft and eminently vascular tissue of the velum, the reason of this icteric suffusion. The fibro-mucous, dense, and comparatively slightly vascular tissue of the arch could only slightly participate in this; and to this we must attribute the line of demarcation between the two regions. A coloration of this part seems indeed to be produced with the greatest facility, as M. Delongeon states he has observed it in cases in which the skin has received a slight tinge in lead colic or cancerous degeneration." (t. xiii, p. 418.)

The *itching* oftentimes so distressing in jaundice, is sometimes accompanied by small papulæ, and followed by desquamation. In old cases the skin may have a soapy feel, the bile being transported there almost unaltered, when, too, it stains the linen, and gives rise to a peculiar odour sometimes termed musky. Occasionally the *fæces* are abundant, and still retain their colour, as in polycholia, but the icterus in such a case is neither intense or obstinate. The deep colour of the *urine* is often seen before that of the skin; and in some affections of the liver the urine becomes bilious, without the skin undergoing any alteration. In some very rare cases, produced by some moral cause, the urine retains its colour throughout. Clear at first, it afterwards deposits a thin brick-coloured sediment, at the same time that its colour becomes lighter and the disease is on the decline. In respect to icteric patients *seeing things yellow*, after adducing the observations *pro* and *con* of various observers, the author adds:

> "For our part, whatever care we have taken during the many years we have occupied ourselves with diseases of the liver, to examine into this phenomenon, never, even in the most intense icterus, has it been in our power positively to verify it. Several patients, astonished at our question, have seemed at first disposed

to answer it affirmatively; but on examination not one was able to confirm what he seemed disposed to recognise. Nevertheless, in presence of such authorities as we have cited, we cannot doubt of its reality." (t. xiii, p. 423.)

The author examines at some length the writings of the various observers who have made the *condition of the blood in icterus* an object of analysis, and sums up the results in the following passage:

"The blood undergoes changes in various modes. In the first place, according to the comparative analyses of Lecanu, it contains a much less quantity of *red colouring matter* (77 vice 128 parts per 1000). Next, the numerous works we have now examined show that a greater or less quantity of the *colouring matter of the bile* is constantly found; and that this in prolonged cases may become so abundant, as to be exhibited by reagents almost as in masses; the best agent for this demonstration being nitric acid. If there is a general accordance as to the presence of the colouring matter of the bile in the blood, it is not the case with respect to the other constituent elements of this fluid. We have seen that Chevreul regards their existence as very probable, and Liebig admits their presence without being able to appreciate them precisely. These elements, in fact, far from being fixed, and retaining their affinities like the colouring matter, so as to be accessible to reagents, may, on the contrary, become decomposed during the vital actions. Nevertheless, it is to be observed that M. Orfila has found in the blood in icterus resinous matter, and M. Boudet cholesterine and a little soapy principle; while M. Braconnot has detected in the peritoneal serosity a notable quantity of cholesterine and picromel. MM. Becquerel and Rodier have gone a step farther, in recognising that the fatty principles constituting the bile accumulate in the blood, either in consequence of the liver not being able to extract them, or from their flowing back to it, the bile being already formed. Although from additional researches we may expect a more complete demonstration, we may even now admit the bile in its entire state passes into the blood, and, moreover, that its materials accumulate in that fluid when they cease to be separated by the liver." (t. xiii, p. 439.)

Of all the products of secretion, the *urine* is that which most constantly and distinctly manifests the presence of bile. The author objects to Berzelius's direction of employing an equal volume of nitric acid for its detection; as the green precipitate then produced is redissolved, whereas it is effectually developed by a smaller proportion of the reagent. An acid offers an excellent means for the practitioner to determine the presence of bile at the bedside in slight cases, or when the colour of the skin may be mistaken for that due to cachexia or the effect of lead. We should be careful, when bilious urine becomes very deep coloured, not to confound it with bloody urine. Nitric acid produces from this last no such precipitate, and the microscope determines the presence of blood-globules.

Passage of the bile by the lymphatics. Cullen first suggested that when the bile was detained in its canals, it became absorbed by means of the lymphatics; and Assalini, Andrée, and Saunders corroborated him by showing that the lymphatics of the liver and the thoracic ducts contained a yellow fluid. The ligature of the duodenum by Portal, and of the choledochus by Tiedemann, furnished similar conclusions; and the experiments of Mascagni, Kiernan, and Meckel showed that injections of the biliary passages escaped rather by the lymphatics than the vena portæ. Additional experiments by the author and M. Lambron found them penetrating the lymphatics, even before they had reached the extremities of the biliary canals. Under the section Dyscholia, a case is given in which the lymphatics leaving the liver and lateral ligaments, were distended with bile to the size of a crowquill. In icterus, with polycholia, the absorbents of the intestines may become in like manner so employed, as in the case cited by Portal.

Diagnosis of icterus. Certain changes of colour may occur, which may be confounded with true icterus. The *saturnine icterus* is distinguished by M. Tanquerel by its lesser intensity, its earthy or dirty yellow (never green), and its especial development on the face. The conjunctiva presents a dirty yellow, which blends generally with a very evident bluish tinge. M. Delongeon mentions a case in which the non-production of the green precipitate in the urine by nitric acid alone could determine its nature. The colour in the *cancerous cachexia* resembles that of the quince; the conjunctiva remaining bluish or of a dull white. The yellow colour observed in *purulent infection* affects the trunk especially, and not the conjunctiva, while fever is also present. In *chlorosis* the conjunctiva is not coloured, and the skin is more clear and transparent. The biliary hypersecretion which follows the *bite of a venomous animal* gives much the appearance of jaundice, but there is no bile in the urine.

Diseases which produce icterus. 1. *Alterations in the liver.* Changes in the condition of the excretory apparatus of the bile have been already treated of. *Congestion* of this organ may be accompanied by slight and sometimes by complete jaundice; but this is rarely observed in that passive congestion of the organ so often and so markedly seen in certain diseases of the heart. In *acute hepatitis,* icterus is a frequent but not constant symptom. According to Annesley and Copland, it is rare in India. Stokes noticed its absence in 9 cases; Piorry has met with it only 4 times in 30 cases; and the present author 7 times in 17 cases. In the 6 cases of hepatitis in children, observed by Rilliet, all had it. The belief that it is only contingent upon hepatitis of the concave surface is unfounded. It is not constant in *hepatic abscess,* the author having found it only 10 times in 23 cases; but in some cases it is the only symptom. Icterus not unfrequently results from *concussion of the liver,* produced by a fall on the pelvis or feet. It is pretty frequent in *chronic hepatitis,* and is almost always present in *atrophy,* especially with attendant induration. In *cirrhosis* it is not frequent, and is rarely seen in *fatty degeneration.* 2. *Affections of neighbouring organs.* These may induce icterus by continuity or contiguity. Thus it may follow the introduction of irritating substances into the *stomach* or *duodenum,* and may complicate *pneumonia.* This complication occurred to Grisolle 20 times in 277 cases, and to Chomel 7 times in 70—attacking patients generally under 50 years of age, and men four times more frequently than women. It occurred especially in spring and summer, following pneumonia of the right side in 16 of M. Grisolle's 20 cases, and in 4 on the left. It attended pneumonia of the upper and lower lobes in the same proportion, and generally came on from the fifth to the ninth day, its mean duration being eight days. 3. *Disturbance of the nervous system.* Under this vague title may be ranged those cases known as nervous, spasmodic, or essential, being dependent upon no discoverable physical lesion. The author does not pretend to explain how this cause operates, but does not doubt its reality, and mentions a case of temporary jaundice frequently occurring, accompanied by severe neuralgia. 4. *Fever.* Rarely seen in typhoid; it is not infrequent in typhus; while in the bilious fever of hot climates, or bilious remittents, it is intense. In the yellow fever, according to some, it is always present, but, according to others, it is occasionally absent. 5. *Infantile icterus.* The liver of children soon after birth secreting abundantly, a resorption of the bile may produce icterus. Of 80 instances, Billard found congestion of the liver in 50. There is a much less general affection of the conjunctiva, urine, &c., than in adults.

Influence of different diseases on the bile, and of the bile on different diseases. Formerly a very great variety of diseased conditions were supposed to originate in altered conditions of the bile; and opinion reacting against this exaggeration, it was then stated that augmentation or alterations in the biliary secretion were produced solely as the results of hepatic or gastro-intestinal disease. Since then renewed attention has been paid to the morbid conditions of the fluid, and some few steps have been taken in advance since Andral declared that we were in a state of complete ignorance as to the changes undergone by the bile. It is now proved that not only may this fluid become altered by disease of the secreting viscus, but also by those of other organs having only a functional connexion with it, as the circulatory and respiratory organs. Other alterations result from the operation of disease on the general system, as in fevers. Orfila tells us that he never finds the bile identical in composition in persons dying of apoplexy, enteritis, typhoid, &c., and it is to be supposed that such altered conditions of the fluid exert an effect on the system in their turn.

1. *Influence of diseases on the bile.* All lesions of the *liver* itself do not necessarily produce sensible alterations, if they are only partial. In hypertrophy, the quantity of bile is increased, as it is diminished in atrophy and cirrhosis. In fatty liver, the bile becomes very albuminous. Sanguineous congestion and acute hepatitis augment it, while chronic hepatitis influences it very variably, always, however, more or less changing its physical characters. The amount of bile may become increased under the influence of *inflammation* of the *peritoneum, stomach,* or *small intestines;* though this last may also give rise to its retention, and the production of icterus. In *ascites,* in which the liver is so often implicated, the bile may, as in fatty liver, show a great increase of albuminous principles, and a diminution of those proper to it. In all *general dropsies* there is a tendency in this humour to assume the characters of a serous secretion. We have seen the relations which the liver and lungs bear to each other physiologically, in the removal of carbon from the system; and it may be inquired whether pathology confirms the conclusions arrived at, by showing that the condition of the bile is modified by the arrest of pulmonary exhalation. Hitherto attention has been paid rather to the state of the liver itself, than to its secretion; and although hypertrophied and fatty liver have been observed in *phthisis,* the condition of the bile has been seldom examined. M. Bouisson states that he has found it abundant, dark, and viscous in persons dying of *pneumonia;* and M. Chevalier found an extra quantity of picromel in a phthisical subject. M. Bouisson also found that if animals were killed rapidly by *asphyxia,* no sensible change in the bile occurred; while, when they died slowly, this was found abundant, dark, and consistent. In congestion of the liver from *disease of the heart,* the bile is dark and abundant. The state of the bile in *phlebitis* has been little noted; but the author's attention having been particularly directed to inflammation of the abdominal venous system, he

found the canals full of a thick bile, and in one case implicating the supra-hepatic veins; this fluid was albuminous, the liver being fatty. In *chlorosis,* the bile is more tasteless and pale; while in *scorbutus,* the gall-bladder is distended with a livid green fluid. The exaggerated action of the liver after the *bites of poisonous animals,* producing a polycholia, may be regarded as an eliminatory effort of nature. In various *pernicious fevers,* and in the *plague,* the properties of the fluid have been observed altered, and in *Asiatic cholera* the bile undergoes inspissation.

2. *Influence of the bile on different diseases.* A mere excess of bile may impress on diseases a character foreign to their nature, and may furnish special indications. From this source a form of *pneumonia,* of which Stoll has furnished the completest description, seems to derive its peculiarities. At one period it would seem to have prevailed with great intensity over a large portion of Europe, if we are to judge from the 80 memoirs referring to it, reviewed by the Soc. Roy. de Méd. during 1782-4. Doubtless much exaggeration prevailed; and so seldom is bilious pneumonia now seen in Paris, that M. Grisolle has only met with ten cases in six years. Still its occasional well-marked existence cannot be doubted. Certain *febrile affections,* too, may acquire a bilious character, not as a mere consequence or complication, but as an essential feature; and M. Delaroque even attributes the production of typhoid fever to the action of degenerated bile upon the intestinal canal. It is, however, especially in the *bilious remittents* of hot climates, that we observe the action of this fluid.

ART. VIII.

1. *Females and their Diseases; a Series of Letters to his Class.* By CHARLES D. MEIGS, M.D., Professor of Midwifery and the Diseases of Women and Children in the Jefferson Medical College at Philadelphia, &c. &c.—*Philadelphia,* 1848. 8vo, pp. 670.
2. *A Practical Treatise on the Diseases peculiar to Women. Illustrated by Cases derived from Hospital and Private Practice.* By SAMUEL ASHWELL, M.D. Third Edition.—*London,* 1848. 8vo, pp. 772.

THE works which we have to review emanate from two physicians, each of whom has filled the obstetric chair in a school of medicine,—the one at Guy's Hospital, and the other at the Jefferson Medical College, Philadelphia; and from whose position and opportunities we have a claim to anticipate a full exposition of what is known upon the subjects they write upon, with such a masterly and mature judgment upon the diseases they describe, as will strengthen and confirm the views of the practitioner, and guide and counsel the inexperienced student. In saying this, we are not placing the standard for our criticism higher than the authors have themselves raised it; nor have we extended the scope of their undertakings. They have both arrived at that professional age, when visionary or immature views are generally forced to yield before the exacting influence of a large responsible daily practice; and it is this conviction which confessedly has been, with both of them, a leading—and, we would add, a praiseworthy—motive for their publishing. But although Dr.

Meigs and Dr. Ashwell may be alike in these particulars, their respective works have marked individual peculiarities. Dr. Ashwell's treatise has been before the public for the last four years, and has reached the third edition; although it ought to be noticed, that the second edition was very limited in number, and without emendation or correction by the author worthy of notice. The present edition, however, has some important additions, which render the work more complete. The treatise is divided into two main sections; the first comprising the functional, and the second the organic, diseases of the Uterus. In the former, besides the ordinary functional disorders, are included the subjects of Chlorosis, Leucorrhœa, Hysteria, and Irritable Uterus; and in the latter, in addition to the recognised organic diseases of the uterus and ovaries, there are the Displacements of the Womb, the Diseases of the External Organs, and two Essays,—one on "Premature Labour in Pregnancy complicated with organic disease," and the other "On the Morbid Consequences of Undue Lactation."

We need here only observe, that, for extent of subject-matter, for copiousness and variety of illustration, for authentic practical knowledge, and for vigorous style of writing, the book is not excelled by any on the same subject; and this we say, without prejudice to our free and unbiassed commentary on some of the views and opinions which it contains.

We must not, however, dismiss our general review of Dr. Meigs's work with so short a notice. We may say, however, that he has written on most of the important diseases of the uterus, both functional and organic; and that several letters are devoted to subjects which are generally included in purely obstetric treatises, such as the Disorders of Pregnancy, Abortion, Flooding after Delivery, Puerperal Fever, &c. In this respect he appears to have followed M. Colombat, whose prolix and cumbrous work he translated in 1845; and as the annotations he then contributed to it formed, in our mind, the best part of the work, we were induced to urge on him to publish an original treatise on the same subjects. It required but very little knowledge of Dr. Meigs, to perceive that, if he did write at all, he would indulge in some peculiarities, if not of opinion, at least of diction; but we were hardly prepared for such a strange infringement upon the established rules of lexicography and good taste, as we have here before us. If, as a French author says, the style is the man, Dr. Meigs ought to have a large share of what is flimsy and fanciful, combined with the more substantial qualities which have made him a sound and successful obstetric physician. The book is over-garnished, and is filled with mannerisms, not unmixed with extravagance.

Of this Dr. Meigs is evidently conscious; for in his Introductory Letter to a Friend, he requests him to say whether he has "committed an unpardonable breach of the forms of our science by writing with such a freedom and *abandon.*" For our own part, we are quite willing to sympathise with even a medical author, who endeavours to lighten the labour of his readers by blending a little literary gaiety and humour with the bald facts of which his art is constructed. But while we welcome this ingredient in a scientific work, if nicely subdued, it becomes offensive and reprehensible when too conspicuous, or if it be paraded—as is too often the case with Dr. Meigs—with a garish ostentation. It is only necessary to read the first half-dozen letters, to be convinced that Dr. Meigs has some very peculiar methods of engaging the attention of the young gentlemen to whom he addresses them. At one time he startles them with a proposition, such as the following—"stroma is sex;" at another he is quite overwhelming with a torrent of fine writing—here coining a word, and there barbarizing another; and then he launches into an imaginary conversation with a patient—with some hypothetical "Miss Mary," or "my darling Helen,"—as *his* way of teaching the diagnosis and management of disease. Now, in all this there is a little personal portraiture; and we soon learn to recognise that Dr. Meigs is a gentleman of versatile talents and varied knowledge, of the most enchanting manners, well dressed, of great conversational powers, fluent, quite at his ease, and very striking; in short, a most agreeable, well-educated, good-humoured man—the very beau-ideal of an obstetric dandy. But, while noticing thus prominently these failings of Dr. Meigs, we are anxious to assure our readers that they are only on the surface; and that the body of the book is worthy of attentive consideration, and is evidently the production of a clever, thoughtful, and sagacious physician. We should, indeed, have willingly passed by these whimsical conceits of our author, in consideration of the high merit which really belongs to him; were it not that, as displayed by a teacher, in which capacity he writes the letters, they have a particular significance. It requires but little acquaintance with that miscellaneous race of young men, the students of a medical college, to know that many are more allured by the gaudy colouring of a turgid style of composition, than by the hard thought and patient observation which may be concealed beneath or obscured by it; and that many a man may live under the delusion that, in emulating the former, he has acquired the latter. But, independently of this, we should unaffectedly regret if Dr. Meigs's formula for catechising patients should be too implicitly followed by his pupils; for we think it more than an imaginary danger that such endearing titles as "my dear child," "my darling miss," &c., should be imitatively lisped, or, it may be, improved upon, by the rising race of Philadelphian practitioners, to the scandal of obstetric practice, and the offence of well-bred female modesty. Perhaps in thus alluding to this subject, we may be thought to be prudishly fastidious; but we more than suspect that the same kind of address is prevalent enough on this side the Atlantic; and we are quite sure that it ought to be discountenanced and rejected, and settled as a part of the peculiar vocabulary of that venerable body of matrons, the monthly nurses.

As a preliminary inquiry into the functional disorders of the uterus, the authors consider the subject of *Menstruation*. The recent physiological researches which have connected the menstrual periods with the spontaneous discharge of ova and the act of impregnation, are alluded to by Dr. Ashwell, and taken up with some enthusiasm by Dr. Meigs. It is to be regretted that Dr. Ashwell has not entered more fully into the subject; especially as we gather, from what little he does say, that he is at variance with the now prevalent opinions of Bischoff, Lee, and others. He relates three cases, in which he examined the ovaria of women who had died during the menstrual flow, in neither of whom was there the physical evidence of a maturated or rent Graafian vesicle; and he concludes generally, that it is by observations on the human female, and not on animals during the period of heat, that this theory ought to be established. Dr. Meigs, on the contrary, in a well-written chapter on the menstrua, thus summarily expresses his opinion:

> "A healthy woman matures, and deposits an ovum every twenty-eighth day, from the age of 15 to that of 45 years, failing only in case of pregnancy and lactation, and sometimes not even then. She sometimes suffers an arrest of the force during lactation, yet in the majority even that arrest is but of short duration, and in many it does not take place at all. The closing stage of the process of maturing and depositing, or discharging the ovum, is attended with a discharge of bloody fluid from the genitalia, which is called menstruation, because it takes place once a month." (p. 385.)

For our own part, we are quite disposed to believe that the establishment of puberty, and the flux of blood from the womb once a month, depend on the organic perfection and functional activity of the ovaries; but we have the same hesitation as Dr. Ashwell in believing that precise dogma, which affirms that an ovum is periodically cast off at each menstrual period, and that observations, which have been made on the lower animals during their œstrum, are to be regarded as strictly applicable to the catamenial periods of women. Passing by several minor considerations, there is this great practical anatomical difficulty to be yet overcome, which hitherto, we feel confident, has not been satisfactorily accomplished;—viz. the presence of a true corpus luteum when *we know*, from the fact of impregnation, that an ovum has been cast off; and the absence of any such body after the ordinary menstrual periods. We do not deny the spontaneous discharge of ova in the rabbit, sow, &c. &c., at the time of heat; or that this periodical oviposition is followed by luteal bodies in their ovaries; but we deny that any such bodies are to be seen in the ovaria of the human female as the simple result of menstruation. We have had ample opportunities of comparing the true corpora lutea, which have followed on conception in the female, with the vacated follicles or the extravasated blood—which may accompany the ovarian congestion of menstruation, and form false corpora lutea. But the two are in no notable particular alike; indeed, they are perfectly dissimilar; and, in our mind, it has yet to be shown that the human female is the subject of a periodical œstrum once a month, and that at this time vesicles are maturated and an ovum discharged, which may be impregnated during its slow progress through the sexual organs, whenever it may come in contact with the semen. We believe that about eight days is given for this transit of the ovum, which, in fact, limits the period of conception to this time after the menstrual period. And yet the Jewish women, who are bound to observe continence from sexual intercourse for just this time after menstruation, are notoriously prolific as a race! We are quite disposed to concur with Dr. Ashwell, that it is an arbitrary assumption to make experiments on the lower animals apply directly to the human female; and that, in drawing inferences respecting a peculiar function of the latter, the only observations which are conclusive, are those derived from the appearances of the ovaria and sexual organs when performing this function.

We may incidentally notice an opinion of Dr. Meigs, that the corpus luteum is a true vitellary body,—a discovery which he thus announces: "These researches," says he "suffice to convince me that the yelk of eggs and the yellow matter from a corpus luteum are of the same apparent constitution, form, colour, odour, coagulability, refractive power, and microscopic appearance." It is not our purpose to enter minutely into the observations of other physiologists on the structure of the corpus luteum, or to attempt the full refutation of this notion of Dr. Meigs; but shall content ourselves with remarking that, whatever may be the similarity in elementary composition—whether chemical or organic—between the vitellus and the corpus luteum, there can be no real analogy, either structural or functional, between them; seeing that the one is within, and the other without, the ovum; and that the ovum of the mammalia has as true a vitellus as that of the oviparous animal, the embryo being nourished by it alone during the earlier stages of its development, to which operation it is evident that the corpus luteum can contribute nothing whatever.

Dr. Ashwell's chapter on *Chlorosis*, with its complications, is very full and practical, and will amply repay a careful perusal; while Dr. Meigs's notice of it is included in a lengthy supposed conversation with a mother, about her daughter Mary, in which the Doctor allows his imagination to have full scope, but still without failing to put forth the prominent features of the complaint, with its pathology and rational therapeutics. We need scarcely observe, that this blood-disease is, by both, treated on those general hygienic principles which are now so well understood; the effect of which is, to improve nutrition, enrich the blood, and impart tone and vigour to the enfeebled constitutional powers. Among special remedies, the iodide of iron is spoken of as most valuable.

Several very interesting cases of organic amenorrhœa resulting from the absence of the uterus, and of retention of the menses from congenital and artificial closure of the vagina, are related by both our authors. The occasional success of surgical operations in opening a passage through the closed vagina, and in giving vent to the collected menstrual fluid, is well known, and is illustrated in these works; but it must be admitted that attempts of this kind not unfrequently fail in their object, and expose the patients to the great additional risk of a fatal peritonitis. Case, No. 21 (page 88), in Dr. Ashwell's book, is very instructive on this point.

The two functional disorders which we shall have to consider are, dysmenorrhœa and menorrhagia.

Dysmenorrhœa. Severe pain during the whole or part of the menstrual times is the symptom which has originated this designation dysmenorrhœa; but, as pain is the result of different, and sometimes even opposite, pathological states, the term is liable to confuse disorders; and we find this the case, to a certain extent, in the chapters of both authors. Dr. Ashwell's divisional arrangement of the subject is into *neuralgic* dysmenorrhœa, *plethoric* dysmenorrhœa, *congestive* dysmenorrhœa, and lastly he notices Dr. Mackintosh's cases of dysmenorrhœa *dependent on mechanical obstruction.* Dr. Meigs makes no division at all; but, in his own discursive mode, brings in something of them all, saying some very good things of each, and in a very descriptive way.

"The pain of dysmenorrhœa is a pain felt in the hypogastric region, in either or both of the iliac regions, in the tractus of the ligamenta rotunda, in the sacral region, in the thighs, and very frequently in the course of the distribution of the obturator nerves. This pain, not unfrequently, extends to the whole belly, and is at times insupportable, from its violence; compelling the patient to lie down in bed, or on the couch; forcing from her both tears and groans, and producing so great a degree of restlessness, that she seems to writhe like a crushed worm." (p. 435.)

In this picture perhaps the only omission is in not referring much of the suffering to the ovaria themselves; which, we think, are very commonly implicated in this morbid sensibility.

Referring to Dr. Ashwell's division, which is substantially identical with that of other authors, as Dr. Churchill, &c., we are quite disposed to concur in the appropriateness of the first and last varieties—the *neuralgic* and the *mechanical.*

The subjects and temperament of the neuralgic cases, the character of their suffering, and the absence of any active congestion of the uterus,—not to mention the direct indication, and the successful result of their treatment during the intermenstrual times, by steel, quinine, and tonics,—sufficiently characterise this division. The cases in which rheumatism has been supposed to have attacked the unimpregnated uterus, which is alluded to as a positive cause of dysmenorrhœa by Dr. Meigs and others before him, may be allied to the hysteralgic cases, and fairly included under this division. "Do you not perceive," says Dr. Meigs, "that a rheumatic womb is likely to be a painful womb during the menstrual act, and that much of the dysmenorrhœa which it will be your destiny to encounter in your future career as practitioners, will be regarded by you as a form of rheumatism?" For this disorder the ammoniated tincture of guaiacum has been highly praised by most obstetric practitioners, although Dr. Meigs has not found it so useful. He speaks in high terms of "moderate doses of the golden sulphuret of antimony, combined with camphor, and small doses of opium or morphia a few days previous to the attack of dysmenorrhœa."

Mechanical dysmenorrhœa. Without following the order of Dr. Ashwell's paragraphs, we may notice that he speaks in very equivocal terms of narrowing of the os and cervix uteri as a cause of dysmenorrhœa. He appears to us to entertain the faintest possible perception that it may just be the case; although even its half-admission in the first part of a sentence is all but smothered before he completes it. "There is no doubt," says Dr. Ashwell, "that dysmenorrhœa has occasionally coexisted with such malformation; but it is by no means certain that it owes either its origin or its continuance to such a state of parts as a cause." And although he quotes Dr. Mackintosh's cases, and others by Dr. Churchill, Oldham, &c., which were treated and cured by overcoming the strictured os, yet he still shows, in a note in the new edition, that he retains his scepticism. Dr. Meigs, on the contrary, speaks of congenital strictures and flexions of the cervix uteri, as mechanical causes of dysmenorrhœa; and he expresses, in the following terms, his experience of the employment of dilatation to overcome it, and of the relative value of incising and dilating the strictured part for this purpose.

"Dr. Mackintosh's practice has been used everywhere, and with undoubted success in cases proper for it. Some of the brethren have preferred to Mackintosh's method, the use of a concealed bistoury, which being introduced to the upper extremity of the canal, is slightly disclosed by a spring, and then being withdrawn entirely from the os uteri, has divided the lining membrane of the canal, allowing of a complete dilatation of it. When I was in London, in 1845, Dr. Locock showed me one of these instruments, with the effects of which he appeared to have been much pleased in his practice.

"If you have read this book, you must have observed in a former letter, that I strenuously opposed the use of all cutting instruments in the procuring of a requisite dilatation, upon the ground that every division must be followed by a cicatrix, which is, essentially, a morbid existence; whereas all necessary dilatation can, I am convinced, be as readily and perfectly brought about by a distending pressure, leaving the part, when the part has been thus cured, without a wound, and in a perfectly healthful and normal state. I repeat it, that a cicatrix is not a normal state; it is always insecure, always dangerous. I therefore profess my adhesion to Mackintosh's practice. My own experience in the use of the bougie, has been to a certain extent satisfactory to me; satisfactory to this extent, that it has not only, in some cases, relieved the woman from distress, pain, and other disorders of menstruation, but that it has cured her of a pre-existing sterility. A woman is likely to be sterile when her uterus is unhealthy, and particularly when it is unhealthy from narrowness of its canal." (pp. 438-9.)

So far as our observation goes in the employment of these means as practised in London, we have no hesitation in saying that they are too frequently had recourse to. The cases of stricture requiring dilatation are *very rare indeed;* and if the operation were less easy and less concealed, we doubt very much whether it would be employed so commonly as we fear it is. A patient of our own, who has been married for some years, but without having been pregnant, consulted a physician, who, on examination, discovered a small os uteri, which he at once proposed to cut open, with the alleged expectation of curing her sterility and dysmenorrhœa together. He quite overlooked, however, a fibrous tumour that projected from the right side of the body of the uterus, which had been the cause of her suffering. The os uteri in this case was normally small, not strictured; and it is highly unphilosophical to fix a certain size as a standard os uteri, and to open it up if it does not quite reach this standard. Dysmenorrhœa, with a scanty flow of the menses, is sometimes associated with an imperfect development of the entire sexual system; the uterus, like other organs, remaining small. Sterility necessarily coexists with this condition; but the indication here is not to open the small outlet of the womb, but so to invigorate the constitutional power as to excite this important system of organs to more complete evolution. We are ourselves disposed to prefer the dilating process to incision with a bistoury, when the case demands this mechanical treatment; and our own plan has been to pass caoutchouc or silver stems, which are perforated so as to receive an ordinary uterine sound through them, and gradually to increase their size. When passed through the os uteri internum into the cavity of the body of the uterus, the sound may be withdrawn, and the stem left for half an hour or more within the canal. By a repetition of this process, a contracted os uteri may be made patent; and both dysmenorrhœa and sterility occasionally cured.

It is in the two intermediate varieties of dysmenorrhœa, that we think Dr. Ashwell has rather confused this subject. Why should plethora or local congestion be made to designate particular forms of dysmenorrhœa? They may exist in amenorrhœa: they may be present in menorrhagia: they may be found without producing dysmenorrhœa. It appears to us that the existence of these conditions is altogether secondary, and subordinate to another morbid condition, which can only be rightly interpreted by examining the membrane that is exfoliated in these cases. Both our authors notice this membrane, and the spurious abortions which occur when it incloses a clot of blood. Dr. Ashwell speaks, in the text (p. 109), of a "modified and low form of inflammation, which produces a false membrane assuming, in some instances, the shape of the uterine cavity, and in others being expelled in detached portions." Dr. Meigs, too, calls this membrane "a plastic deposit like the plastic deposit in croup, which proves the existence of an inflammatory state;" so that both our authors consider the membrane as a false inflammatory membrane, just as the Hunters regarded the decidua vera as a lymphy product. But the important and interesting investigations which proved the decidua vera in pregnancy to be nothing more than a growth of the uterine glands, transforming the thin transparent mucous membrane of the uterus into a rich, soft, raised, vascular, cribriform structure, gave a clue to the pathological comprehension of the membrane cast off in dysmenorrhœa. Denman and Dr. Montgomery have very well described the physical character of this membrane; although the differences pointed out by the latter between it and the decidua vera, which are quoted at length by Dr. Ashwell, may, we think, be rather ascribed to different degrees of development, than regarded as disproving their common origin. There cannot, we believe, be a doubt, in accordance with late investigations, but that the decidua vera of pregnancy and the dysmenorrhœal membrane are structurally identical. And if so, we may gather,—1. That, as in pregnancy ovarian congestion is followed as a sympathetic action by *engorgement of the uterus,* and by the rapid development of the *uterine mucous membrane,* so in membranous dysmenorrhœa, a primary morbid irritation and congestion of the ovary calls forth a local determination of blood to the womb, *congesting* it, and occasions a similar enlargement of the uterine follicles:—2. That as the mucous membrane of the uterus, when thus altered by the new glandular growth, never shrinks again, but is cast off as a decidua, leaving the uterus bared of mucous membrane until it is reformed, so in dysmenorrhœa, this change of the membrane is necessarily followed by its exfoliation, whether in shreds, or films, or more perfect moulds; and that this detachment, or as Müller has described it, this moulting, of the mucous membrane recurs again and again at the monthly period. We might easily follow out the analogy between the two, into the kindred sympathetic affections which attend on each, were not our object at present to correct what we believe to be a somewhat singular misconception of these views on the part of Dr. Ashwell. In a note Dr. Ashwell refers to the original and identical opinions of Drs. Simpson and Oldham on this subject, which he characterises as new and extraordinary, and he says that—

> "One would require rigid demonstration before admitting as true that every month, for many years in succession, the uterus, even with all its extraordinary endowments, should possess the astonishing power to cast off and reproduce—in the short space of a fortnight or three weeks—its entire mucous coat. I would ask the author in what way he supposes this almost miraculous process of separating the mucous from the subjacent muscular tissue is accomplished? I have not forgotten that the union of these structures is so firm, that the most dexterous anatomist cannot separate them uninjured, even by the knife. What may we not

anticipate in reference to this wonderful organ, after the new views of menstruation, and these startling opinions?" (p. 110, *Note.*)

We trust that what we have said above supplies the simple answer to Dr. Ashwell's question, and will relieve him from the suspicion that these opinions of the formation of the dysmenorrhœal membrane rather overtax the womb, or invest it with miraculous and wonderful endowments. Had he only remembered that it is of the mucous membrane in its *changed and altered state* that it is declared to be separated and cast off, he could not have fallen into such a palpable error as to confound it with its removal by dissection when clean, thin, and closely attached. He might as well affect to ridicule the spontaneous desquamation of the epidermic coats of the fingers and knees occurring after scarlet fever, because in the natural state this process cannot be accomplished by the knife.

Women who are the subjects of membranous dysmenorrhœa, are liable to have the womb permanently bulky, and eventually displaced. If in the early stages a copious menstrual flux relieves the attendant engorgement, this complication is retarded or even prevented; and the patient is in a far better state than when a gradually augmenting solid hypertrophy accumulates in the body and neck of the uterus. "The woman's womb aches," says Dr. Meigs, "until nature bleeds her." And we may add, that if nature does not bleed enough, local depletion, by leeches or cupping, with hot hip-baths and mild mercurials, are our principal therapeutic means.

Menorrhagia. Dr. Ashwell's chapter on this subject is more comprehensive than the clever and succinct one of Dr. Meigs, and includes two principal forms of the disease, which are again subdivided. The first form is "Profuse menstruation, either as to frequency of return, or the amount of the secretion, or both, without uterine bleeding." The second form is "Profuse menstruation, accompanied by direct loss of blood from the uterine arteries." Without entering fully into the subject, we may notice at the outset a discrepancy in the opinions of the two authors; which consists in Dr. Ashwell regarding the blood which flows and coagulates as it flows, as something superadded, and distinct from the ordinary catamenial fluid; while Dr. Meigs, more correctly we believe, regards the menses as blood altered only by its admixture with vaginal mucus. We cannot help transcribing Dr. Meigs's first paragraph, which is quite *ad rem.*

"The word menorrhagia means immoderate flow of the menses; which flow consists of a purer blood than that of the regular catamenial evacuation. The blood is purer, because the impetuous haste of the discharge causes it to come off less mixed with cervical and vaginal mucus and epithelium, than in the more moderate or normal mensual hemorrhage;—and this, I believe, is the only difference between them. As the blood in menorrhagia is less mixed and combined with foreign matters, you would naturally expect to find it to coagulate more readily; while this is quite true, it is not the less true that the real normal menstruous excretion does, in many women, coagulate, without any suspicion of menorrhagia; and it is reasonable *à priori*, to suppose so, since one woman may be seven days getting rid of her six ounces of fluid, whereas another woman will part with ten or twelve ounces, in three or five days. She who discharges the menses *pleno rivo*, will be sure to find some coagula; she who passes it slowly away, will find it to sink into her napkins without a clot,—for her blood has time to be much combined with mucus and epithelium." (p. 424.)

The pathology and treatment of menorrhagia are well discussed by both the authors; although we must own our surprise at Dr. Meigs's treatment of a young lady, æt. 18, whom he allowed to flood on during the whole night, "suffering her to faint again and again," before he would use the tampon. His commentary on the case is "I do not regret that I allowed her to bleed so much and so long. I would permit another young lady to do the same thing; because, having confidence in the power of the tampon to suppress *such* a flooding, I would let her go very far towards a dangerous state, rather than subject her to the mortification of the surgical intervention." We must say that we have rarely met with such a cool determination. Surely Dr. Meigs grievously underrates the serious results of such a loss of blood in so young a subject,—the chances of subsequent feeble life and health, with the danger of the invasion of organic disease: to risk it all for a mere feeling, which any obstetric physician ought to know how to mitigate or overcome,—when he confessedly has the means in his power to arrest it. And this is what Dr. Meigs calls being thoughtful and circumspect, and not "snapping at a conclusion like a duck at a June-bug"!

We pass by the chapters on Leucorrhœa and Hysteria in Dr. Ashwell's work, with simply the remark that, as our more precise knowledge of disease has shown leucorrhœa, in the varieties of discharge mentioned by Dr. Ashwell, to be mainly symptomatic; so it would almost have been better had the author in this new edition either greatly compressed it, or sunk it in the description of the structural disorders which it accompanies. Dr. Meigs does not consider leucorrhœa separately.

On the organic diseases of the womb. As an important practical preface to the consideration of the special organic diseases of the womb, Dr. Ashwell gives a chapter with the very comprehensive title of "General remarks on the history, symptoms, diagnosis, pathology, and prognosis of the organic diseases of the uterus." It appears to us that this chapter, for the student at least, wants both condensation and precision. It is discursive, running over, in an allusive manner, various points of pathology, &c., and omitting some of the most important elementary principles of diagnosis.

The second section is on the "Examination by touch;" and this, which is a principal source of diagnosis, is defective in that form of precept which is most wanted as a clinical guide to the student or practitioner. Dr. Ashwell details only one mode of examination, so far as the position of the patient is concerned, viz. when in the "recumbent posture, on her back, and without stays, the shoulders elevated, and the lower extremities flexed upon the trunk;" and this mode of diagnosis is applied only to one class of cases connected with the body of the womb, viz. those in which there is increased bulk; and these are illustratively supposed to be confounded with pregnancy. Even taking this example of bulk, no one knows experimentally better than the author, that the physical diagnosis between tumours of or in the womb and pregnancy, and between one tumour and another, is greatly facilitated by the patient being placed in different positions, according to the circumstances of the case; that what may be ascertained in one position with one finger, may be overlooked in another position with another finger, and so on. It is true, that all which is to be learned on this subject is, in one way or another, expressed or implied in detached observations throughout the work; but our objection is, that they are not to be found in a section devoted expressly to the subject, and that this section is proportionately obscure. If we might venture to make a suggestion to Dr. Ashwell, it would be, in any future edition, to put down in so many paragraphs the mode of examining patients,—in the recumbent posture on the back,—on one or other side,—when the pelvis is inverted,—and in the standing posture, with the advantages, whether direct or incidental, which are to be gained by each; and in doing so, not to be afraid to express plain rudimentary facts. We think, too, that the kind of running commentary which he has made on the diagnosis of pregnancy, ought—at least for the purposes of instruction—to be so rearranged, as to present the marks of diagnosis in a more lucid and appreciable shape. The subject itself is almost too extensive to be discussed in this way; and the details of diagnosis, for practical purposes, ought to be fully described, or so grouped together as to present the points of contact in as defined a way as the varieties of tumour or solid enlargement will allow. The evil is, that if the description stops short of these details, or includes them only in part, it is, so far as its instructive adaptation for clinical purposes is concerned, rather embarrassing than otherwise; and we must confess, without criticising this section of the chapter more minutely, that such is the impression which its perusal has left on our mind. The remarks on the diagnosis of diseases of the os and cervix uteri, especially the paragraph on the congenital differences in these structures, are interesting. The author remarks, in reference to vaginal investigations, that "prolonged examination is generally unnecessary; and without arbitrarily limiting the time, such an inquiry ought never, as a general rule, to exceed a few minutes,"—an observation, in the truth of which we fully acquiesce.

The use of the speculum and stethoscope, or, in other words, the mediate use of the eye and ear, are discussed as important auxiliaries to the diagnosis of uterine disease. The author does not speak of the best *position* of the patient for the employment of the speculum; nor does he give any particular preference to one kind of speculum over the other. He thinks, however, that there is no need of division in the cylinder, and that the complicated screw is not required: and he certainly rather bewilders the individual choice of an instrument by saying, that "the best and most easily used speculum is made of tin, pewter, steel, or glass." We might, we think, add yet another, in which a glass tube is covered externally with caoutchouc and a bright surface (Mr. Fergusson's speculum, in short); which, in our experience, exceeds all others in its cleanliness, simplicity and cheapness of construction, and easy application. Its introduction is greatly facilitated by the addition of a smoothly-rounded wooden stem, which, when passed through the cylinder, projects beyond the margin, and prevents the edge either catching or hurting the structures at the vulva. Dr. Ashwell, while fully alive to the great advantages which the speculum offers in the diagnosis of uterine disease, and the precise application of remedies, is yet anxious to damp that enthusiastic use of it which some individuals encourage. He considers that the "hymen in the young, and the shrinking and contraction of the sexual passages in old age, forbid its employment." There are unquestionably some affections of the cervix and os uteri in virgins, attended with a copious leucorrhœal discharge, which admit of treatment by the speculum without injuring the hymen, on account of the dilatability which a long-continued leucorrhœa imparts to the sexual passages. But we quite think that any man is guilty of a most reprehensible and indiscreet zeal for the employment of this instrument, who thinks fit to divide the hymen, in order to pass it; and we make this observation designedly, as we see that such a proceeding has recently found an advocate in a contributor to a weekly journal. We have never yet seen a case, which appeared to us to justify in the slightest degree such an operation; and we much doubt whether all the benefit of local remedies might not be obtained by administering them in solution with a syringe, or in small suppositories.

On the employment of the stethoscope as a means of diagnosis, Dr. Ashwell only refers to the similarity of sound produced by the pressure of a fibrous tumour on the iliac arteries, and the placental souffle; and the certain value of the sounds of the fœtal heart, as a sign of pregnancy. We might perhaps have anticipated, from a gentleman of Dr. Ashwell's experience, some remarks on the sounds of adhesive bands in the abdomen, and the gurgling noise of the intestines; both of which have practical reference to the diagnosis of ovarian and fibrous tumours.

The uterine sound or bougie is not mentioned in this chapter by Dr. Ashwell, and is altogether omitted by Dr. Meigs; and it is only when speaking of retroflexion of the uterus, that Dr. Ashwell gives his opinion of it. This opinion is very adverse to its general adoption; and the instrument is discarded perhaps too summarily, we might almost say impatiently, by the author. Our own experience of this metallic sound leads us to regard it as one of the most hazardous instruments which has ever been invented; and we think that Dr. Simpson and those who have followed him have something to answer for, in having set forth all that can be said of its real or supposed good, without a corresponding reference to the harm it may accomplish. We have found a very cautious use of it of value in the diagnosis of pelvic tumours; but then it is liable to the chance of puncturing an ovum in the course of these investigations; and we unhesitatingly declare, that to pass it within the uterine cavity, and explore the size, and restore any misdirection of this organ, is very often a painful operation, and is sometimes followed by metritis or pelvic cellular inflammation. The dangers, too, of such an instrument are immeasurably increased by the facility with which it may be passed; and it appears to us to need a far greater habitual caution in its use, than has been thought sufficient by its advocates. It is impossible to be blinded to the fact, that, within the last few years, novel methods of diagnosis and treatment of uterine disease,—almost exclusively of a mechanical kind, have been lavishly and hastily pushed into practice; and although they are very attractive to the inexperienced and enthusiastic, yet, naturally enough, they are looked upon more coldly and cautiously by those, who, like our authors, have learned to weigh the good with the evil which is beside it.

On fibrous tumours of the uterus. Dr. Ashwell distinguishes two varieties of these tumours, classifying them according to their locality: the first class including those which project externally, and carry the peritoneum before them; and the second those which are submucous, and in their growth become invested by the mucous membrane, and deform the cavity of the uterus. This distinction is obviously practical; the former being harmful principally by their mechanical interference; the latter, by the hemorrhage they produce. There are some one or two points connected with these tumours, on which Dr. Ashwell entertains some peculiar opinions. He regards them, for instance, as belonging to "the scirrhous variety of carcinoma," and assigns the following reasons for this view:

> "1. They possess the structure of compound adventitious cysts, the basis of this class of heterologue formations. 2. In the colour of the contained mass, and in the arrangement of the membranous septa or bands, the containing tissue, they are identical with scirrhus. 3. In hardness, occasionally justifying the application to them of the term stone cancer, they are not to be distinguished from the varieties of carcinoma already mentioned. 4. They occur very frequently in conjunction with growths of undoubted malignancy in other parts of the uterus. 5. And, lastly, they possess one especial attribute of malignancy, incurability." (pp. 289-90.)

If, as Bayle declares, the fifth part of women above 35 years of age have fibrous tumours, it certainly assigns, on Dr. Ashwell's supposition, a fearful preponderance of malignant disease to the female over the male sex. We are by no means convinced, however, of their malignant character, by Dr. Ashwell's reasons; and we are more disposed to concur in thinking that "the sum of these reasons does not endow these growths with power to assimilate different tissues to their own substance, nor with the capability to produce that peculiar cachexia by which cancer destroys life."* Perhaps any one who was anxious to establish a general law, that compound adventitious cysts form the basis of cancer-growths, might call the white septa of a fibrous tumour the cyst-walls; although in so doing, we confess that he would have to draw somewhat on the credulity of those who were indifferent to the support of the theory. With our present knowledge, however, of the spread of true cancer by cells, it would have been better to have obtained a microscopic analysis of these tumours; when we think our author would have found them to be composed of a fine unstriped fibre, just like the muscular structure of the uterus itself, without the cells of malignant disease. It appears to us, too, that the fact of their never returning when they have been enucleated, or removed by ligature when polypoidal, or spontaneously detached,—of their enormous volume in some instances,—and the number of them in other specimens,—besides the compatibility of their existence with good health,—and the freedom of the adjoining structures from cancerous infiltration,—are very decisive proofs of their not coming within the range of malignant diseases.

When speaking of the submucous fibrous tumours, Dr. Ashwell denies the fact that "a genuine hard fibrous tumour ever becomes a pediculated polypus;" and he recounts several marks of difference between them. "In *structure*," says Dr. Ashwell, "with some few exceptions, in *sensibility*, both in the growth and the surrounding parts, and in *vascularity*, as well as in *many other particulars*, there is a marked difference between the hard or fibrous tumours of the cavity now described, and polypus." We may mention incidentally, that Dr. Meigs differs from this singular opinion of Dr. Ashwell's, and speaks familiarly of the fibrous polypus. We are not a little surprised to find Dr. Ashwell opposed to what we conceive to be a most familiar morbid demonstration; viz. that some polypi are nothing more than fibrous tumours, which, by the action of the uterus have descended lower and lower in the cavity of this organ, dilating the os uteri in their descent, and at length escaping into the vagina or even beyond the vulva. We speak at least from some knowledge of the Museums in London, and with the confidence of personal experience too, when we say that amongst the polypi which have been removed and preserved, such specimens are common; and that their structure is simply the structure of a fibrous tumour and nothing else. Such a tumour has a stalk, more or less thick, of uterine tissue; and an investment either of mucous membrane only, or with a thin shell of the muscular wall of the womb, according to its original position. That such a tumour has structural peculiarities which make it differ from other varieties of polypi, will readily be conceded; and Dr. Ashwell's subsequent observations appear to be based on this admitted fact. "The internal tissue of many polypi," says he, "is spongy and cellular, and copiously permeable by blood; a circumstance scarcely ever appertaining to a hard or fibrous tumour." In this, as in other remarks, our author appears to us to confound the distinctions which mark the varieties of the same disease, with those positive characteristics, either of structure or attendant symptoms, which alone ought to justify their separate classification. A fibrous tumour which has in its descent acquired a stalk, is a bleeding growth, the blood flowing principally from its pedicle: a tumour made up of loose muscular tissue, with large arteries and veins permeating it, and the latter opening on its surface, is another tumour which bleeds like the former one: a third class, originating in the glands of the body or neck of the womb, whether appearing in pendant clusters or with deep channels within them, are bleeding uterine tumours, differing as much in their intimate structure from the second variety, as that does from the fibrous tumour; and yet they all alike are polypi, and admit of cure by removal. Our own experience as to the relative frequency of these varieties, if we except those little nascent polypi from the cervix, is in favour of the fibrous tumour which has been converted by descent into a polypus.

---

* Vide page 290.

"The want of sensibility," says Dr. Ashwell, "is an almost invariable condition of true polypus; while the hard or fibrous tumour is never entirely bereft of sensation." On this supposed distinction we are bound to say, that if a fibrous polypus is sensitive, which it very rarely is, it is only because it has carried with it in its descent a portion of the uterine structure which still retains its organic sensibility. Generally, however, we are experimentally convinced that no such sensibility is apparent; and that the growth may be cut or pricked with a pin, or noosed by a ligature, without the patient being conscious of pain.

The non-vascular character of fibrous tumours is assigned as a mark of difference between them and polypi. That some polypi are more vascular than others we readily admit; but Dr. Ashwell can hardly have examined the preparations in Guy's Museum attentively, if he denies that fibrous tumours are susceptible of artificial injection. "In the Museum of Guy's Hospital," says Mr. S. Lee,* "there are three preparations illustrating this fact (the vascularity of fibrous tumours); in all these cases the injection has penetrated the morbid mass." The number and size of the blood-vessels, principally arteries, in fibrous tumours, vary extremely in different cases; and are in a great measure proportioned to the condensation of the tissue, and the degree of its infiltration with calcareous matter. We have seen large-sized fibrous tumours very vascular; the trunks of the vessels running in the septa, and dividing freely and minutely in the unstriped fibre of which they are composed; while a belt of largely developed veins surrounds the growth, which rarely can be traced into the tumour itself.

---

* On Tumours of the Uterus, page 8.

The coexistence of fibrous tumours with pregnancy, which "rarely, if ever," occurs when there is polypus, except where the growth arises from the cervix or os, is cited as a diagnostic difference between the two. On this point we would say, as a general observation, that pregnancy is prevented in both cases; although, if there be no mechanical impediment to the passage of the semen through the tubes, it may by chance occur in either. It appears to us that Dr. Ashwell's assertion of the excessive rarity of polypus in the cavity of the uterus with pregnancy, ought to be modified; for although as a complication of pregnancy it is no doubt unfrequent, yet several cases are on record, which would bear no mean proportion to those related of the same complication from fibrous tumours on the walls of the uterus.

We have thus briefly argued this point with the author, as it is a matter on the right interpretation of which our prognosis in the submucous variety of hard tumours is in a measure founded. In cases of this kind attended with dangerous bleeding, we may at least hold out the expectation that there is yet a chance of cure left in the descent of the tumour, so as to bring it within reach of a ligature. The hemorrhages in this variety of growth are most formidable; and Dr. Ashwell speaks of "palliation in most being all that can be expected." He refers, in the chapter on polypus, to the subject of their enucleation; but only, however, to deride it; and on the same point of practice Dr. Meigs speaks of its feasibility, when the growth is in the tissue of the cervix,—where, however, we believe it to be very rarely indeed implanted.

In the treatment of fibrous tumours, the two authors are at variance. Dr. Ashwell speaks in high terms of iodine, which he believes in most cases to be equal to the restraint of the growths; while Dr. Meigs talks of medicine in the following terms.

> "People talk of taking medicines for such tumours, and they even take homœopathic pellets in decillionths of grains! Leaving out of question the unspeakable nonsense of the homœopathic dosings, I see not on what ground they should take even real physic for such complaints, since drugs cannot, and were never designed to heal such tumours and make them return under obedience to the natural-development laws of the organ, and restore its outline and contexture to a normal form and dimensions." (p. 265.)

We must confess ourselves to have been greatly disappointed in the employment of iodine or the iodide of potassium, which in no instance, out of very many in which we have administered it, has perceptibly diminished or even stayed the progress of the tumours. More good is effected by general means; such as by regulating the diet and exercise, and keeping down by local depletion the tendency to uterine congestion. If there be a medicine which, more than another, will accomplish this diminution or arrest, it is, we confidently believe, the bichloride of mercury, given in small doses, in connexion with tonics, for many months together; but we have never witnessed any such reduction in their size by any plan of treatment whatever, as to encourage us to hope that they are capable of removal by any means short of their extirpation. We may incidentally notice an ingenious idea of Dr. Simpson, by which he expected to favour their consolidation, and so stop their increase; by transfixing them with metallic pins, which would keep up an electro-galvanic current through them. Dr. Meigs also talks of tying the uterine arteries, and shaving off the utero-cervical ganglion of nerves. Dr. Simpson's treatment has been tried, but we believe without any marked success; and, like some other plans of his, we fear that the remedies may be worse than the disease. As for Dr. Meigs's theoretical suggestions, we fancy they are likely to remain only in the page on which they are transcribed.

We must refer our readers to a very admirable chapter in Dr. Ashwell's work, on the induction of premature labour in pregnancy complicated with organic disease. Dr. Ashwell has the merit of having extended the adoption of this simple plan of treatment to cases in which fibrous tumours imbedded in the uterine walls coexist with pregnancy; and his principal reasons for doing so are, that the danger arises chiefly from the injuries which the growths themselves sustain, which lead on to their inflammation, softening, and suppuration; and that by shortening the term of gestation artificially, these morbid actions are not so likely to supervene.

> "To establish its propriety, it is necessary to prove two or three positions, amongst which, I may especially mention the following:
>
> "1st. That when death occurs, after a labour so complicated, the result is only slightly, if at all, referable to the uterus, which rarely sustains any serious mischief; but is mainly produced by inflammation, softening, and unhealthy suppuration in the growth itself;—these pathological changes leading, in some instances, to rapid sinking; while in others, the powers of the system having been less impaired, death ensues in a few days, from the constitutional collapse, induced by the protraction and difficulty of parturition, and by the contusion and injury done to the tumour and other soft parts. And, 2dly, that premature parturition, artificially induced, rarely occasions constitutional mischief, is easily accomplished, and affords the best, and in many instances, the only chance of a safe result to the mother." (p. 323.)

There are some interesting cases appended to this chapter, which is altogether one of the most instructive and original in the work.

Cancer of the uterus. We shall not follow our authors into their description of this disease; but refer only to their opinions on the important subject of its curability. Dr. Montgomery, it is well known, entertains the belief that cancer in its early stage, when the hard deposit has collected around the glands of the cervix, elevating them into shot-like bodies, may be cured; the principal remedies being rest, abstinence from sexual intercourse, mercury to touch the gums, iodine both internally and externally, and local depletion. On this point Dr. Ashwell concurs generally with him. Dr. Meigs's opinion is thus stated:

> "I have certainly met, in the course of thirty years, with several cases of diseased uterus, which I had the greatest reason to suppose cancerous, but which yielded to persevering treatment, and ended in the perfect recovery of health.
>
> "Far be from me the intention to proclaim that I have been more fortunate than my brethren, and that I have cured cancer of the womb. My desire is to say, that I was mistaken in my diagnosis, and that I treated a curable and not an incurable malady. I am of the opinion that everybody holds on the subject of this terrible evil, viz. that it is one of the *opprobria medicorum*, and that it cannot be cured." (p. 281.)

Dr. Meigs believes that the deposit of the malignant cells is preceded by chronic inflammation and induration. "I cannot bring myself to believe," says he, "that cancer ever commences anywhere as a punctum saliens of disease. There is always an antecedent state of alteration of tissue; a state which lays the foundation for the wild evolution and increase of cells, out of and beyond the control of the accretive and waste laws of the part or organ."

On the employment of iodine in the early stage of cancer, Dr. Ashwell says "that the external application of this drug to the cervix is sufficient to secure its beneficial effects, *especially when the friction is persevered in for ten or twelve minutes.*"

We have ourselves added the italics to this injunction; as we feel bound emphatically to express our dissent from this mode of employing iodine. We should decidedly object to rubbing a stimulating ointment on a part, which, to be cured, requires to be relieved from every source of excitement and congestion; and it appears to us that the act of *friction* for ten or twelve minutes is sure to excite the aphrodisiac sense, and that, more than probably, it might engender revolting and pernicious habits. It is hardly necessary for us to do more than allude to this; and it is only the sanction which a name of Dr. Ashwell's respectability gives to the practice, which prevents our using stronger language in discountenancing it. Surely Dr. Ashwell may obtain the full effects of the local application of iodine, by the simple prescription of vaginal suppositories.

On excision of the neck of the uterus. Dr. Ashwell yields apparently a reluctant and rather wavering assent to the occasional use of this expedient in early cancer; "opportunities may occasionally present themselves for its justifiable performance." Dr. Meigs, on the contrary, concurs in the expression of an eminent surgeon of Philadelphia: "If the cervix was cut off, and the woman recovered, it affords the most incontestable proof that the operation was unnecessary." We believe that those who have either witnessed or performed the operation of the entire removal of a carcinomatous cervix uteri, will generally be very indisposed to repeat it; as it is one difficult to accomplish, dangerous from the attendant hemorrhage, and rarely, if ever, successful. On the propriety of attempting to remove the whole womb, it need only be observed, that Velpeau collected twenty-one instances, of which not a single one was permanently cured.

We have so lately commented at length on ulcerations of the os and cervix uteri, in our notice of Mr. Whitehead's book on Abortion, that we have no need to refer to the chapters on this subject by our authors. We merely transcribe, without comment, the following opinions of Dr. Ashwell, in opposition to those of Dr. Bennet:

> "In concluding this part of the subject, I may remark that Dr. Bennet regards 'ulceration of the cervix as common with pregnant women.' This, probably, if at all, is only true of the lowest class of females. My own experience is entirely opposed to such an opinion, as well as to another of the same author (pp. 37-8), 'that laceration followed by ulceration, is a frequent occurrence in the first stage of labour,'—a statement entirely at variance with the acknowledged fact, that nature has the power to effect the completion of her own work. Nor can I regard with more favour the conclusion, 'that the state of pregnancy predisposes to inflammation and ulceration of the cervix uteri.' It may be so in prostitutes, and patients suffering from primary and secondary syphilis, in whom pregnancy is happily not a

common event; and perhaps it may not be regarded as savouring too much of criticism, if I say, that many of the observations of French writers, and even some of our own countrymen who adopt the sentiments of the Parisian school, must be received with great caution in these matters. They are too prone to generalize from the observations they are permitted to make on one section, and that the lowest, in female society. Thus it happens, that the morbid peculiarities of women of abandoned habits, are not infrequently regarded as attaching to the far more numerous class, with whom they have nothing but their sex in common. Hence, also, the too indiscriminate, and often injurious use of the speculum,—the abuse of which has thus far delayed its necessary and justifiable adoption in this country." (p. 433.)

Amongst the affections of the mucous membrane of the cavity of the uterus, Dr. Ashwell includes *physometra uteri*, a disease which he believes may exist as a primary affection, "the air being secreted by the extensive branches of the uterine vessels;" although he has never met such a case. Dr. Meigs, who has devoted a letter to the subject, is perfectly incredulous about it; and the first paragraph of the letter is so characteristic of the author's unambiguous way of expressing his opinion, and in our minds so perfectly true, that we cannot avoid copying it:

"Gentlemen,—If you will look into the books, or listen to the relations of your patients, you will, perhaps, be led to believe that the womb is occasionally to be found distended with air, which, after having caused it to expand, until it attains the size of a womb six months gone with child, more or less suddenly escapes; whereupon the signs of the woman's pregnancy disappear, to the great astonishment of the hopeful patient, as was the case in the celebrated instance of Mrs. Commodore Trunnion, of whose baby the author said, '*tenues in auras evadit.*' These ventose pregnancies are nonsense, and no thoroughly-bred, and close-thinking physician ought to be for a moment misled by such a story. It is against physiology; it is against pathology; and it flies in the face of common sense, to talk of collections of wind distending a material like the womb, a material which creaks under the edge of the bistoury, and expanding it like a normal ovum, whose gentle slowness of growth is the sole reason for the deployment of the gravid uterus." (p. 288.)

Vesicular moles or hydatids of the uterus. This disease, which is now well known to be a transformation of the tufts of the exochorion, is allowed by Dr. Ashwell to exist in women, quite independent of impregnation or sexual intercourse. Dr. Meigs, on the contrary, affirms, "that he has never heard of hydatids in the virgin, nor does he believe that a virgin could have an hydatid or anything like it developed in her womb." It is perfectly obvious that, if the exochorion be formed upon or from the zona pellucida of the ovum (as Bischoff maintains), as the result of impregnation, it is quite impossible that an hydatid degeneration *of it* can occur in a virgin. Dr. Ashwell is indeed constrained to have recourse to a membrane, which he calls "opaque, and similar to the decidua, and connected to the uterine vessels," to account for them. We fear that the doctor is hardly aware that he is describing the altered mucous membrane of the uterus; which as a dysmenorrhœal membrane he rather scoffs at, but which he unconsciously admits, and really describes pretty accurately, when he wants it for another purpose. If there be any such bodies as vesicular moles in the virgin, they unquestionably result from the expansion of the decidual glands, broken up into fragments, and expelled with some menstrual blood from the womb. We have ourselves seen such masses; but they cannot for one moment be confounded with the dilated exochorion cells, which form the well-known hydatid mole.

In the prognosis of this disease, Dr. Ashwell speaks of the exhausting effects which may result from the attendant hemorrhage; but he does not notice the possibility of death from internal hemorrhage without a trace of bleeding externally. We have ourselves witnessed an instance of the kind, in which a female died with all the signs of internal hemorrhage; on examination, the uterus, which was supposed to be quite in an early state of pregnancy, reached as high as the umbilicus, and formed a tense hard sac, rather triangular in shape from the stretching out of its cornua; and on opening it, the cavity was quite filled with a mass of hydatiniform exochorion tufts, floating in a large quantity of partly fluid and partly clotted blood, none of which had passed the os uteri internum. The uterus, however, very rarely fails to expel these bodies.

Displacements of the uterus. The displacements of the womb, downward, backwards, and forwards, with its inversion, are severally described by the authors.

Prolapsus uteri. We shall only refer to this displacement, which is fully treated of by Dr. Ashwell, and very graphically described by Dr. Meigs, to allude to the employment of the pessary, which is advocated with positive enthusiasm by Dr. Meigs, and with a very strong predilection for it by Dr. Ashwell. The latter author says that he has used it for the last twenty-six years, and that "he has met with very few instances out of a great number where it could not be introduced, and none where its employment occasioned permanent mischief." Dr. Meigs's allusion to it in the following paragraph, is to the purpose, and is so characteristic of his style that we are induced to transcribe it:

> "As to my own preferences in the matter of pessaries, I have only to say, that whatever may keep the womb at its due height without irritating it, or incommoding the other organs and textures implicated in the descent, is a good pessary; but I deem the globe the most perfect and most suitable for the ordinary simple cases. An instrument of two inches, pressed upwards to the uterine extremity of the vagina, lifts the womb high enough; higher, in fact, than the position occupied by it in eight out of ten of those women who have had a child, and that is high enough. It is kept up by the double and consentaneous actions of the sphincter vaginæ muscle, and the levator ani. It has no angles, no sides; it cannot be displaced, save by being ejected. Its pressure is uniform over its whole superficies, save where its lower segment looks down the tube of the vagina. This cannot be so truly said of any other instrument, if we except, perhaps, the gum-elastic bottle, used and recommended by Dr. Hervez de Chegoin, in his paper in the 'Mém. de l'Acad. Roy. de Méd.' The globe has a perfect polish, and an unoxydizable surface. It may be worn a year or more without displacement, if required; and it has no aperture to admit of the collection of putrescible materials within it. It does not prevent the escape of the mucus of the uterus and vagina, nor of the menstrua; and, in short, is the least uncomfortable and most perfect of instruments. It is a true suspensory, as needful for the descended womb as the suspensor scroti for a hernia humoralis or orchitis. It is as neat and perfect in its kind, as Petit's tourniquet in its kind, and as indispensable for the cure. Dr. Physick used to tell us in his Lectures, that a man might as well attempt to improve the Bible as Petit's tourniquet, and I, after him, repeat, that you might as well attempt to improve Petit's tourniquet, as Physick's globe pessary. But you know I always have warned you to be *Nullius addictus in verba jurare magistri*, and so I say, judge of it for yourselves." (pp. 172-3.)

We must say, that personally we do not sympathise with the authors in their excessive admiration for pessaries; and we are not a little surprised to hear that Dr. Ashwell has never known them do permanent harm. We are quite sure that neither of them has given weight enough to the fact, that partial descent of the womb is a very common result of congestion and hypertrophy of its body and neck; and that a weakened vagina is by no means so exclusive and proximate a cause of it, as Dr. Meigs professes to believe. We have seen pessaries of box-wood and caoutchouc of various shapes, used very extensively; and they have left on our minds the impression that they are clumsy, filthy, and odious contrivances, exciting great irritation in the cervix uteri and vagina, followed frequently by fetid discharges. They are often very difficult satisfactorily to adjust, and sometimes more difficult to remove; and we quite concur with the late Dr. Hamilton in believing that they rarely produce, even at the best, more than a palliative effect. And in proportion as in practice we have discarded the use of vaginal pessaries, so have we adopted a form of external support, in the shape of a more or less deep perineal pad, fixed to the edge of an elastic abdominal belt. That this is not equal to keeping up an uterus which has long been procident, or which, from the excessive relaxation of the sexual passages, becomes so on the slightest exertion, we freely admit. In such cases, a well-made stem-pessary, such as that described by Dr. Meigs as Blundell's pessary (vide p. 181), will be found necessary. But our observation applies to the general run of cases of prolapsed uterus; and we confidently affirm, in spite of Dr. Meigs's pedantic theorizing,* that very great relief, and, with the concurrent aid of astringent applications, frequent permanent cures, are effected by them.

It is certainly very perplexing to notice the diversity of opinion which two practical physicians, like the authors, entertain occasionally on the use of the same thing; and in illustrating this, we would refer to their views on the sponge pessary. Dr. Ashwell says of it: "I know only of one case in which it should be used,—where there is excessive irritation and tenderness of the vagina;" while Dr. Meigs "protests against the sponge—most particularly against the sponge."—"I protest," says he, "against its employment as a pessary, not only on account of its irremissible fault of uncleanness, but for the additional objection of its irritating nature; the points of the sponge could not fail to vex and fret the mucous tissue of the vagina." We must say that we have always found it a most irritating substance; and we should, from our own experience, make a point of excluding it in the cases which Dr. Ashwell has selected for its employment.

Dr. Meigs assures us, from repeated observations, that the globe pessary does not prove a bar either to conception or gestation.

Retroversion of the uterus. This accident, as an accompaniment of pregnancy, has been known and described by authors since Gregoire and Dr. William Hunter first revealed its nature; and there is nothing in the opinions of the authors concerning this particular complication, which requires notice. Retroversion of the unimpregnated uterus is a subject of some interest at the present time, on account of the attention which, by several writers, has lately been directed to it. And here again we have a discrepancy of opinion, not only between the two authors, but we may also say, amongst obstetricians generally. Dr. Meigs speaks of it as one of the "most common of the deviations of the womb met with in practice;" while Dr. Ashwell censures M. Lisfranc as a man indulging largely in the marvellous, when he endeavours to show that it is a frequent displacement of the unimpregnated uterus, resulting from hypertrophy of the posterior wall. Dr. Ashwell speaks of it "as almost entirely a disease of early pregnancy;" and yet, with an inconsistency which, after his lavish censure of M. Lisfranc, is very apparent, he says:

---

* Vide Observations against the Use of Utero-Abdominal Supporters, p. 188.

> "I believe partial retroversion of the unimpregnated womb to be a more frequent occurrence than is generally supposed; its most common cause being hypertrophy, or more serious disease, of the posterior wall of the organ. If in such cases, where there is difficulty of micturition and defecation, a careful examination were made, the uterus would, I am persuaded, be often found more or less retroverted." (p. 626.)

We can quite understand Dr. Ashwell's habitual caution in receiving statements from M. Lisfranc, after the story which is abroad of his cases of excision of the cervix uteri; but we suspect that in his estimate of the frequency of retroversion, this very intelligent surgeon has not gone beyond the truth; and that our present knowledge on this subject will be found to corroborate it. For our own parts, we perfectly concur with M. Lisfranc in thinking that it is so frequent a displacement, that a man who has had large public opportunities of examining uterine disease, may in a few years number up, without the least exaggeration, his hundreds of cases. And we further concur with him, that it is a large bulky womb, with the back wall particularly prominent, which is the most common cause of this deviation. It is certainly important to remember, especially when considering the numerical frequency of the complaint, that retroversion, like the descent downwards, has different degrees; and we particularly insist on this, as Dr. Meigs has supplied a drawing in which an unimpregnated uterus is completely retroverted, its fundus being quite low down in the recto-vaginal pouch, and the cervix looking directly upwards in the axis of the brim of the pelvis. If Dr. Meigs means this to be the practical representation of an unimpregnated womb in the ordinary extent of retroversion, we do not hesitate to say, that it is the rarest of uterine deviations. Retroversion is only a *common* complaint, when it occurs in a far less marked degree; the fundus falling backward towards the upper part of the sacrum, and giving the womb as much an inclination backwards, as in its normal position it would be directed forwards. This observation, we know from our own experience, applies also to a womb retroverted in early pregnancy; and the cases related by Dr. Ashwell, which required the introduction of the entire hand into the rectum, to get bearing enough on the bulging fundus uteri to raise and restore it, are extreme cases but rarely encountered; whereas a less decided retroversion is more frequently met with amongst cases of abortion (to which it is related as a cause) in ordinary practice.

Dr. Meigs labours to prove that the cause of retroversion, "the maladive or pathological fault, is in the round ligaments; and not in the womb itself." He does not appear to have noticed the overweighing of the womb, which, from affecting the posterior wall principally, inclines the whole organ backwards, and, as a secondary effect, puts the round ligaments on the stretch. His treatment of a case is to raise the womb by a vaginal pessary so as to relax the round ligaments, which will then contract to their proper length and recover their tone. We, however, who accord with those who regard the womb as primarily in fault, (with but few exceptions), consider that the first indication to be fulfilled is to reduce its size, and then to restore tone to the weakened structure.

The opposite displacement, viz. anteversion, is regarded by both the authors as a very uncommon disorder. Dr. Meigs has only seen three or four cases of it in the course of his professional life. Dr. Ashwell relates a case furnished to him by Dr. Walshe, in which the uterus was found after death to be "flexed on itself, at an obtuse angle, at the union of its body and neck, in such a manner that the fundus concealed by the bladder was inclined forwards and downwards, while the neck was inclined backwards to the sacrum, the posterior surface of the body being antero-superior." "The body of the organ, as well as the neck, is hypertrophous; their substance is of a grayish hue, and hardened, firm, and resisting throughout, except at the union of those parts, where there is a band of the organ flattened from before to behind, extremely soft, flabby, and yielding, and corresponding exactly to the angle of flexion."*

This interesting case, which was reported by Dr. Walshe from a patient of M. Louis, of Paris, is one in which the flexion of the womb forwards is more decided even than its anteversion.

Flexions of the uterus.—The two displacements of the uterus which we have just noticed, are connected with those bends of this organ which have respectively been called Anteflexion and Retroflexion, and which have been reported by some recent writers to be of very frequent occurrence. We looked with some interest at the opinions and experience of the authors on this subject; and it is satisfactory to find that there is a general accordance in their views. They both regard these flexions as part only, and an unimportant part too, of some displacement of the uterus, whether prolapsus or retroversion; although Dr. Meigs does not deny but that a congenital cause, such as an unequal development of the cervix, "causing the anterior or posterior half to be too short," may occasion them. "Retroflexion," says Dr. Ashwell, "may be regarded as partial retroversion, the same in every respect save one—the alteration of position of the cervix, which, from preserving its proper locality, renders curvature or flexion of the body necessary to the production of the displacement."

> "Many women," says Dr. Meigs, "have the cervix uteri very slender; some of them are not bigger than the little finger. Now if a woman with such a slender, flexible, and weak cervix, should have some degree of descent of the womb, it might happen that the long slender cervix, being driven, or having descended far enough to rest, upon the posterior part of the pelvis, or even on the posterior wall of the vagina, should bend with its own weight and any superadded weight, so that the cervix would cease to be straight, but become curved or angular; and after being kept so for many months, would acquire such a form, as its permanent character." (p. 224.)

Our own observation as to these flexions is quite in keeping with the above-quoted opinions; and excepting only those curious cases in which the womb is congenitally twisted, or in which, as in Dr. Walshe's case, there has been a loose, thin, flaccid texture at the junction of the body and neck of the uterus, allowing the former to fall or be drawn forward, we think the flexion might well be merged in the graver displacement in which it partakes. Like Dr. Meigs, we "do not know that these curved or angular conditions of the cervix are, in themselves considered, to be esteemed as matters of any great consequence;" and we are well persuaded that those who have written in the periodical journals so much about retroflexion, might just as well have avoided complicating the real disease, viz. retroversion of the body of the uterus, by the introduction of the title. It may no doubt be very entertaining, and wear a specious appearance of originality, to elicit new diseases from those which are clear and well understood: and we quite accord with Dr. Ashwell, in believing that "many cases of slight and unimportant uterine displacements have been most erroneously set down as examples of the anterior and posterior flexion."

---

* Page 607.

The diagnosis and management of these versions and flexions of the unimpregnated uterus, have been said by Dr. Simpson, and several other obstetric practitioners, to be greatly facilitated by the use of the uterine sound, and by a particular contrivance invented by Dr. Simpson for maintaining the uterus *in situ*. The lavish encomiums which have been expended on the former instrument by these gentlemen, have, we believe, caused it to be pretty extensively employed in diagnosing these affections. By introducing the sound into the cavity of the uterus, the direction of the body of the organ is indicated; and when once within the womb, it can be made to restore the displaced organ, or to carry it from side to side, or to remake the displacement at will. All this may be done, it is confidently said, without any evil resulting from it. We have already said something on this subject, in our allusion to the sound as a means of diagnosis in uterine disease; but we cannot help expressing our cordial concurrence with Dr. Ashwell, when, in reference to its application to these particular complaints, he says—

> "I cannot avoid thinking, that this uterine sound not only detects, but makes many of these supposed displacements. All practical men know that the uterus varies naturally in its position, in its degrees of mobility and immobility, and in the influence exerted upon it, as to position, by a loaded or empty rectum or bladder; and it must be kept in view, that the curve of this steel bougie may not be the curve of the uterus; and if, therefore, it is to be introduced at all (and I wish it were far less frequently so), the normal position of the organ, thus spiked, must be made to follow the curve of the iron instrument, entering, and thus necessarily intruding upon, its cavity." (p. 635.)

And then, again, we find Dr. Meigs agreeing with Dr. Ashwell in deprecating the employment of such an uterine supporter as Dr. Simpson's; the peculiarity of which, as our readers are doubtless aware, consists in an ivory stem, which is passed *into the uterine cavity,* and is made, by its union with the other parts of the instrument, to be the immediate support to the replaced womb. "Some have proposed," says Dr. Meigs, "to pass up a small spatula of wood (it might be well denominated a peg) into the canal of the cervix, to straighten the bent cervix, by leaving it *in situ*. Such a process of spitting or skewering the womb appears to me ridiculous."

Will the womb bear this foreign body? Does it not occasion irritation, discharges, direct or contiguous inflammation?—are questions which at once suggest themselves to every practical inquirer; and an anxious desire to hear the worst as well as the best of it, must be present in the minds of all who are disposed to try it. Dr. Simpson's report of it is unexceptionably good. A gentleman quoted by Dr. Ashwell—Dr. Hensley, has known peritonitis induced through the patient not having been kept quiet in bed for some days after its introduction; while Dr. Ashwell's own experience of it in two cases, is thus described:

> "Two cases have recently come under my care, and I have heard of more, where the results arising from the use of this intrument have been very serious. Looking at it pathologically, I can scarcely imagine anything better devised for inducing disease. According to this practice, a piece of ivory, two inches and a half long, is to be introduced into the uterine cavity, and its bearing must of necessity be on some part of the lining membrane, a surface ill adapted to support the pressure for two or three months together of such an instrument. The consequences may be supposed. One of the patients from whom, after a long and distressing journey, I removed this ivory one-pronged fork, told me she had never been free from pain since its introduction. In addition, it had produced frequent and intense sexual excitement, preventing sleep for many nights together, and had kept up constant leucorrhœal discharge. In the other example, during the two months this instrument had been worn, the sufferer, in addition to the previous complaints, had never been free from sanguineous discharge, lumbar pain, and frequent desire to micturate. In both, the speculum showed that abrasion of the os and the ostium vaginæ had resulted from the use of the so-called uterine supporter." (p. 636.)

Our own experience of the practical value of this supporter is certainly unfavorable. It is sometimes difficult to adjust; and the irritation which it sets up we have found to be great and formidable. There is much difference, no doubt, in its effect on the uterus of different women, some tolerating it comparatively well; but at the best, we confidently believe, there is much risk run of immediate or remote disorder. Within three weeks of the time we are writing, we saw a young unmarried female, whose previous history and disease we were already acquainted with, and who some months before had had the ivory stem passed into the uterus by one of that docile flock of London obstetricians who bow their ear obsequiously to the tinkling of their northern leader's bell, and who applied to us in consequence of a return of the same symptoms as those from which she had previously suffered. The uterus was slightly prolapsed and partially retroverted; but, as usual in such cases, it was easily restored by pressing the bulging back wall upwards and forwards. For this the sound had been passed, and then the ivory-stemmed supporter. The immediate effect of the latter was to bring on severe pain and fever, which was followed by cellular inflammation and suppuration, the large cicatrix of the opened abscess being visible on the abdominal integument on the left side. When she recovered from this, however, the stem was again passed, and at last she was able to bear it; and the restoration of the womb gave her relief from her lumbar and bearing-down pains. She wore this stem for several weeks, and when it was removed she was thought to be cured. The symptoms, however, soon again returned, and were both aggravated, and in a great measure reproduced, by the habit of masturbation, which she had acquired since the application of this support. The os uteri of this young woman is open enough to admit the tip of the index-finger.

There is an asperity, and a tone of ironical contempt, about Dr. Ashwell's remarks on both the uterine sound and this supporter, which can hardly be justified, excepting on the supposition that he believes folly is mixed up with their invention, and rashness with their use. If not, they deserve a more respectful notice, and a more deliberate trial. We feel, too, that these incidental remarks suggest a question which his own sagacity has partially brought to bear to the present complaints, and which may well be made to include other methods of treatment in other diseases of the womb. Is there anything so serious in a retroversion or retroflexion of the unimpregnated uterus, as to justify a practitioner in running the risk of exciting peritonitis, cellular inflammation, and abscess,—injuring the structure of the womb,—and enkindling desires, or implanting habits, which destroy purity of feeling and physical health? Our own conviction is, that this supporter will be found to create more disease than it cures; and with the belief that the uterine sound will be extensively because easily employed, the same observation will, we think, be verified in it. The habitual use of powerful escharotics in simple inflammatory hypertrophy of the cervix, cicatrizing and contracting the os and cervix, and frequently too the vagina, and risking deeper-seated pelvic inflammation, is open to a similar inquiry, and the same animadversion. May we not add, too, the practice, now becoming common, of opening and dilating the os uteri, whenever it happens to feel rather small, and smearing the uterine cavity with the solid nitrate of silver, to excite the menstrual flux; and, at last, to cover the spirit of obstetric adventure with the mantle of Quixotism, the announcement that the Fallopian tubes are to be explored to remove sterility? Surely, in all these, there is much that is visionary; more that is meddling; and in all there is the tendency to create more disease than they cure. It appears to us that much of what has been proposed and adopted in the treatment of uterine disease within the last six years, has been empirical, and rashly experimental; and that the evil results have been in a great measure concealed. Those who see the greatest diversity of practice,—men like Dr. Locock, Dr. Ferguson, Dr. Lee, &c.,—ought, as a debt which they owe to their profession, to speak out upon these matters; and we do not think we anticipate too much in believing, that they would condemn much that we have ourselves censured.

Diseases of the ovaries. Acute and chronic ovaritis, dropsy of the ovary, atrophy of the ovary, fibrous tumours and malignant diseases of the ovary, are described by Dr. Ashwell; and a letter on these diseases is supplied by Dr. Meigs.

Dropsy of the ovary. So much interest has of late been excited about this disease, from a revival of the formidable operation for the extirpation of the cyst, that we are glad to have the opinions of two practical physicians like the authors on the subject. They both, however, agree in condemning it. Dr. Meigs acknowledges the fact, which Mr. Lee's tables seem to prove statistically, that tapping the cyst is far more frequently followed by death, either within a few hours or a few weeks, than might have been anticipated; and that therefore, although simple enough in one sense as an operation, it is dangerous in its issue. Dr. Meigs states his own impression to be, that of first tappings, of which he has been witness, nearly one half have been speedily followed by the death of the patient from peritonitis, developed soon after the performance of it; and all, with one exception, soon ended fatally. To this we may also add another fact elicited from Mr. Lee's tables; viz., that there are on the average three recoveries to one death after ovariotomy; and that the results of other capital operations in surgery yield a greater mortality. Dr. Meigs, however, thus summarily states his objection to it, and disposes of the argument derived from its comparison with other operations:

> "I am opposed to the operation for extirpating the diseased ovary. The ground of my objection lies in that principle of surgery, which I hold to be one of its highest principles, that there can be no duress, no binding obligation to perform it, and that the surgeon's conscience may be at peace, though he refuse and the patient perish by the progress of the malady.
>
> "I have read with great pleasure Professor Simpson's remarks in favour of ovariotomy, in a pamphlet, for a copy of which I am indebted to his politeness. I admit the considerable force of many of the arguments arrayed in Professor Simpson's publication, but I see no parity in the *duress* of the surgeon who decides upon an ovariotomy, and that of the surgeon who performs the operation for the stone, or an amputation, or the ligature of an important vessel, &c., which he dare not refuse to perform, whereas no such urgent, imminent, and definite necessity can ever be supposed of an ovariotomy operation.
>
> "In the one case, the surgeon resorts to the operation under compulsion of an imminent and pressing danger; in the other, he takes his instrument in his hand under the dictation of a cold calculation of chances as to a human life. Now who, I pray, has the right to calculate as to the value of a man's life, even for an hour, much more for a week, a month, a year, or a quarter of a century!
>
> "I am opposed then to the operation of ovariotomy, and I am opposed to it on the grounds of objection I consider valid against all surgery that is not unavoidable.
>
> "The ovariotomy operations are not unavoidable, since no one, I presume, would think of operating for a patient not remote from dissolution, and since, in any case not menacing a speedy termination of the life of the patient, the unavoidable necessity has not arrived." (pp. 313-14.)

Dr. Ashwell is also very strongly opposed to it, and considers that it will ever remain an eminently uncertain operation. Still he appears to admit that it may sometimes be justifiable, and he limits it in the following manner:

> "If the operation is to come into established practice, of which I have the strongest doubt, it must be confined to examples of the malady where tapping has already been so often performed as to preclude, from the experience of similar cases, any idea that it can ever be dispensed with; and where, we are confident, that great suffering must lead to early death. Perhaps this may be regarded as too limited a view of the value of extirpation; but it is, I think, the correct one. In such cases, if the diagnosis excludes the belief that there are serious adhesions, or malignant and solid growths complicating the tumour, and if the patient strongly desires it, the operation is defensible." (p. 663.)

We cannot say that the principle on which Dr. Meigs acts, in altogether excluding ovariotomy, is valid or decisive. It has, perhaps, a limited application to early cases, when the size of the tumour has not interfered mechanically with important organs; and any such operation exposes the subject prematurely to the serious risks attending it. But when the disease has been of long standing, and is steadily increasing, and our ordinary resources have proved but feeble palliatives,—when its bulk has begun to encroach on the thorax, and the swelling of the abdomen is the source of incessant distress, and is a hinderance to the performance of the duties and enjoyments of life,—when we feel the practical assurance that its further increase must destroy life, and the patient (being apprized of the risks) is anxious, often urgent for the aid, and there is no forbidding complication, either in the probable attachments of the tumour, or malignant disease elsewhere, or other organic disease,—surely, under such circumstances, it becomes an *unavoidable* operation, the conscience of the surgeon making it so; unless, indeed, this has been rendered insensible to the stigma of remissness and neglect, by the extreme adoption of an arbitrary principle. There is a reasonable expectation that a patient so circumstanced may not only be released from distressing suffering, but may literally be restored to perfect health. The statistics even give three recoveries to one death. And even allowing that this result is too favorable, it is impossible to have witnessed the issue of favorable cases, as we have had the good fortune to do, and not to feel that surgery has had a perfect triumph, in rescuing the subjects of this disease from impending death, and from sufferings and social privations almost worse than death.

It is beyond our purpose to enter in detail upon the diagnosis of the disease itself, and of adhesions around the tumour, as bearing on the feasibility of the operation; but we feel well persuaded that the eager attention which has been paid to these points will greatly diminish the uncertainty concerning them. The careful preparation of the patient for the operation, and the attendant circumstances of temperature, &c., during it, all contribute greatly to its success; and it is not, we think, anticipating too much to believe, that a watchful experience will render abdominal surgery generally far less formidable, and far more successful, than it has hitherto been.

There is a brief account of the diseases of the external organs, by the authors; and a short notice, in Dr. Ashwell's work, of a painful affection of the urethra, which he has termed chronic urethritis. We know something of the class of cases to which he refers, and fully concur with him as to their intractability, and the extreme distress which they occasion. Whether they are truly the result of chronic urethritis, may perhaps be questioned. We have examined the urethra with a speculum, in such cases, without detecting any appearance which could be denominated chronic inflammation; there is no purulent discharge; and local applications have been of little avail. We quite accord with Dr. Ashwell in the expediency of a mild mercurial course in this affection. We have known the disorder, in one instance, disappear suddenly, after having resisted a variety of treatment for a length of time, apparently from the action of a strong purgative. Dr. Meigs's letters on the diseases of the external organs, contain many interesting and rare cases, and many instructive observations.

In bringing our notice of these works to a close, we have only to remark, that our criticism of them has been in a measure proportioned to the high reputation of their authors. We think that Dr. Ashwell's work will bear a careful revision, and some condensation. The cases require to be much curtailed, and their points brought out by the author himself. It would be all the better if some of them were incorporated with the text, in connexion with the subject which they immediately illustrate, and many may be sacrificed altogether. There is a good deal of the haste of a busy man in the work; and here and there an observation occurs in one part, which is out of keeping with another subsequently made. The author, too, is sometimes circumlocutory, almost obscure, in the statement of his opinions; and, when this is the case, it requires the reader to reperuse the paragraph again and again, before it is quite intelligible. If the author, in the next edition, would bestow some time in revising the present volume, we are quite sure that he would soon weed it of these faults. We take our leave of Dr. Meigs, with a high opinion of his talents and originality; and we trust that, if we meet him again as an author, we shall have to notice his solid, substantial merits only, without such an obtruding, meretricious dress, as he has chosen in his present Letters.

Art. IX.

On Femoral Rupture; its Anatomy, Pathology, and Surgery. With a new Mode of Operating, applicable to Cases of Strangulated Hernia generally. With Plates. By John Gay, F.R.C.S., Surgeon to the Royal Free Hospital, &c.—*London*, 1848. 4to, pp. 97.

We opened this new quarto with considerable interest, being curious to know what could possibly be written *new*, on a subject so exhausted as that of hernia—a subject so fully considered in all our surgical dictionaries, manuals, and lectures, and in the numerous special works which its importance has called forth. The interest was not diminished by the statement in the preface, that the author proposed to describe the anatomy of the parts concerned in femoral rupture, both before and after its formation, "from their careful examination in above seventy subjects;" and that from this study of anatomical relations he had been led "to relieve the mass when strangulated by a *new method* of operating, which I believe to be more *simple and safer than any which has hitherto been practised or described;*" the principle of which is capable of being applied to the relief, not only of femoral, but to all outward forms of strangulated hernia. This appeared very promising; but on carefully going over the first half of the book, we found to our great disappointment, that the dissection of the seventy subjects had led to nothing more than such a description of the surgical anatomy of hernia, as any student might compile from the works of Cooper, Lawrence, and Hey, with an occasional reference to Cloquet; the descriptions not being remarkable for clearness, nor the language for correctness—although both are free from any errors sufficiently glaring to call for especial notice. Even where one would expect the seventy dissections to *necessarily* yield some results, as in the account of the normal and abnormal courses and branches of the obturator and epigastric arteries, and of the different measurements from the centre of the sac to the orifice, the rings, arteries, and other neighbouring parts, Mr. Gay says not one word from personal observation. His measurements are all taken from Sir Astley Cooper, and his anatomy of the arteries from Quain, Harrison, and Burns: but we learn nothing of the statistics of the seventy cases here, or indeed anywhere but in the preface. We are not told where the seventy subjects were so opportunely obtained, where the dissections were made, where the dissected parts are preserved, nor, as we said before, can we find a single result of original research in the whole of the anatomical portion of the book.

A few observations follow, equally destitute of novelty, upon strangulation of hernia, and on the ordinary and unusual seats of stricture; and then we come to the surgical part of the work, which is commenced, singularly enough, by a short account of some of the different measures proposed for the radical cure of hernia. This section is concluded by the following extraordinary sentence.

"The device of these various plans, with the view of effecting a radical cure in cases of rupture, resulted from mistaken ideas of the nature and connexion of the hernial sac, as well as from the small amount of information which anatomical research had elicited in reference to the subjects of hernial displacements generally. A strangulated hernia, at the period when they were so commonly practised, was known only in as far as its symptoms and tendencies were concerned, but its treatment by liberation of the strictured parts was hardly thought of; and that person was accounted sadly blind to his own safety, who, with a rupture, neglected by the means which these proceedings afforded to secure himself from the peril which, in case of its strangulation, he would assuredly incur." (p. 55.)

All this, it must be remembered, is after quotations from Gerdy, Bransby Cooper, Bonnet, Mayo, Chelius, &c., all of course "hardly ever thinking" of division of the stricture, and entertaining most "mistaken ideas" of the nature and connexion of the hernial sac. We do not believe that Mr. Gay *means* this, but he *writes and prints* it.

A number of quotations follow, to show the opinions of different authors upon the seat of stricture, and the proper direction of the knife in its division. We hear nothing of the seat of stricture in Mr. Gay's famous seventy dissections, nor does he seem to be at all aware of the fact, that whether the ligaments of Hey, Gimbernat, or Poupart, be the seat of stricture, a division of the deep crescentic portion of the fascia lata, beneath which the saphenous vein passes, liberates it. Other authors, however, are equally silent on this point, although Mr. Colles long since demonstrated the fact, and any one can convince himself of its accuracy by walking into a dissecting-room, even although he may not be able to meet with seventy subjects. Again, with regard to the direction of the division of the stricture, we find all the advantages and disadvantages of division inwards, outwards, upwards, upwards and outwards, upwards and inwards, &c., and the recommendation of Mr. Gay, "first, to divide the seat of stricture *towards the pubes;* and then, from the same wound, *forwards*, should the rupture, after the first incision of the stricture, resist the most gentle efforts to return it." (p. 62.)

The plain, common-sense view of the matter, however, has quite escaped Mr. Gay, and not him alone. What are the parts to be avoided when dividing the stricture of a femoral hernia? Simply the femoral vein, the cord and its components, and the obturator and epigastric arteries. The vein can be in no danger from any man in his senses; but no one can possibly say, in any given case, where he will find the arteries. It is all very well to know, that in two cases of three, they have one origin; in one case of three and a half, another: and in a small known proportion, a common origin; but this says nothing as to their origin or course in the patient actually under the knife. The spermatic artery and vas deferens, too, are always endangered to a certain degree; and yet, with all this, we are assured that, in one of our Metropolitan Hospitals, distinguished for the large annual number of operations of this class performed at it, only one case has occurred for a very long period, in which death resulted from hemorrhage in the division of the stricture of femoral hernia. We would suggest that even this risk may be avoided, if surgeons will avail themselves of the well-known fact, that tense fibrous structures may be readily divided by a knife so *blunt*, that it will push arteries before it without wounding them. We have repeatedly shown this in the dissecting-room; and have been in the habit, when practising herniotomy on the living subject, of blunting the knife just before dividing the stricture, by drawing the edge over the back of the forceps. If this precaution be adopted, it is of little consequence in what precise direction the knife is carried, except towards the vein; as the arteries are safe wherever they may be, and the vas deferens also.

This brings us to the subject of division of the stricture without opening the sac,—a method first proposed and practised by Petit and Le Dran, afterwards by Munro *secundus,* and to some extent by Sir Astley Cooper, more recently advocated and extensively adopted by Mr. Key, Mr. Luke, and many other surgeons. Mr. Key's memoir is so well known, that it is hardly necessary to remind any reader of his arguments. The matter stands simply thus. Extensive statistical research has shown that the mortality after the operation for strangulated hernia by opening the sac, amounts in different hands from a third to more than a half of the patients operated on; and that a fatal result is extremely rare after a careful and successful reduction by taxis. These considerations have led many to persevere much longer than is prudent in the taxis, and to defer the operation until the period when it might have been practised with a probability of success had passed. They led Mr. Key to propose division of the stricture external to the sac; and he revived Petit's operation as an aid to the taxis, and as a means of avoiding the dangers consequent both upon opening the hernial sac, and upon a protracted and ineffectual employment of the taxis. Mr. Luke, however, appears to have carried out this principle into practice more extensively than any of our London surgeons; and his experience will be found in a paper, read before the Medico-Chirurgical Society in February last, and published in the 'Lancet' of March in an abridged form. He operated in 82 cases. In 57 the sac was not opened; in 25 it was necessary to open it, in order to obtain complete reduction of the hernia. Of the 57, 7 died; of the 25, 8 died. Omitting some exceptional and selected cases, Mr. Luke prefers stating the results of his whole practice since 1841, thus;—52 patients were operated on; sac opened in 21, of whom 3 died; not opened in 31, of whom 2 died. Of these cases, 29 were femoral, 20 inguinal, and 3 umbilical. Mr. Gay has collected other cases and drawn up the following table, still maintaining a remarkable reserve as to his own observation.

Whence obtained.	Total number of Cases.	Sac opened.	Deaths.	Recoveries.	Sac not opened.	Deaths.	Recoveries.
Miscellaneous	57	48	25	23	9	2	7
Mr. Howship's practice.	8	6	3	3	2	2	...
Mr. Poland's Report of Cases in Guy's Hospital	17	12	7	5	5	2	3
Mr. Luke's	82	25	8	17	57	7	50
St. George's Hospital, 1842-3	34	34	9	25	...	...	...
Total	198	125	52	73	73	13	60

(p. 74.)

He has also compiled a number of observations leading to some commonplace remarks upon the direct and indirect causes of death in strangulated hernia, and the indications of treatment. The last desideratum is "a more simple operation," and then comes a section on the Operation, but this is unfortunately as destitute of novelty as the rest of the work. The "new operation" is nothing more than division of the stricture external to the sac; the first incision, however, being made not over the hernial tumour, but internal to it. Mr. Luke had said that he considered it "very desirable to avoid, as much as possible, interfering with the tumour in conducting the operation," Mr. Gay says the principle of his operation is "non-interference with the hernial tumour." (p. 90.) Mr. Luke makes a perpendicular incision, the centre of which is between the upper part of the tumour and the abdominal surface. Poupart's ligament is then reached by carrying the finger from above downwards, and the stricture is divided upon a director introduced into the femoral ring. Mr. Gay makes his first incision between the inner side of the tumour and the spine of the pubes, introduces a sheathed bistoury "between the sac and pubic margin of the ring, the edge of the knife is then to be turned towards the pubes, and by projecting the blade, the seat of stricture in that direction may be effectually divided." (p. 91.) He says this operation "leaves the hernial tumour and its diseased coverings untouched," (p. 92;) but although his *first* incision does not interfere with the tumour, yet the deeper ones must do so, or the fibres which are constricting the neck would not be divided. The real principle of the operation is division of the stricture external to the sac, the identical principle of the operation of Petit, Key, Luke, &c. There is nothing *new* in it. The slight modification made in the situation of the first incision is rather objectionable than otherwise, as it does not conduct so directly as others to the seat of stricture; and we entertain strong objections to the use of concealed or sheathed knives in any operation for hernia.

Mr. Gay concludes his book upon his new operation for femoral hernia by short notices of some cases. Four only of these were cases of femoral hernia operated on by himself, and one by Mr. Catherwood of Hoxton. All were successful. The sixth case is a case of *inguinal* hernia operated on by Mr. Coulson. Cases 7 and 8 are instances of *umbilical* hernia in which Mr. Childs and Mr. Gay were the operators, also successful. The eighth is a fatal case of *ventral* hernia. Of course the last four cases can only be considered as common cases of the operation external to the sac; so that Mr. Gay's personal experience of his "new operation" consists of four cases, and upon this foundation he raises his quarto. This is really too bad. The principle of his operation is neither new nor unknown, and upon the results of his experience in four cases, he introduces a slight modification in the first steps of the method of those who have published the results of experience exceeding his more than twentyfold, and deludes unfortunate surgeons who wish to keep pace with the times into the outlay of half a guinea, entailing upon them, at the same time, all the vexation any reasonable man must feel when he knows that he has wasted his time and money upon a useless book. From our general condemnation we should, however, make an exception in favour of the Plates; which are beautiful as works of art, and are drawn with Mr. Bagg's usual accuracy and knowledge of his subject. The general "getting up" of the book, also, is very creditable to the parties concerned. We could have wished that the matter had been more worthy of its dress.

ART. X.

A Dispensatory, or Commentary on the Pharmacopœias of Great Britain. By R. CHRISTISON, M.D., &c., President of the Royal College of Physicians, and Professor of Materia Medica in the University, of Edinburgh. Second Edition. Edinburgh, 1848, 8vo. pp. 1003.

THE very excellent Dispensatory of Dr. Duncan long formed the text-book of students, and the guide for junior practitioners, in all matters relating to drugs and their uses. It passed through twelve editions; and its plan seems to have been well approved of, as it was adopted by Dr. Thompson for the London Dispensatory. This plan was, however, far from a convenient one, either for learner or teacher, as it was that of the Pharmacopœias, in which, as is well known, the articles of Materia Medica are first enumerated in an alphabetical list, and the directions for the preparations are then given in a series of chapters beginning with acids and alkalies, and ending with unguents and wines. This arrangement may possibly offer some advantages in the laboratory; but any one who wishes to know the various modes in which any particular drug may be made up or prescribed, will have to look through all these chapters both for negative and positive information, or refer to the index, where the same information is also alphabetically, but more compactly arranged. Dr. Duncan observes that "every scientific classification is liable to particular objections;" this is no doubt true, but the worst is, we conceive, better for the purposes of learning, than so empirical an arrangement as the alphabetical.

Dr. Christison having felt the inconveniences of the Pharmacopœia plan, has yet, without abandoning the alphabetical arrangement, made a great improvement in the treatment of his subject, by abolishing the numerous heads under which the preparations of one substance were dispersed. He has thus brought the whole into one article, which is treated of under the most commonly received name of the substance; or, in other words, he has incorporated the pharmaceutical preparations with the Materia Medica part of the Dispensatory. On this the author remarks:—

> "Finding it necessary to write the whole of the following treatise, and believing it practicable to improve Dr. Duncan's plan, so as to condense it in many places, without impairing its comprehensiveness or practical spirit and tendency, I should have in some measure misled the reader, had I retained either his name or the designation of his book on my title-page. But I cannot make this departure from my original purpose, without acknowledging—what every writer in the same department of medical science must feel—the deepest obligations, both as regards literary research and practical inquiry, to the author of the New Edinburgh Dispensatory."

The Edinburgh Dispensatory contained, as the London Dispensatory still does, an introductory treatise on pharmaceutic operations, with several very useful tables. This has been omitted by Dr. Christison, who observes, that in order to be useful, such a treatise must be far more comprehensive, and that it ought indeed to constitute a work of itself for practitioners and druggists. There is no doubt that the latter class require a systematic treatise, in which the principles of pharmacy should be carefully discussed, and its details minutely described. But we cannot agree with the author in considering that it was entirely useless and never read, as he states—"Nor will it be easy to find a practitioner or student who can say that he has perused the treatise on General Pharmacy contained in any Dispensatory, not even excepting the masterly summary given by Dr. Duncan." In reply, we can say that we have not only read, but studied it when a student, and found it useful when in practice. Dr. Kane's Elements of Pharmacy, and works on Practical Chemistry, may render such an introduction now less necessary; but the multiplication of books which a medical man is obliged to have, will, we fear, induce many to neglect the subject altogether by omitting to purchase any separate treatise on Pharmacy. Dr. Christison has not, however, entirely passed over the subject, for the present work contains useful commentaries on the general heads under which the preparations of the Materia Medica have been arranged in the three British Pharmacopœias. We would particularly call attention to the observations respecting the superior method of obtaining spirituous solutions or tinctures by "the method of displacement or percolation, which consists substantially in nothing else than the gradual transmission of the spirit through the solid materials in a state of moderately fine division."

This introduction concludes with a section on weights and measures, in which, besides those employed by the British Colleges, a few of other European countries are noticed, together with those employed by native practitioners of Lower India. But as this forms but a small portion of our Eastern Empire, those of other parts of India should have been added; and some are now easily accessible, by having been reprinted in the Bengal Dispensatory.

We must regret that the nature of Dr. Christison's subject prevents us, unless we enter into numerous details, from giving such an account of the full and varied contents of his work as may be desirable. The information is given under the heads of Chemical or Natural History descriptions, with the Adulterations, Actions, Uses, and Doses. The first edition was published in 1842, and the second in 1848; the former extends to 955 pages, and the latter to 957 pages, with a supplement of 25 pages, describing the most important new remedies which have come into general use since 1841. But the reader must not conclude from this, that the two editions are nearly alike, for we observe that the second has undergone careful revision, and, by the adoption of a more condensed style, and the omission of less important passages, space has been obtained for numerous additions of recent information to almost all the important articles. Thus, to notice only the acids: under the head of the Acetic, we have Melsen's process noticed for obtaining it by the alternate formation and decomposition of the binacetate of potash; and under vinegar, its formation from weak spirit, "which is oxidated by exposure over an extensive surface to the action of atmospheric air, under the disposing influence of a little old vinegar, carrot-juice, or other organic substances." (Wagemann and Schuzenbach.) With respect to the tests for ascertaining the presence of sulphuric acid in vinegar with a solution of nitrate of barytes, the author states that "Böttger says so small a quantity as a thousandth of free sulphuric acid may be easily detected by a deposit of sulphate of lime forming, when the vinegar is boiled with a little chloride of calcium, and allowed to cool; the natural sulphates of lime not being then decomposed."

Under Muriatic Acid, is introduced Dr. Gregory's convenient formula for obtaining a pure acid from four ounces of patent salt and five fluid ounces of sulphuric acid of density 1600, in a common flask, connected by a bent tube with a corked bottle containing two fluid ounces of distilled water. So is noticed the detection of sulphurous acid in the muriatic, by crystallized protochloride of tin, as practised by Girardin. With reference to the presence of arsenic in sulphuric acid, which may be detected by the yellow precipitate, or diluting the acid and passing through it a stream of sulphuretted hydrogen, the author observes:

> "It is stated to exist in the form of arsenic acid, and is left entirely in the retort when the sulphuric is carefully distilled. Sulphuretted hydrogen is said not to remove it entirely from sulphuric acid, but this may be effectually done by an alkaline sulphuret." (Dupasquier.)

A copious article is given on hydrocyanic acid, with Thaulow's observations on the proportion of sulphuric acid to the cyanide of potassium in the ferrocyanide when preparing the acid by this method, now the most generally practised. But at p. 21, in the directions of the London Pharmacopœia for making dilute hydrocyanic acid, by decomposing cyanide of silver with dilute hydrochloric acid, we are sorry to observe that the errata in the original copies of the Pharmacopœia, of 9½ grains of the one to 9 minims of the other, in a fluid ounce of distilled water, are reprinted. These numbers were directed, in a fly-leaf inserted in copies which were subsequently issued, to be corrected to grs. xlviiss. of the cyanide to grs. xxxixss. of the diluted acid in the same quantity of water; and these numbers have been introduced into works which have been subsequently published. Dr. Christison observes, "the College, through oversight, directs a fluid ounce of water to be used for every 9½ grains of cyanide. The quantity ought to be 95 grains." In a similar case of the Edinburgh Pharmacopœia, where 32 ounces of olive oil and nitric acid of density 1500 were ordered, Dr. Christison has corrected (in the formula for hydrargyri nitratis) the one to 38½ fluid ounces, and the acid to density 1380 to 1390. With respect to the antidotes against hydrocyanic acid, Dr. Christison observes:

> "For a poison so speedy in its operation, scarcely any antidote can be practically available. The best, however, is the administration of carbonate of potash, followed by the mixed sulphates of iron, to convert the poison into Prussian blue (Messrs. Smith). The proportions are 144 grains of carbonate of potash in two ounces of water, and a similar solution of a drachm of sulphate of protoxide of iron, with two drachms of the same salt converted into sulphate of peroxide by sulphuric and nitric acid, as directed by the 'Edinburgh Pharmacopœia' for preparing *ferrugo*. Fifty-two minims of each solution will convert into Prussian blue all the acid contained in 100 minims of the Edinburgh acid. The inhalation of ammonia or diluted chlorine, combined with cold affusion of the head and chest, has seemed the only other efficacious treatment." (p. 27.)

The present edition of the Edinburgh Pharmacopœia having been reviewed on its first appearance, in the Thirteenth Volume of the 'British and Foreign Medical Review,' and the peculiar preparations which were then for the first time introduced having been noticed, we need not dwell upon these in the Dispensatory of the distinguished author who had so large a share in the preparation of that Pharmacopœia. On that occasion the introduction of the formula for making the *ferri carbonas saccharatum* was noticed. With respect to this preparation, Dr. Christison observes:

"The action of the sugar in the saccharine carbonate of iron is somewhat obscure. Klauer supposes that a regular compound is formed between the sugar and the protoxide of iron. But the protoxide is combined with carbonic acid. The whole iron, however, is never preserved in the state of carbonate of the protoxide. Klauer found his compound to contain twenty per cent. of sesquioxide and eighty of protoxide; and I have never observed the carbonic acid to exceed two thirds of what ought to be present, were the whole iron in a state of carbonate. It has been alleged that the formation of sesquioxide may be prevented altogether by dissolving the carbonate of soda and sulphate of iron in water sweetened with an ounce of sugar for every pound, washing the precipitate with the same solution, adding honey, and evaporating with a gentle heat to the proper consistence for pills." (Vallet, p. 426.)

"The saccharine carbonate of iron is a powerful and excellent chalybeate;—fifteen grains have occasioned sickness, and ten grains, twice a day, have, in my observations, produced headache and a sense of fulness in the head. This new preparation has proved of service in neuralgia and other diseases in which the sesquioxide has been currently used, and I have found it an effectual as well as convenient chalybeate tonic. Doses, gr. v. ad gr. xxx; best given in the form of pill." (p. 426.)

As sugar was found by M. Frederking, of Riga, to have a similar protecting effect in the peroxidation of protiodide of iron, the Edinburgh College, in their revised edition, took advantage of the discovery in their directions for making the *syrupus iodidi ferri*, though, in the directions for making the iodide of iron, it continued to be mentioned as liquor iodidi ferri. With regard to its value and uses, the author says:

"It has been employed as a tonic deobstruent in scrofula, chlorosis, pseudosyphilis, chronic cutaneous diseases, amenorrhœa, and chronic rheumatism; and from the evidence furnished by Dr. Thomson and others, it seems to be an important remedy in several of these diseases, more especially scrofula, chlorosis, and chronic rheumatism. But doubts may be reasonably entertained whether the iodine has much to do with its therapeutic action; and the opinions of late observers incline to the conclusion that its effects are owing to its composition as a salt of iron, rather than to its being a compound of iodine." (p. 431.)

In reference to the utility of the hydrated sesquioxide of iron, or ferrugo, as an antidote against the effects of arsenious acid, Dr. Christison states:

"The rust of iron, as prepared by the Edinburgh formula, was proposed by Messrs. Bunsen and Berthold as an antidote for poisoning with arsenic; and repeated experiments, physiological as well as chemical, have confirmed their statements. The results of numerous trials, both with animals, and in cases of poisoning in the human subject, are such as to render it indispensable on the part of every druggist to have the hydrated sesquioxide of iron in readiness. There can be no question, notwithstanding some statements to the contrary, that when well prepared, either in the form of magma, or dried at a moderate heat, and afterwards pulverized with water, it immediately removes arsenious acid so thoroughly from an aqueous solution, that even sulphuretted hydrogen will not indicate the presence of the poison. The antidote should be given freely, to the extent of at least twelve times the arsenic swallowed." (p. 441.)

In the Supplement an account has been given of the still newer preparations of iron, ferri ammonio-citras, ferri ammonio-tartras, ferri citras, and ferri lactas.

Though we have hitherto noticed only the chemical articles of this

Dispensatory, many of those in the vegetable part offer much information, and valuable matter for extract. The article on aconite is an instance of how much is yet to be learnt respecting drugs which are ever within the reach of many, and yet the points which determine their value as a medicine remain uninvestigated. No one can hesitate to admit that some aconites are virulent poisons, and that they might be useful as medicines; yet some practitioners pronounce the extract of aconite to be a comparatively inert substance. The first point, in this case, is to ascertain the species of plant which we should employ. The London College selects *aconitum paniculatum* for the purpose, respecting which Dr. Christison says:

> "I have ascertained that the leaves of this species cultivated at Edinburgh are inert, and Dr. Alexander Fleming has found that even the root is similarly circumstanced. Contrary to the opinion expressed in the former edition of this work, it has been rendered highly probable, by the researches of Dr. Fleming, that the very peculiar acrimony of the monkshoods is a measure of their narcotic properties, and consequently of their medicinal virtues."

Among the species distinguished by their acridity, the *aconitum napellus* is mentioned; and that is the species which was formerly officinal in the London, as it now is in the Edinburgh Pharmacopœia. But determining the species is not the only thing that is required, for we may collect the leaves when they are comparatively inert. This is a point to which attention has no doubt been paid, but not to the extent that is necessary; for the secretions of the leaves undergo changes which we cannot judge of by their external appearance, and are connected in the napellus, as probably in many other plants, with the ripening of their seeds.

> "The root, one of the officinal parts of the plant, consists of numerous slender radicles proceeding from a long tap-shaped root-stock, like the navet or French turnip in form, and brownish-black externally. It is formed during the summer and autumn, as a tuber from the side of the old root-stock, which then decays; and the new one is in perfection for medicinal use from the close of autumn till the commencement of spring, when it begins to push up the new plant for the approaching summer. Neither the root nor the leaf of *A. napellus* has any odour; but when chewed, they slowly occasion a singular sense of tingling and numbness to the lips, cheeks, and tongue. This property is possessed by the leaves from their first appearance till the leaves begin to form. But, as Zeiger first remarked, and as I have often since observed, it afterwards quickly disappears; and when the seeds are ripe, it is entirely lost, though the leaves become even more vigorous than before. The ripe seeds themselves are intensely acrid. The leaves of *A. paniculatum* are bland throughout every period of their growth, and so are its seeds, and its root." (p. 55.)

With respect to the best mode of exhibition, the author observes that the watery extract is a precarious and irregular preparation which ought to be abandoned, and only the alcoholic extract of the leaves employed.

> "In preparing the alcoholic extract of the Edingburgh Pharmacopœia, a vapour-bath heat should alone be used; and the concentration must be stopped as soon as the proper consistence is attained, and the odour begins to change, otherwise decomposition speedily commences, and a comparatively inert extract is the result." (p. 55.)

The author confirms, in a great measure, the observations of Dr. Fleming on aconite, as reviewed in the Twentieth Volume of the 'British and Foreign Medical Review.' If our space allowed, we might refer to many important articles, as opium, cinchona, and colchicum, &c., all of which the author has elaborated with great care. Dispensatories being alphabetical, the account of many nearly allied substances, and preparations of the same substance, are often widely separated from each other. This Dr. Christison has in a great measure obviated; and a completion of his plan would bring together the articles on the poppy, opium, and morphia; also those on cinchona and quinine, as it would certainly be preferable to treat fully of quinine in the same place with cinchona and the other ingredients of the cinchona barks. One defect we should gladly see remedied; and this is, that the directions for the preparations are given at the beginning of an article, the observations on them in the middle, and the doses of each at the conclusion of each article; requiring, therefore, the reader to refer to three different places for all the information respecting a single preparation.

We have left ourselves little space to dwell long upon the supplementary articles to this edition of the Dispensatory, in which there are articles on gallic acid, on the new preparations of arsenic, cod-liver oil, pyroxylic spirit, &c. Of cannabis, Dr. Christison says:

"Indian hemp has been used as an antispasmodic in hydrophobia, tetanus, malignant cholera, and infantile convulsions, with marked relief in repeated instances. Some cases of tetanus appear to have been cured in the East Indies by it; favorable cases have been reported also in this country, and my colleague, Professor Miller, had a case under his care which proved successful without any other important remedy. It has also been employed with success as an anodyne in chronic rheumatism, toothache, and other varieties of neuralgia. I have used it a good deal, and with decided success, in diseases at large, to obtain sleep. On the whole, it is a remedy which deserves a more extensive inquiry than any hitherto instituted." (p. 969.)

Chloroform having been so extensively employed in Edinburgh, it will no doubt be interesting to quote some of the author's observations on the subject.

"The special uses of chloroform are numerous: it has been substituted for the inhalation of sulphuric ether, to induce insensibility during surgical operations. For this purpose it has this advantage over ether, of being much more rapid in its action, and more certain of acting in a much less dose, of requiring no nicer apparatus than a handkerchief, and consequently being applicable in some circumstances, such as in operations near the mouth, and in almost all operations on children, in which ether can scarcely be made available at all. Since its first recommendation by Dr. Simpson, it has been extensively used in this city in the way of inhalation to subdue the sufferings of child-bearing. Its effect is perfect, and may be maintained uninterruptedly for many hours, without influencing the frequency or force of the uterine contractions, and without any eventual harm. It is only when the deepest coma, with suffocative stertor is brought on, that the uterine contractions are apt to be arrested for a time; a property which may be taken advantage of to facilitate the operation of turning. I have used it with great advantage in the same manner to arrest hiccup, even during fatal peritonitis, and to put an end to the paroxysms of tic douloureux. Others have found it efficacious in asthma, hysteria, chronic vomiting, and delirium tremens. It has also been found of service occasionally as a hypnotic in the noisy forms of chronic insanity, and as a temporary measure it has proved most serviceable in the removal of obstreperous lunatics from one place to another.

"The dose for inhalation is a fluid drachm at a time, which must be renewed in two minutes if the desired effect be not induced. There seems no limit to the safe repetition of it. Dr. Simpson has used eight fluid ounces in thirteen hours in a case of labour. The patient experiences least annoyance, and is least apt to show a precursory stage of excitement, when a full dose is used at once. Innumerable chloroform inhalers have been contrived in London and Paris, but nothing is so convenient as a small muslin or silk handkerchief, loosely twisted into the form of a bird's-nest; no other apparatus is used in Edinburgh; and no inhaler can be more efficacious. When the effect produced is too intense, the best remedies are the horizontal position, cold air fanned across the face, and a stream of cold water poured on the brow and head only. Ammonia may be inhaled also; but internal stimulants should not be given till the patient revives in some measure, otherwise the respiration may be further embarrassed. In urgent circumstances, artificial respiration must be promptly added to these means. The dose internally is probably from five to twenty minims. It may be given dissolved in water with the aid of a little spirit." (p. 973.)

In conclusion, we strongly recommend Dr. Christison's Dispensatory, as a worthy successor to that of Dr. Duncan, and as exhibiting a comprehensive view of the several scientific and practical subjects embraced within the limits of such a work.

Art. XI.

A Course of Lectures on Dental Physiology and Surgery, delivered at the Middlesex Hospital School of Medicine. By John Tomes, Surgeon-Dentist to the Middlesex Hospital. With 135 Wood-engravings.—*London,* 1848. 8vo, pp. 392.

We have great pleasure in introducing to our readers the work of Mr. Tomes; since, if we cannot bestow on it unqualified praise, we can conscientiously recommend it as embodying the most elaborate exposition yet given of the late discoveries in minute dental anatomy and physiology, many of them the product of the author's own researches; combined with much excellent practical information, the result of a thorough knowledge of all that has been previously published on the subject by others, and of extensive and correct observation on the part of the author himself. The position which Mr. Tomes holds as the Surgeon-Dentist to a large hospital, has afforded him opportunity of obtaining extensive practical experience; and of these opportunities he has generally availed himself with equal zeal and good sense.

It is not many years since there was but one hospital in London, to which a practitioner in this branch of surgery was attached. Nor was there, even in the time of Mr. Fox—who lectured on Dental Surgery for many years at Guy's Hospital, and who may be considered as the first who attempted the combination now happily not uncommon of the Surgeon and the Dentist—any recognition of that gentleman as holding such an appointment at that institution. We believe that his successor in the chair of dental surgery was the first who received any public appointment as an hospital Dental Surgeon; and the example then set by Guy's has since been followed by almost every other hospital in the metropolis.

When it is considered how extensive are the opportunities thus afforded, not only for acquiring a knowledge of the diseases of the teeth themselves, but for investigating the connexion which so often exists between the abnormal or diseased condition of these organs, and a morbid state of others standing in a more or less intimate relation to them, it is to be lamented that so little has been done to clear up these ambiguous and obscure relations, to classify the complex and apparently anomalous cases which are so common and yet so little understood, and to force upon the attention of the professional officers themselves, as well as of the students, and indeed of the profession at large, the importance and frequency of these secondary and relative diseases. It is surely to the well-educated and practical surgeon-dentist to a public hospital, that the profession has a right to look for the elucidation of these common and too much neglected cases; and it is scarcely creditable to these gentlemen as a body, that they ordinarily satisfy themselves with the mere routine duties of their appointment, without an effort to use the only legitimate means of elevating their humiliated and degraded profession, to a rank and position which it can never otherwise be entitled to attain.

The work before us, although exhibiting a very creditable tendency in the right direction, has perhaps scarcely fulfilled, in this respect, all that might have been anticipated. The statistics of dental diseases, the symptoms and treatment of the morbid conditions of the teeth themselves, and of the parts immediately in connexion with them, are sometimes elaborately, and always satisfactorily treated; but the remote nervous affections in various parts of the body, the constitutional derangements arising from these local causes, the discrimination, often difficult, but very often possible, between those affections which are, and other very similar ones which cannot be, thus traceable,—these and many other matters immediately or remotely connected with the subject, have not, as it appears to us, received all the attention, even from Mr. Tomes, which his opportunities have demanded from him. Whether we have ground to anticipate any considerable improvement in the general state of the profession of dental surgery, from the leaven which in such small proportions is diffused over the crude, and heavy, and unwholesome lump, is a matter of scarcely hopeful speculation. If such a happy change should ever take place, it can only be by the renunciation of the captivating profits arising from the mechanical trade of the tooth-maker; and it is perhaps doubtful whether it might not be better that all the operations on the mouth itself, even down to the filling of the teeth and the removal of tartar, should not be the office of the surgeon; and the mechanical adaptation of artificial appliances for mastication, be exclusively the work of the mechanical dentist. Of one thing we are sure, that so long as the surgeon-dentist condescends to combine with the professional, this mechanical and artistic business, so long will he fail either to raise himself above the head of advertising quacks, or to elevate them, or any of them, to the status of professional character.

This is the incubus which hangs upon him, and presses him down; and no wonder that it should be so, when the public fails to recognise the distinction. The only test which at present exists, by which the surgeon can be distinguished from the dentist, is the Membership of the College of Surgeons; and that this is not infallible, is proved by two contrary facts, the one that Mr. Tomes is not a Member of the College,—the other that sundry advertising quacks are so. But it is time to afford our readers an opportunity to judge for themselves of the merits of the work before us.

Mr. Tomes has in his first two lectures given the usual descriptions of the teeth in general, and of the particular classes into which they are divided. In these subjects he does not materially deviate from the usual statements of other works on the descriptive anatomy of these organs. There is, however, not only in this portion of the work but elsewhere, a want of a correct appreciation of the relation of one structure to another, and especially of the distinction between analogy and affinity, between typical structure and accidental form, which appears to us to betoken a mind scarcely broken in to the metaphysics, so to speak, of natural science. The relations between osseous and cuticular teeth are thus described.

> "The transition from the one class to the other is not, however, sudden; for, although we do not find a mixture of the two structures in any tooth, yet we do find that the osseous teeth of some animals have properties in common with the cuticular teeth. Thus the incisor teeth of the rodentia grow during the whole life of the animal, and the cutting edge becomes extra-vascular from the recedence of its tubuli from their connexion with the pulp-cavity; a condition common to cuticular teeth, which are constantly increasing at the base and are extra-vascular. Indeed, these ever-growing teeth may in many respects, save in situation, be compared to the nails and horns. From these considerations we must admit the bills of birds and of tortoises as belonging to the dental system.
>
> "A further confirmation of this opinion will be found in the manner of development, as instanced in the mandibles of the parrot and tortoise. In the family of birds called Merganser, the mandibles are armed with sharp conical teeth.
>
> "In the jaw of the balæna mysticetus, we have the whalebone developed in the place of teeth; the structure of which is somewhat similar to that of the mandibles of birds.
>
> "By making this distinction in the two kinds of teeth, we are able to understand the conflicting opinions that existed so many years, but which recent investigations have set at rest; namely, as to whether the teeth were to be regarded as possessed of life and organization like bone, or as inorganic excretions like the hair and nails." (pp. 5-6.)

Here we see that the mere modifications of the mandibles in the merganser into dentiform processes, is instanced as a proof of the relation of the beak of birds to the teeth of mammalia! and again the same relation is exemplified by the continued growth of the rodent teeth of certain mammalia, and of the cuticular covering of the jaws in birds and tortoises! It is true that a certain vagueness of expression renders the precise ideas of Mr. Tomes on this point somewhat obscure, as, for example, the sentence "these ever-growing teeth may in *many* respects, save in situation, be compared to the nails and horns." Now it really appears to us that the *only* comparison which can be drawn between nails and horns on the one part, and true rodent teeth on the other, is that they *are* "ever growing." The true relation of ever-growing teeth, is clearly very close to those teeth which, after a certain fixed condition and development, never grow at all. The molares of the ruminant ordinarily grow during life, but it is only because the process of development is protracted to a long and almost indefinite period. In extreme old age we often find that roots are found in these teeth, and that, abstractedly, they have their fixed duration and their definite form, as well as the molars of the carnivora; and that the incisor teeth of the rodents do not follow any other rule, is to be deduced from their analogy to others in their growth and structure, and from the shedding of these teeth in their temporary form. To contend, therefore, for the relation between two normally different structures, merely from such an analogy as this, is clearly bad philosophy.

In order that we should not be supposed to found a general accusation upon a single instance, we will add another, in which an attempt is made at generalization, where the relation is merely accidental, and where in fact the illustration is not only imperfect, but absolutely inconsistent with itself.

> "The typical form of a tooth, as developed in the mammalian class, is a modified cone or combination of cones; the apex of the cone being sometimes on the masticating surface, at other times at the end of the root. In teeth that are perfected in a short space of time (as the human teeth), the masticating surface, or the base of the crown, will present the base of the cone; while the fang presents its apex; on the other hand, in cases where the development of the tooth is progressive through the whole life of the animal, the base of the cone will be at the edge situated deep in the jaw where the dental development is going on. In man the base of the cone is in the crown of the tooth, and the apex at the extremity of the root, or on the grinding surface.
>
> "In the front teeth we have a simple cone when seen from the front, the cutting edge corresponding to the base, and the end of the root to the apex of the cone; seen from the side, they present two cones united by their bases, the point of union being the neck of the tooth. The crown and the root of the canine teeth present each a cone uniting at the neck of the tooth.
>
> "The molar teeth exhibit in their fangs modified cones, which, uniting laterally at their bases, form the neck of the tooth, while the prominences of the grinding surface are formed of lesser cones." (p. 7.)

In the description of the different classes of the teeth, the anatomy of the neighbouring parts, the relation of the two jaws to each other, and other parts of descriptive anatomy, there is and could be no novelty calling for notice; we therefore proceed to consider the author's views on the intimate structure of these organs.

It is unnecessary for us to refer to the ordinary structures of which the teeth are composed, at least in their normal form and condition; but there are certain unusual or abnormal deviations to which it may be right to allude, on the one hand, in justice to our author who was the first to notice them, and, on the other, as bearing on the theory of the structure and formation of the teeth.

> "Cells," says Mr. Tomes, "are but seldom found in the coronal dentine. They are, however, occasionally met with, and when present, are usually large and elongated, and placed at an angle with the tubuli, which latter run into them. The occurrence of cells in this part of the tooth is in well-formed teeth unusual, while, in imperfectly-developed teeth, they are very numerous and irregular in size and shape; hence their presence in any case must be regarded as an indication of imperfect development." (p. 34.)

And again:

> "In some specimens, however, the tubes, during the greater part of their course, are subject to slight dilatations. These occurring at short and tolerably regular intervals, and in a number of neighbouring tubes, at similar points in their course, give a striated appearance to a moderately thick section, if viewed by transmitted light. The striæ are of course transverse to the direction of the tubes, and are short, extending, perhaps, across twenty or thirty tubes, and then stop short.
>
> "The terminal branches, having arrived at the line of junction between the dentine and enamel, are there lost, or recurve and anastomose with contiguous tubes, or pass across the line of junction into the enamel, or else end in a dilated extremity near the surface of the dentine." (pp. 34-5.)

In proceeding to notice a statement near the close of this passage, where it is stated that the tubuli "pass across the line of junction *into the enamel,*" it may be proper to make a remark or two on the degree of dependence which is to be placed on microscopic observations relative to the intimate structure of such substances as dentine and other elements of the teeth; and in these remarks we hope we shall be held guiltless of any desire to depreciate the legitimate use of this invaluable means of investigation, or to limit within unjustly narrow bounds the sphere of its real utility and practical application. We are quite aware that, in the eyes of many of our most distinguished modern physiologists, it is a crime of no slight degree of flagrancy to doubt the absolute infallibility of all the impossibilities which have been fathered upon this much-abused instrument. It is, one would suppose, a sufficient excuse for a slight degree of modest scepticism, to consider the extreme minuteness of the structures which require the highest powers of the microscope to detect them, the acknowledged inferiority of definition under those highest powers, and the discrepancies which continually occur in the statements and descriptions given by various observers of the selfsame structures, arising out of the latitude of interpretation permitted by the indistinctness of the view. But when we see that these descriptions sometimes involve an absolute discrepancy with what is known of the actual structure and formation of the substance described, we are surely justified in withholding our assent to the apparent fact, on the ground of its inconsistency with one which is actually certain. Now, such appears to us the exact case with the statement of Mr. Tomes just quoted,—that the dentinal tubuli occasionally "pass across the line of junction into the enamel." Another very remarkable instance, as it appears to us, of Mr. Tomes's mistaken estimate of the value of facts and of their relations, occurs in his considering the accidental cavities that now and then occur in the structure of the dentine, as analogous to the Haversian canals in bone.

"At present, the dentinal tubes and cells, and the pulp-cavity, have alone been described as existing in the dentine; but, in addition to these, we have, in many instances, canals for vessels traversing the tissue, just as we have the Haversian canals perforating bone. In the teeth of man, these vascular canals are never numerous, and occur only in a few specimens. I have seen eight or ten sections only in which the dentine is vascular.

"At the present time I have several such specimens in my possession. Generally, the vascular canals pass through the fang, from the outer surface into the pulp-cavity, or *vice versâ.* In one case I found a canal passing through the dentine that lies between the fangs of a molar tooth, into the body of the pulp-cavity. In each instance, many of the neighbouring dentinal tubes have terminated by open mouths on the surface of the canal, and in one case the canal was clothed on one side with a thin layer of cementum." (p. 49.)

Now, judging from Mr. Tomes's own figure, we are borne out in saying, that there is no analogy whatever in the appearance of the amorphous cavities there exhibited, and the Haversian canals. The cavities are clearly accidental, and may, in some cases, be prolongations from the pulp-cavity itself; a fact, which the formation of the tooth would entirely explain.—We do not mention these erroneous views in a spirit of cavil, or with a disposition to find unnecessary fault; but because, with a due regard to truth and fair criticism, we could not conscientiously pass over these minor blemishes in a work deserving of so much general consideration.

But, in the following passage, a more important error is, as we conceive, committed:

"To return to the consideration of the relative degrees of organization of the dental tissues. The cement, or tooth-bone, when collected in any amount, is possessed of vessels, as well as with cells and radiating tubes, in connexion with the vascular surfaces. To this element of the tooth we must, in accordance with the above plan, give the highest place. The dentine, possessing sometimes, though not constantly, vessels, has in all cases its tubes or capillary pores opening directly upon a vascular surface; this, then, must be considered as the second, while the enamel, itself without vessels, is only connected with a vascular surface by the intervening dentinal tubes, and holds the third, or lowest degree of organization of the three dental tissues.

"If the relative density of tissues be in proportion to the low degree of vitality, still the dental substances will hold the above arrangement. Again, if the relative sensibility of tissues be regarded as an index of their degree of vitality, still the same places must be accorded to the cement, dentine, and enamel.

"It is a law of nature, that in passing from one form of organized matter to another, no sudden transition shall be made, but that the individual changes shall be so gradual as to be almost imperceptible.

"This law we find beautifully exemplified in the gradual change of structure in passing from the cement to the dentine, and from the latter to the enamel.

"The cement and dentine possess so many properties in common, and are often so like each other, that in some specimens it is difficult to determine to which of the two tissues the various parts belong. Thus, in the cementum, we find tubuli terminating in open mouths upon a vascular surface, such as the surface of the root of a tooth, while in the dentine we observe the presence of cells with radiating tubes: indeed, we see the dentinal tubuli themselves taking the form of cells with tubular branchings. In the tooth of the kangaroo the two tissues are fairly mixed. Numerous vascular canals pass from the pulp-cavity to the exterior of the tooth, and each canal has scattered round it cemental cells, the radiating tubes of which either pass into the canals, or connect themselves with the dentinal tubes. Some few specimens, taken from the fangs of human teeth, exhibit the two tissues fairly intermingled,—tubes with distinct parietes lie amongst the cemental cells, and anastomose with them.

"From the facts which I have laid before you, we are led to infer, that the dentine is but a modification of the cement; that the dentinal tubes are but elongated cemental cells, and that this elongation is necessary to enable the tooth to perform its allotted part in the animal economy.

"In tracing the relations existing between the dentine and enamel, we find the change in passing from one structure to another equally gradual." (pp. 63-4.)

Now here we are obliged at once to join issue with the author. The dentine is formed from without inwards. The cement is deposited subsequently upon the surface of the already existing dentine, from within outwards. The first is either deposited upon the surface of the pulp by a vascular formative structure surrounding it, according to some observers, or, according to others, by the conversion of the substance of the pulp into dentine; the other is the product of an enveloping body, "the cemental pulp," in the words of the author. On the crown of the tooth, these two substances are actually separated by a third and still distinct substance, the enamel, in those animals in which the three substances are coexistent and all three extensively deposited. We have already, in the pages of one of our predecessors,* given some reasons which have led us to believe that the conversion theory is untenable; but that is not the present question.

The descriptions of the structure of the three component portions of a tooth are given in very full detail, and are illustrated by numerous and well-executed woodcuts. The analogies between the dentinal and the true osseous tissues are elaborately given; and although in some portions of the following extracts we still see reason to complain of the want of correct appreciation of affinities, we think it due to the author to give the passage entire, especially as in it he lays claim to an independent, if not a prior, discovery of some of the most important peculiarities in the dentinal structure. At the close of the passage it will be seen that Mr. Tomes alludes to a controversy, into which, as he is himself contented with a modest and simple statement of its occurrence, we do not consider ourselves called upon to enter.

---

* Medico-Chirurgical Review, Vol. III, N. S., p. 302.

"From what has already been said of the dental tissues, it will be seen that the area of the tubes of the cemental cells, and of the tubes or interspaces of the enamel, form no part of the tissues themselves, but are in fact spaces in them. These spaces, we have seen, are of characteristic form in each tissue, and we have considered their relations, &c. &c. It now remains to say a few words on the structural relations of the dental to other tissues. Before going to this point, allow me to state, in a few words, the ultimate structure of osseous tissue, in order that, in comparing the dental structures with each other, we may understand their relations to the tissues to which they are most nearly akin.

"Bone is composed of extremely minute granules closely united to each other, and so disposed as to form laminæ. The laminæ are concentrically arranged, the inner layer forming the parietes of a tube for the transmission of one or more vessels. Between, or in the lines of the laminæ, cells of an oval or round shape, flattened on their sides, occur, from which proceed numerous minute branchings—tubes which are directed either to a vascular surface, on which to end by an open mouth, or advance towards other tubes, with which to anastomose, thereby connecting the cells. Thus, in bone, as in the cementum, and in dentine, and indirectly in the enamel, we find a set of capillary tubes, commencing upon a surface, bathed by vascular currents, and passing into the structure, there to establish a perfectly continuous network of tubes; so that a fluid entering at one point may find its way through the whole mass. That this arrangement is subservient to the nutrition of the texture, is sufficiently apparent, when we consider its relations to the vascular system, coupled with the fact, that these tubuli must be filled with fluid, even by atmospheric pressure, and that the only source of fluid is the blood.

"That the tubes do contain fluid is proved by the following experiments: after removing several teeth, I carefully wiped the external surface; I then broke the tooth, removed the pulp, and made the surface dry. The fragments of dentine and cement (the enamel having been in great part broken off) were then placed in a warm room to dry, and in the course of a few hours lost one part in eight by weight, without having suffered any loss in bulk. On repeating this experiment with another batch of teeth, I found they lost only one part in twelve; and in a third experiment, seventy grains lost only four grains.

"From these considerations it is seen that osseous and dental tissues are, in the form and arrangement of their cells and tubes, very closely allied. But the relations of the ultimate tissues are yet closer; for dentine and cementum, and probably enamel, are built up, like bone, of more or less spherical granules, the difference in the tissues being in the arrangement of the granules, and in the relative quantity of earthy matter with which they are impregnated.

"Till lately, little was known upon the structure of the teeth, and that little was overlooked. Luenooke (!) made out the dental structure to be tubular, but his observations were disregarded until the subject was taken up by Purkinje. Professor Retzius, of Stockholm, about the same time, pursued the minute anatomy of the teeth, and with similar results to Purkinje; these were published in Stockholm in 1836. Late in the year 1837, prior to the appearance in this country of the works either of Purkinje or Retzius, I commenced the investigation of the structure of the teeth, under the impression that little or nothing was known upon the subject. The whole of my time which was not occupied on lectures, or necessary professional study (I was then a pupil at King's College), was devoted to the pursuit of dental anatomy, and in a short time I found many points, as I then thought, quite unknown. After I had worked up the subject, so far as human teeth were concerned, and examined the teeth of all the common animals, I became acquainted with Professor Owen, who, after examining many of my preparations, and accepting a few, mentioned, in his lecture at the College of Surgeons in 1838, my researches in terms of great commendation, as having confirmed those of the continental anatomists, and in some points extended further than they. I drew up an account of my researches in a paper, entitled 'On the Structure of the Teeth, the Vascularity of those Organs, and their relation to Bone,' which was read before the Royal Society, June 21st, 1838, having been presented by Thomas Bell, Esq. This was the first account written upon the structure of the teeth in this country; and, in addition to the confirmation of Purkinje's and Retzius' views, it contained several new points. The most important of these was the vascularity of the tubular structure.

"At a later date in the same year, Professor Owen read a paper on the structure of the teeth before the British Association. Shortly afterwards Mr. Nasmyth published a work on the same subject, which contained a full account of Retzius' views, together with a detail of the results of his own researches." (pp. 64-7.)

On the subject of the origin and development of the rudiments of the teeth, Mr. Tomes appears to follow implicitly the views of Mr. Goodsir; and as it is not our object on the present occasion to enter into any controversy on this matter, we pass over this part of the work with the mere remark, that it requires repeated independent observation to confirm or refute Mr. Goodsir's doctrine, which has appeared to us to be far from satisfactory. We must not, however, wholly pass over Mr. Tomes's account of the conversion of the pulp into dentine. We are anxious to give the author the advantage of a long quotation from this part of his work, not only as exhibiting his own views on one of the most important and interesting parts of his subject, but also as affording a good example of his mode of investigating and theorizing on a difficult and intricate question.

"The papilla, which is now designated the pulp, assumes the shape of the future tooth, and commences to fulfil the ultimate purpose of its existence, in the formation of dentine. Previous to the development of tooth-substance the inner surface of the sac becomes separated from the surface of the pulp, the intervening space being occupied by a soft gelatinous granular matter. This is the formative pulp for the development of the enamel. At this stage of dental formation we have a closed sac containing two formative pulps, one for the development of the dentine, the other for the formation of the enamel; the former in the shape of the crown of the future tooth, the latter forming a cap over the dentinal pulp. At a later period we have a matrix for the formation of the cementum.

"The dentinal pulp, from its earliest appearance to the time of its transition into dentine, is mainly composed of a series of nucleated cells, united and supported by plasma, is supplied with vessels, and, during the greater part of its existence, with nerves.

"In addition to these elements, there is one other, that pertains to the pulp, prior to the commencement of those changes which prepare for its conversion into dentine. This element is a fine areolar tissue. A meshwork of delicate fibres and bands of homogeneous matter, in the thicker part of the walls of which are scattered here and there nucleated cells; while the meshes are occupied by a thick, clear, homogeneous fluid or plasma; and in this are a number of nucleated cells.

"In order to examine the pulp when in this early stage of development, very thin sections must be taken from the part most distant from that where calcification has begun, or is about to begin; or any part of the pulp may be examined at a very early period of its existence. Then, with a high magnifying power, the conditions I have described will be recognised; and, in addition, loops of capillaries, with their convexities directed towards the coronal surface of the pulp, will be seen.

"I am not aware that the dentinal pulp when thus constituted has been before described. It is, however, when in this stage, an important subject for examination; since it is more than probable that a partial persistence of the areolar tissue occasions that imperfection in formation of the dentine to which I drew your attention in a past lecture.

"I shall presently recur to this point, and, to avoid circumlocution, will call this the *first* or *areolar stage* of the dentinal pulp.

"If we take sections from the pulp, intermediate between the coronal surface and the point I have described, the microscope will show them to be composed of nucleated cells, and a sub-granular uniting medium. The cells appear to have almost homogeneous coats that inclose a cavity which is commonly occupied by granular matter.

"In this, the *second* or *cellular stage* of the dentinal pulp, the cells have no definite arrangement as regards each other, but are thickly scattered, at pretty regular intervals, throughout the whole section. The pulp, too, is something firmer in this than in the earlier stage.

"Immediately preceding the conversion of the pulp into dentine, its elements undergo a third change. The component cells acquire a linear arrangement. Those nearest to the coronal surface are the first to take this position. The so-formed columns are, as regards the pulp, nearly vertical to its coronal surface, and lie nearly parallel to each other; holding, in fact, the same relative position amongst themselves as do the dentinal tubes of the corresponding part of a perfected tooth. In a favorable section the columns of cells will sometimes separate from each other, each line of cells taking with it its share of the sub-granular matter in which it is imbedded.

"For the convenience of description, we will call this the *third* or *linear stage* of the pulp.

"Neither in this, nor in the second stage, have I been able to find more than the faintest trace of the areolar tissue, which forms a conspicuous part of the pulp in its earlier stage of formation.

"The blood-vessels, too, become rare in the second stage, and wholly disappear in the third.

"I have assumed this arbitrary division into stages, for the sake of facilitating the description. In the advanced dentinal pulp, you will find the three conditions, not separated from each other by distinct hard lines of demarcation, but beautifully blended the one with the other—passing from the one extreme of condition to the other so gradually, that the transitions are not at first recognised, and when fully recognised, are again lost in the gradations towards a further change.

"The linear arrangement of the cells commences in those nearest the coronal surface of the pulp, first at its apex, afterwards at its sides.

"Those cells on and near the surface are the larger, the relative size decreasing the further they are removed from the surface. The lesser cells, however, increase to an equal size to the larger ones, when the time for their calcification arrives. Intermediate and connecting laterally the lines of cells is the sub-granular and now firm gelatinous tissue, corresponding in position to that described as the plasma, with which it is identical, but is now further developed. Each cell after falling into line divides into two or more in its length, and each division elongates. A central nucleus or space is seen in each cell, which lengthens with the cell. The cells by their increased length become placed end to end, and ultimately unite; and the elongated central space of each individual, by a further development, joins and opens into those of the superimposed cells; thus forming a central tube common to the linearly-united cells. At, or a little before, this period of development, the earthy matter is received into the cellular or rather tubular and inter-tubular tissue, whereby the gelatinous matrix, having assumed the required form, is converted into tubular and inter-tubular tissue; in other words, into dentine. In some instances the linearly-arranged cells have two or even three central cavities, but these, in the progress of development, become joined in one. Sometimes they appear empty, at other times occupied by granular matter. In either case they are usually described under the name of nuclei.

"So soon as the cells have begun to receive the earthy constituents, they become adherent to the previously formed mass of dentine, and then undergo the final development into tubes. Thus it happens, that the dentinal pulp, when separated from its cap of dentine, brings with it few, if any, of the calcifying cells.

"Covering the pulp is a transparent membrane closely united to the external cells. This membrane, which forms the exterior of the dentine, is the first to undergo calcification. Upon its external surface are developed the hexagonal pits for the reception of the ends of the enamel fibres, each pit corresponding to the end of a line of cells.

"The progressive stages of development are very readily observed in the molar teeth of the hog, or in a kitten, at birth. In each of these animals, and in the calf, I have examined the formative pulps, and from these examinations I have given this description, and have made my illustrative diagrams.

"Calcification begins on the surface of the pulp commencing at the apex or apices, and gradually extends down the sides and towards the centre. As the more external and larger cells become hardened, the inner ones increase in size, assume the linear arrangement, and they in their turn become converted, by the addition of the salts of lime, into dentine, till at last the great bulk of the pulp is transformed, leaving only a comparatively small portion, which, with the nerves and blood-vessels, occupies the central cavity of the tooth.

"In the progressive development of dentine from without inwards, two of the more external unite with one of the more internal lines of cells. The two central canals also unite and form one. By the frequent repetition of this process of union between the contiguous cellular lines, as they proceed inwards and towards the centre of the pulp, the number of tubes are gradually diminished, and the trunks of the dentinal tubes are formed. Here, then, we have the ordinary manner of growth reversed. The branches are formed first, and the trunks are developed from the union of the branches.

"The structure composing the walls of the cells prior to the addition of the phosphate of lime, is, as far as has at present been seen, homogeneous; further investigation will, however, probably show it to be minutely granular, and similar to the ultimate structure of the inter-cellular tissue of bone.

"The cells, after their confluence and impregnation with lime, and subsequent adhesion to the mass of already formed dentine, may still for a while be distinguished, their perfect conversion into tubes being gradually completed.

"The most external cells of the coronal dentinal pulp fall into lines, and contribute to form the ultimate branches of the coronal tubuli. The peripheral cells of the dentinal pulp of the fang do not, however, fall into lines, but exist as a coherent mass; excepting a few only, and these contribute to form those branches of the tubes that pass into the cementum. Many of the peripheral cells are like large granules, having no nuclei, while others have nuclei which become developed into cells, with which the terminal branches of the dentinal tubuli communicate. Other irregular cells are formed by intervals existing between the larger granules at the time of their calcification." (pp. 83-89.)

But we must pass on to the more practical portion of the work, in which we find continual instances of sound and judicious treatment founded upon accurate and acute observation. With such opportunities as the author has enjoyed for a long period, combined certainly with much talent and a hearty interest in his profession, it would be impossible that the result should be otherwise than highly valuable. "Eight years since," says the author, "when resident within the walls of this hospital, filling the office of house-surgeon, I determined to direct my attention to diseases of the teeth, and to practise exclusively this branch of the reparative art. From that time to the present, it has been my habit to take tolerably full notes of the more instructive cases of dental disease that have come under my notice, and to these have been added others related by the various writers on dental surgery, whose works I have read." That more might have been made of the opportunities subsequently enjoyed by Mr. Tomes, as the dental surgeon of the same hospital, we have before ventured to express our conviction; still the value is not small of what has been done, as embodied in this division of his work. The statistics of tooth-drawing may, at first sight, strike the reader as more amusing than useful. There is, however, a valuable practical bearing in the construction of such tables as those with which Mr. Tomes has furnished us, and which are given in full at the close of the volume. The following abstracts are sufficient for our purpose, and indeed for all practical purposes whatever.

"Table I.

"Of 3000 permanent teeth, extracted from various causes,

72	were	central incisors.
117	„	lateral incisors.
78	„	canines.
273	„	first bicuspides.
434	„	second bicuspides.
1124	„	first molars.
637	„	second molars.
265	„	third molars.
3000		

"In this table is expressed, of the teeth lost, the relative amount of each kind of tooth; and from this, in conjunction with others, we may learn which tooth to sacrifice, if one of several sound ones must be removed to make room for the others." (pp. 140-1.)

The second, third, and fourth tables we pass over, as taking up too much space, and as requiring a long explanation to render them intelligible; but in the fifth we have shown the relative frequency of the several conditions which render the removal of teeth necessary.

"Table V.—*Showing the relative frequency of the several conditions that render the removal of the teeth necessary.*

	Caries.	Necrosis.	Exostosis.	Periostitis.	Loose and painful, from absorption of the gums and alveoli.	Insufficient size in the jaw.	Mechanical injury.	Neuralgia and tic douloureux.	Pain in the teeth.
	p. ct.	p. ct.	p. ct.	p. ct.	per cent.	per cent.	per cent.	per cent.	p. ct.
Central incisors .	33½	4	0	1½	41½	2¾	16¾	0	0
Lateral incisors .	48¾	3½	0	1¾	24	19½	1½	1	0
Canines	40½	0	0	2¾	33¾	23	0	0	0
First bicuspides .	81¼	1	0	1	9½	6¾	0	0½	0
Second bicuspides	86¾	1	0¼	1	9½	0½	0¼	0½	0¼
First molars . . .	96	0¼	0	0½	2¾	0	0	0½	0
Second molars . .	88¾	0¾	0	1	9	0	0	0	0½
Third molars . . .	82¼	0	0	2½	13	1	0¾	0¼	0¼

"The table reads thus:—Of central incisors extracted, 33½ per cent. were removed to relieve conditions resulting from caries, 4 per cent. from necrosis, 1½ per cent. periostitis, 41½ from absorption of the gums and alveoli, the teeth being sound, 2¾ from irregularity, arising from insufficient size in the jaw, 16¾ per cent. were removed in consequence of having been broken or injured by falls.

"TABLE VI.

"If we take the teeth without separating the different kinds, and examine the causes of loss, they will be found to hold the following proportions:—
Thus 88¾ per cent. were removed for conditions resulting from caries.
¾ per cent., ditto, from necrosis.
1 per cent., ditto, from periostitis.
9 per cent., ditto, from absorption of the gums and alveoli.
2¼ per cent., ditto, insufficient size of the jaw.
¾ per cent., ditto, mechanical injuries.
¼ per cent., from neuralgia.
¼ per cent. (nearly), from pain in the teeth, without apparent disease of the dental or of the adjoining tissues." (pp. 149-50.)

These statements are interesting to the dental surgeon, and doubtless present a near approximation to the truth.

In the treatment of irregularity of the permanent teeth, it appears to us that Mr. Tomes is not quite free from the common error of treating them *too roughly*. The application of pressure, by means of metallic plates applied to the irregular teeth, and placed over those of the opposite jaw, is a contrivance so merely mechanical, and has so little regard to the real condition of the teeth themselves, in relation to the alveoli and other parts belonging to them, that we were prepared to find Mr. Tomes preferring a mode of treatment somewhat less violent and injurious. But even that which he advocates, must, we should think, in many cases occasion permanent loosening of the teeth; although its ingenuity, as well as its efficacy in fulfilling the immediate object in view, would probably recommend it to most practitioners.

" . . . A fixed point is gained by fitting a piece of ivory to the hard palate and the surfaces of the necks of the teeth, to which it is tied, so as to prevent the possibility of motion: pieces of caoutchouc, or dry compressed wood, are then interposed between the ivory and the necks of the teeth you wish to be moved outwards. The wood on absorbing moisture, swells, and, as the fitted ivory cannot be moved backwards, the teeth are necessarily moved forwards. After the wood has remained about forty-eight hours, it should be removed, and a fresh piece of dry wood put in its place, which, on swelling, forces the teeth still further onwards. The renewal of the wood is repeated till the teeth have assumed the required position; the ivory is then removed from the palate, and the caps from the teeth, and the process is allowed to be completed by the action of the upper and under teeth upon each other; the under forcing the upper outwards, and the upper pressing the under inwards, till the molar teeth come in contact, when the front teeth assume their permanent place." (p. 159.)

On the subject of want of room for the normal development of the dens sapientiæ, that fertile source of varied and severe mischief, there are many very interesting cases, and sound practical suggestions. Indeed we are not sure that we do not prefer this to any other section of the work.

The quæstio vexata of the nature and origin of "caries" does not receive any particular illumination from the author. After quoting the opinions of two authors who have advocated opposite views on this subject, one believing that the cause is internal, and essentially connected with a diseased condition of the dentine, and the other that it consists exclusively in chemical decomposition from external causes, Mr. Tomes solves the difficulty by considering both these opinions right, "when taken together, but that neither taken alone will suffice to account for all the phenomena of caries," and he adds,

> "I believe that the dentine from abnormal action loses its vitality, and with the loss of vitality the power of resisting chemical action, and that consequently the dead part is, under favouring circumstances, decomposed by the fluids of the mouth.—That there must be a concurrence of dead dental tissue, and of a condition of the oral fluids capable of decomposing the dead part, before the phenomena of caries can be developed." (p. 199.)

Now this is precisely the view taken by the former of the authorities to which Mr. Tomes professes to demur. The definition of caries and gangrene to which we allude is as follows,—"Mortification of any part of a tooth producing gradual decomposition of its substance;" and we really see no difference between the practical bearing of these words, and of those of Mr. Tomes, who states it to be "death and subsequent progressive decomposition of a part or of a whole tooth." Mr. Tomes, it is true, considers a peculiar condition of the oral fluids necessary to render them "capable of decomposing the dead part, before the phenomena of caries can be developed," a statement to which we presume no animal chemist will be ready to assent, as nothing but the access of warmth and moisture are required to effect the decomposition of dead animal matter. This is another instance in which the author hunts for a recondite or superfluous explanation of a phenomenon, which requires only the ordinary simple and obvious one for its solution.

Our space warns us, however, that we must now recommend our readers without reserve, to a careful perusal of the remaining portion of this work, assuring them that they will find in it a large portion of sound practical instruction. We avoid, for obvious reasons, offering any observations on that part of his work which treats on artificial teeth, as wholly out of our "line."

In reviewing this work, we have bestowed more time and space upon its consideration, than to some of our readers probably it will appear to have demanded. But we have done so, because it seldom comes within our office to introduce to the profession a work of real practical merit on this surely not unimportant subject; and because we think it of great consequence that the medical practitioner should have his attention called, as frequently as possible, to a class of diseases, which are usually but little cared for, and as little understood.—We should be committing an act of injustice both to Mr. Tomes and his publisher, were we not to mention that the work is "got up" in the very best style, and that many of the wood-engravings rank among the most beautiful productions of modern art.

Art. XII.

A Practical Treatise on the Use of the Microscope, including the different Methods of preparing and examining Animal, Vegetable, and Mineral Structures. By John Quekett, Assistant Conservator of the Museum, and Demonstrator of Minute Anatomy at the Royal College of Surgeons of England. Illustrated with Nine Plates and Two Hundred and Forty-one Wood-engravings.—*London,* 1848. 8vo, pp. 464.

We know of no subject on which a good book was more needed, than on the Use of the Microscope; and we could not name any individual more competent than Mr. Quekett to write one upon it. Early disciplined in its use, devoted to its employment as a favorite object of pursuit, enjoying, from his position at the College of Surgeons, the most extensive opportunities for carrying forward the class of investigations in which its value as an instrument of research has now been abundantly tested, in continual communication with almost every one of the principal microscopists of this kingdom, and himself distinguished not merely by his acquaintance with what has been accomplished by others, but by the zeal and success with which he has followed out various branches of original inquiry, Mr. Quekett unites all the qualifications, save practice as an author, which could give a value to his production. Our strong desire to see our young microscopists furnished with a faithful guide, under whose direction they might make their instruments really do good service to science, instead of allowing them to lie comparatively useless in their cases,—coupled with the anticipations which our personal knowledge of Mr. Quekett led us to form,—caused us to look forward to the long-delayed appearance of this work with no common interest; and it gives us great pleasure to be able to say with sincerity, that our expectations have been in the main fully realized. We do not mean to say that the book is incapable of improvement; there are no human productions which are not; but after having carefully perused it, and carefully tested it by our own experience (which, if less extended, has been at least a long as our author's), we can most conscientiously and cordially recommend it to all such of our readers as, already possessing microscopes, desire to know how to bring them into most advantageous use, and to such as are only deterred from acquiring the instrument by the want of knowledge how to use it.

We shall not attempt to make an analysis of this book; for the simple reason that no analysis could be of any use to our readers; and our notice of it will be entirely confined to an enumeration of its principal contents, and to an indication of the points on which we think there is most room for improvement. After what we have already said, we feel sure that neither Mr. Quekett nor our readers will accuse us of a captious spirit in specifying these; did we entirely pass them by, we might be charged by some of the latter,—who will undoubtedly be acute enough to discover, in making themselves acquainted with the book, that it is not perfectly faultless,—with having given it too unqualified a recommendation.

The work is divided into three parts: the first treating of the *mechanical arrangements* of the microscope; the second, of the use of the *microscope and its appendages;* and the third, of *manipulation,* including the methods of preparing and mounting objects.

As an Introduction to the first part, we have a chapter on the history of the microscope, without which the book would certainly have been incomplete. We think, however, that the history might have been somewhat more restricted to the progress which the instrument has made, in the hands of its various improvers, towards its present perfection; and that in the place of some of the descriptions and figures of the earlier forms, which are of little value save to those who have a taste for antiquities, it would have been interesting to have had a fuller account of the labours of some of those early microscopists,—Hooke, Leeuwenhoek, Malpighi, and others,—who so remarkably made up by their industry and penetration for the imperfection of their instruments, and made such near approaches on many points to the discoveries of modern times. We have no such fault to find with the later portion of this introduction, in which the history of recent improvements is detailed; since we deem it but right that every important step in that truly wonderful advance, by which, in little more than twenty years, the microscope has been metamorphosed from a scientific toy into an instrument second to none in its capacity for the revelation of the hidden operations of Nature, should be attributed to its real originator. Here we think that our author might have been a little fuller with advantage, and have given us somewhat more information respecting the labours of foreign opticians, especially those of Germany, who have received scarcely any mention. We happen to know, from a careful comparison of instruments, that about ten years ago, the achromatic combinations of Schiek of Berlin, and of Ploëssl of Vienna, were decidedly superior, in many points, to those of our best London makers; and though we believe that the latter are now decidedly the first in the world, it would have been as well to state in what their superiority consists, in order to prevent even the appearance of that exclusive spirit of which we frequently complain in our continental neighbours. They would seem to be very closely pressed, of late, by a young American backwoodsman, of the name of Spencer; an achromatic object-glass of whose construction is stated to have detected three sets of lines upon a species of *navicula,* which other glasses of equal power, made by the first English opticians, entirely failed to define. The latter, we suspect, must either have been constructed some few years back, before they had arrived at the present perfection, or they could not have received a fair trial; for the identical specimen of the *Navicula Spencerii* which they failed to resolve, having been sent to this country, the lines described by Mr. Spencer could be clearly seen, and (what is a sufficient test of the superiority of the British lenses) could be resolved into a series of minute dots. That so near an approach could be made, however, to our boasted superiority, by a self-taught artist, living out of the sphere of that emulation which has been so fertile in improvements among the opticians of England, France, Germany, and Italy, indicates an extraordinary amount of ability on his part, from which we may expect yet greater results.

We notice in Mr. Quekett's writings, as in those of many other authors, a want of proper discrimination between the terms *invention* and *discovery.* Thus we are told (p. 9) that Newton *discovered* the compound reflecting microscope, and again (p. 14) that we owe to Lieberkuhn the *discovery* of the solar microscope. Yet in the very next sentence, we have "the solar microscope, as *invented* by Lieberkuhn." Here, then, these terms are used synonymously. But that they are not really synonymous, must be apparent to every one on a little consideration; for who would speak of the *invention* of the circulation of the blood? We apprehend that the term *discovery* is only applicable in cases where something which previously existed, but which had remained unknown, has been brought to light; thus, that the parenchyma of the liver is made up of cells, is an important discovery. On the other hand, we employ the term *invention* in cases in which the product emanates from the mind of the inventor; thus we speak of the invention of the steam-engine, of the telescope, microscope, &c., which have not resulted from observation, but from a constructive effort of mind.—This, however, is a minor blemish, which we should not stop to correct in Mr. Quekett's book, if the error were not so frequently committed by others.

The First Chapter treats of the *Simple Microscope*, of the *Doublets and Triplets* which are used in the manner of simple lenses, and of the various forms of *Dissecting Microscope.* The last of these three heads is the one on which our author most profitably dwells; since single microscopes will never be used again by scientific men, save as mere magnifying glasses for examining the general characters of objects with low powers. The doublets and triplets were vast improvements in their time; but no one who can avail himself of an achromatic compound microscope, will ever employ them in preference. A great variety of forms of dissecting microscopes are described and figured; and the reader is rather likely to be disturbed by the *embarras des richesses*, in the absence of any very definite indication afforded by Mr. Quekett's own experience as to which he should prefer. For ourselves, if a single or doublet microscope be alone required, we do not hesitate to avow our preference for that invented by Mr. Slack (p. 57). Where it is desired, however, to add a compound body to the same apparatus, the instrument of Messrs. Powell and Lealand (p. 52) will be found extremely advantageous, especially if furnished with the erecting eye-piece and draw-tube, by means of which a great variety of power may be attained.

In Chapter II, Mr. Quekett passes on to the *Compound Microscope;* and here, after a general account of the essential components of all forms of the instrument, he describes and figures, with most laudable impartiality, no fewer than nine different forms of microscopes, constructed by London makers, namely, three by Messrs. Powell and Lealand, two by Mr. Ross, and four by Messrs. Smith and Beck. Here, again, those not in possession of any previous information on the matter will be perplexed in their choice, by the want of some statement of the relative advantages of these different forms; and we cannot but think that, from a practical microscopist, who has had opportunities of using them all, some such indication might be fairly expected, without justly giving occasion to any suspicion of favoritism or unfairness. In fact, all parties are now so well agreed upon certain general principles, that the differences are only in details; and we think it would have been preferable to have selected a single instrument of each maker, which might have been regarded as the type of a class, its special merits being elucidated, and the chief variations in construction by the several makers briefly explained. Thus we are ourselves satisfied that, for simplicity and cheapness, combined with sterling excellence of workmanship, nothing that is made comes near to the larger of Messrs. Smith and Beck's "Student's Microscopes" (fig. 1, plate 4), in which the peculiar excellence of their plan of construction—the mode in which the body is supported and moved—is as well seen as in their larger and more costly instruments. So, again, we believe that Mr. Ross may say with truth that his large microscope (plate 1) is equally complete and perfect in its way, and may be fairly taken as the type of a costly instrument furnished with all desirable appliances. The mode of supporting the body adopted by Mr. Ross has this advantage over that of Messrs. Smith and Beck, which is in other respects preferable—that the body can be turned completely away from the stage, which, for certain purposes, is very convenient. And of the productions of Messrs. Powell and Lealand, their portable microscope (p. 80) would, perhaps, be a very fair sample. By a little abridgment and discrimination in these particulars, Mr. Quekett would have found room for an account of some of the Parisian and German microscopes, most of which are constructed on a plan very different from those of London makers ; and this would have been the more useful, as many students and medical travellers purchase microscopes abroad, under the idea that they are better as well as cheaper than those of London makers,—from which it would have been well to undeceive them. Considering, too, that the vast amount of microscopic information which has emanated from the Continent, bespeaks acquaintance on the part of the makers of these instruments as to what is practically valuable, we should have liked to find our own knowledge extended by a few examples of the varieties in microscopic construction, which have found favour with our industrious brethren of Germany and France. The only one of which we find any account is the dissecting microscope of Oberhauser, described in a later part of the volume (p. 326). Some such addition might very well replace the account and figure of the cumbrous and imperfect microscope of Mr. Varley (p. 95), which no one, we think, save the inventor, would find any comfort in using.

In the next chapter we have an account of the various *accessory instruments* which may be adapted to the microscope for various purposes. This we shall pass by with little remark, as it contains nothing redundant or superfluous, and presents no deficiency. Although there is scarcely anything in it with which practised microscopists are not likely to be acquainted, yet it must be remembered that the book is not written for them, but for learners. As a novelty emanating from Mr. Quekett himself, we may specially mention the "indicator," which will be found very useful in class demonstrations. Chapter IV, on that important accessory, the lamp, gives very explicit directions for obtaining a satisfactory light ; on these, however, we need not dwell. With Chapter V, on the *magnifying powers of the microscope*, the first part of the work concludes. In this chapter we notice some repetition of the latter part of the introductory history, which should have been avoided. If the history were complete, nothing would be here required but a description of the optical action of the lenses, as now constructed by all the best makers. Here, as in his history, Mr. Quekett pays a just tribute to Mr. Lister, to whose combination of practical and theoretical knowledge we owe the development of powers in achromatic object-glasses, which it had not been previously imagined that they could attain.

In Part II, the *use of the microscope* is described, under the following heads:—Chap. I. Preliminary Directions. Chap. II. Illumination of Objects. Chap. III. Micrometer. Chap. IV. Determination of the Magnifying Power. Chap. V. Camera Lucida. Chap. VI. Polarization of Light. Chap. VII. Goniometer. All these subjects are very fully and clearly discussed. We should have liked an additional chapter, containing hints to young microscopists as to the interpretation of the appearances which they witness, and the sources of fallacy to which they are liable. Mr. Quekett's large experience as a microscopic demonstrator must have given him ample materials for such an exposition, which would have constituted, we think, one of the most valuable chapters in the whole treatise.

Those who are already in possession of microscopes, and who understand the method of using them, will probably turn to the Third Part with the greatest anticipation of benefit; and we do not think that they will, on the whole, be disappointed, although there are some omissions which we cannot but regret. The first two Chapters, on the *Diamond*, and on *Cutting Glass*, need not detain us. The Third treats of the methods of *Cementing Cells;* the Fourth, of the method of *Mounting Objects in Fluid;* the Fifth, of *Cements;* the Sixth, of *Preservative Fluids;* the Seventh, of mounting objects in *Canada Balsam;* the Eighth, of mounting objects in the *Dry Way;* and the Ninth, of mounting *Opaque Objects.* On all these matters, the author's large experience enables him to give a complete account, from his own personal trials, of all the most valuable methods that have been proposed; and the only fault we have to find with this portion of the book is, that he does not always clearly indicate which of these methods he himself prefers, or why he does so, which is exactly what his readers most want to know. Many very useful and ingenious methods of manipulation, which are but little known, save to those who are in intercourse with their several inventors, are described in this part of the work.—Having thus given directions for mounting objects when we have got them, Mr. Quekett proceeds to enlighten us as to where and how we are to "catch our hare," or, in other words, to instruct us in the arts of collecting and preparing objects. We have not been able to discover any particular order in the succession of subjects; and shall enumerate the heads of the chapters as they successively present themselves. Chap. X gives directions for making sections of *Bones and Teeth;* and Chap. XI for making sections of *Shell* and other *Hard Tissues.* As the hard tissues here referred to are of vegetable growth, it would have been surely more natural to have classed "shell" with "bones and teeth," in the preceding chapter; since it so closely resembles those substances, and the manner of preparing it is the same. The second of these two chapters would then have been left clear for the vegetable tissues, if it was thought requisite to make them form a distinct division. In Chap. XII we are instructed how to make *Sections of Wood* and of animal substances that can be treated in like manner. In Chap. XIII, we have general directions for the dissection and preparation of the *Softer Vegetable and Animal Structures;* with representations and descriptions of all the most useful instruments; and in Chap. XIV we find more specific directions in regard to the method of preparing particular textures, especially the *Nervous* and *Muscular*. The best methods of exhibiting the *Circulation of the Blood* are described in Chap. XV; and the *Circulation in Plants*, together with the mode of cultivating Chara, Vallisneria, &c., are treated of in Chap. XVI. In Chap. XVII are detailed the best methods of procuring *Infusory and other Animalcules;* the localities of the most interesting kinds found in the neighbourhood of London are pointed out; and directions are also given for mounting *Fossil Infusoria.* Chap. XVIII is the longest, and one of the most important in the whole volume; being devoted to a general account of the principal classes of microscopic objects, arranged in the first place under the heads of animal and vegetable, and then under various other minor divisions. Not only are the general sources indicated, from which objects of interest may be obtained, but lists are given of the specific forms which are most worthy of attention. The chapter also embodies a concise account of the principal forms of animal and vegetable tissue; which, coming from one so familiar with their characters, possesses considerable value. We are somewhat disappointed, however, at finding some matters omitted, or very scantily treated, as to which we had quite calculated on meeting with full information; such, for example, as the mode of making minute injections; and the whole chapter might be considerably improved in regard to the arrangement of its materials.

The next chapter, entitled *Methods of Examining Morbid Structures,* consists of only two pages; and we are constrained to say that the tyro in microscopy will find the directions contained in it by no means sufficient for his guidance. Perhaps Mr. Quekett has here put a restraint upon himself, from the consideration that his book is not intended for medical men exclusively; but he might have bethought himself that at least two thirds of his readers will be medical men, and that as he has fully treated polarization and other matters in which the interest is by no means universal, a subject so important to a very large proportion of microscopists should scarcely have been passed by with so little notice. We may notice, among other omissions, that we find no mention whatever of urinary deposits; for the examination of which the microscope is, or ought to be, more used by medical practitioners, than for any other single purpose. Chapter XX treats of test objects, and is illustrated by a series of extremely beautiful figures, drawn by Leonard, and engraved by Sowerby. Under this head is described a new test, first brought to this country (we believe) by Professor J. H. Bennett, which is remarkable for being an *artificial* production. It consists of a series of bands of lines etched on glass by M. Nobert; the lines on each band being at a certain definite distance from one another. In the first band, the distance of these lines from each other is $\frac{1}{1000}$th part of a Prussian *line;* in the tenth band, the distance is no more than $\frac{1}{4000}$th of the same quantity, or $\frac{1}{46608}$th part of an English inch; in the intermediate bands, the distance of the lines is gradually diminished in a geometric ratio. It has been regarded in this country as a wonderful performance to rule 10,000 lines to the inch; but our artists must acknowledge themselves beaten by M. Nobert, whose lines, when more than 4½ times as numerous, are as clear as those at a greater distance. This seems to be the best test of the separating or resolving powers of object-glasses that has yet been devised. Thus a $\frac{1}{4}$th inch glass of the best quality, and under the most favorable circumstances, will show nine of the bands, and will resolve the lines in seven of them; but no trace of the tenth can be seen. A good $\frac{1}{8}$th will define the lines in eight or nine of the bands, and will show the tenth; whilst, to define the lines in these last, a very good $\frac{1}{12}$th is required.—The last chapter contains some useful miscellaneous hints on the management of the microscope, and the conservation of microscopic preparations.

In bringing our notice of Mr. Quekett's work to a conclusion, we would remark that, although we have freely commented upon what we deem its redundancies and defects, we have nothing to say in disparagement of its accuracy in regard to the great variety of subjects on which it treats; and we can cordially recommend it, therefore, as a safe and valuable guide to Microscopists of all degrees of advancement, with the full assurance that there is not one among the most practised who will not derive from it some useful hints, whilst to the beginner it will prove of most important assistance. We must not omit to mention, that in the profuseness and excellence of its illustrations, it fully sustains the character of the Series of Standard Scientific Works of which it forms a part.

ART. XIII.

1. *Hints towards the Formation of a more Comprehensive Theory of Life.* By S. T. COLERIDGE. Edited by SETH WATSON, M.D. Oxon.—*London,* 1848. Post 8vo, pp. 94.
2. *Reflections on Organization; or Suggestions for the Construction of an Organic Atomic Theory.* By HENRY FREKE, A.B. M.B. T.C.D., M.R.I.A. —*Dublin,* 1848. 8vo, pp. 80.

THE question of the abstract nature of Life has, by common consent, lain in abeyance for some time. It has been felt that the new views opened up by microscopical and chemical research needed to be matured and digested,—the true relations of vital and physical phenomena to be more precisely determined,—and the share which the properties of inorganic matter have in the functional operations of the living organism to be more clearly discriminated,—before the metaphysics of physiology could again be profitably pursued. Yet it has seemed more and more obvious, in what direction speculation would tend. Physical philosophers have been gradually abandoning the idea of electricity, magnetism, affinity, &c., as *entities* distinct from matter; adopting the far more simple and logical method of viewing them as properties of the matter through whose intermediation they are exhibited. At the same time, they have been pushing their inquiries into the penetralia of the organized body; and have shown that, so far from there being nothing in common between the phenomena of life and the physical and chemical actions of inorganic matter, a large proportion of the former may be brought under the same category with the latter, being due to the operation of the very same properties under different conditions. But the physiologists, on their side, have not been idle. In proportion as the physics and chemistry of organization have been more clearly discerned, in that proportion has the residuum which neither physics nor chemistry will account for, been more markedly isolated; and this residuum has been shown essentially to consist in that series of phenomena, which are included under the collective designation of *cell-life*. Physiology, in fact, may be said to be the science of cell-life, as physics and chemistry are the sciences of matter in its (supposed) atomic condition. In the humblest cryptogamic plant, the phenomena of life are reduced to a simplicity which renders them incapable of further analysis without the loss of their distinctive character; and in the most complex structure we find the life of the whole to be nothing else than the aggregate or combination of the life of the individual parts, each of which is a repetition of the single cell, or of some modification of it. Whenever the philosophical physiologist may think it a fitting time to re-enter upon the path that conducts from the concrete to the abstract, we venture to affirm that he must follow the track which we have thus indicated, and must frame his definition of life so as to express the *idea* which is carried into action in the history of the single integral cell.

Strongly impressed with these views, we turned with much interest to the modestly-entitled "Hints" of Samuel Taylor Coleridge, a man whose influence upon the progress of philosophy is only now beginning to develope itself, and whose mind, with all its eccentricities, was gifted with that *insight* which predicts truths, at which others arrive by a more circuitous process. And, notwithstanding the imperfect knowledge of the subject which he possessed, and the errors of detail into which he fell, we are not disappointed in the general result, which is valuable both for itself, and also as showing the manner in which a really profound thinker may succeed in such a pursuit, whilst shallower speculators, with perhaps more of special knowledge, altogether fail. The editor has judiciously condensed in the preface which he has furnished, what may be termed the *argument* of the book; and we cannot do better than extract from this the portions which most completely embody Coleridge's views:—

"The prevalent natural philosophy of the present day, is that which is called *corpuscular*, because it assumes the existence of a first matter, consisting of *corpuscula* or atoms, which are supposed to be definite, though extremely small *quantities*, invested with the *qualities* of extension, impenetrability, and the like; and from certain combinations of these qualities, life is considered, by some persons, to be a necessary result. This philosophy Mr. Coleridge combats. The supposed atoms, he says, are mere abstractions of the mind; and life is not a thing, the result of atomic arrangement or action, but is itself an act or process. He refutes various definitions of life, such as, that it is the sum of all the functions by which death is resisted; or that it depends on the faculty of nutrition or of antiputrescence. His own definition he proposes merely as an hypothesis. Life, he says, is 'the principle of individuation,' that is to say, it is a power which discloses itself from within, combining many qualities into one individual thing. This individualizing principle unites, as he conceives, with the cooperating action of magnetism, electricity, and chemistry. At least such is the inference to be drawn from the present state of science; though it is conceivable that future discoveries may bring us acquainted with powers more directly connected with life. The most general law governing the action of life, as a tendency to individuation, is *polarity*......

"Beginning with magnetism, by which, in its widest sense, he means what he improperly calls the first and simplest differential act of *Nature* (he should rather have said the first and simplest conception that we can form of a differential act of God, in the work of creation), he supposes the pre-existence of chaos, as one vast homogeneous fluid; and even *that* he suggests, not as an historical fact, but as the appropriate symbol of a great fundamental truth. The first effort of magnetic power, the first step from indifference to difference, from formless homogeneity to independent existence, is seen in the tranquil deposition of crystals; and an increasing tendency to difference is observable in the increasing multitude of strata, till we come to organic life; of which the vegetable and animal worlds may be regarded as opposite poles; carbon prevailing in the former and azote in the latter; and vegetation being characterised by the predominance of magnetism in its highest power, as reproduction; whilst the animal tribes evince the power of electricity, as shown in irritability and sensibility. Passing over the forms of vegetation, we come to the polypi, corallines, &c., in which individuality appears in its first dawn; for a multitude of animals form, as it were, a common animal, and different genera pass into each other, almost indistinguishably. In the *mollusca* the separation of organs becomes more observable; in the higher species there are rudiments of nerves, and an exponent, though scarcely distinguishable, of sensibility. In the snail, and muscle, the separation of the fluid from the solid is more marked, yet the prevalence of the carbonic principle connects these and the preceding classes, in a certain degree, with the vegetable creation. 'But the *insect* world, taken at large (says Mr. Coleridge) appears as an intense *Life*, that has struggled itself loose, and become emancipated from vegetation—*Floræ liberti, et libertini!*' In insects we first find the distinct commencement of a separation between the muscular system, that is, organs of irritability, and the nervous system, that is, organs of sensibility; the former, however, maintaining a pre-eminence throughout, and the nerves themselves being probably subservient to the motory power. With the fishes begins an internal system of bones, but these are the results of a comparatively imperfect formation, being in general little more than mere gristle. In birds we find a sort of synthesis of the powers of fish and insects. In all three, the powers are under the predominance of irritability; but sensibility, which is dormant in the insect, begins to awaken in the fish, and, though still subordinate, is quite awake in the bird, of which no better proof can be given than its power of sound, with the rudiments of modulation, in the large class of singing birds, and in some others a tendency to acquire and to imitate articulate speech. The next step of ascent brings us to the *mammalia;* and in these, including beasts and men, the complete and universal presence of a nervous system raises sensibility to its due place and rank among the animal powers. Finally, in Man the whole force of organic power attains an inward and centripetal direction, and the 'apex of the living pyramid' becomes a fit receptacle for Reason and Conscience." (pp. 14-16.)

Now with much that is erroneous, and still more that is hypothetical, we consider that Coleridge has hit upon some great fundamental truths; and that the "principle of individuation" more appropriately embodies the *idea* of the existence of a living being,—that power of drawing-in and combining with itself the materials supplied by the world around, which is seen alike in the most complex and in the simplest organisms,—than any other which has been suggested. At the same time, we must protest against regarding this "principle" as an *entity*, or considering its action to be anything else than a manifestation of the properties with which certain forms and combinations of matter have been endowed. The remarkable prescience with which Coleridge fixed upon magnetism as the most general form of polarity, operating in the first arrangement of the previously homogeneous mass, cannot but forcibly impress those who have since learned inductively from the experimental researches of Faraday to regard it under this aspect. We shall conclude our notice of this interesting work, by citing the beautiful passage with which it opens;

which will show how truly one really great genius can estimate another of a far different order.

> "When we stand before the bust of John Hunter, or as we enter the magnificent Museum furnished by his labours, and pass slowly, with meditative observation, through this august temple, which the genius of one great man has raised and dedicated to the wisdom and uniform working of the Creator, we perceive at every step the guidance, we had almost said, the inspiration, of those profound ideas concerning life, which dawn upon us, indeed, through his written works, but which he has here presented to us in a more perfect language than that of words—the language of God himself, as uttered by Nature." (p. 17.)

We now turn to Dr. Freke's 'Suggestions for the Construction of an Organic Atomic Theory,' in which also we find much that is deserving of attention, though less of novelty, perhaps, than the author imagines. The book consists of three chapters: the first on *inanimate creation;* the second on *animate creation;* the third on *the blood.* Upon the first chapter we need make no other remark, than that it embodies those views of the actions of inorganic matter, which are made by the author to serve as the foundation of his subsequent speculations. We shall perhaps best exhibit his ideas of vital action, by quoting his explanation of the term *organizing atom,* which conveys the fundamental idea of his system.

> "What we design to convey is, that as evidence supports the opinion that at the creation of the inanimate universe an omnipotent fiat called mineral matter into being, in the form of minute indivisible particles, called mineral atoms; which mineral atoms, though limited in the number of their species, constitute, by the operation of the attributes impressed on them at their creation, the countless multitude of distinct forms of mineral matter found in the inanimate world, as also the harmonious arrangement now recognisable throughout inorganic creation—so it will be our effort to adduce evidence in support of the opinion, that at the creation of the world of organization, the same Divine power called organized matter into being, in the form of minute indivisible (that is, physiologically indivisible) particles, which we would call organizing atoms—which organizing atoms, though in all probability limited in the number of their species, have constituted, by the operation of the attributes impressed on them at their creation, that vast aggregate of distinct forms of organized matter which decorate with living loveliness an animated world, as also the harmony of arrangement visible throughout all organic creation. Each mineral atom is simple, that is, composed of but a single species of mineral matter. Each organizing atom is, we conceive, compound, that is, composed of a (it may be an inconceivably minute) combination of several distinct species of mineral matter, to which combination, at the atom's creation, was imparted the organizing agency to constitute it an organizing atom. This agency, by antagonising with the specific attributes of the atom's component constituents, retain them in a relative position distinct from that which would result from the operation of their chemical laws; while the function of each atom is, we conceive, to impart (on the destruction of the antagonistic equilibrium) the organizing agency to other components. We say, the organizing atom is composed of several distinct species of matter; but though anatomically (for it is not chemically) compound, it is, notwithstanding, physiologically atomic or simple. Each mineral atom we have seen to be under the control of but two classes of attributes, namely, those we have called its laws, general and specific; but each organizing atom is, we conceive, influenced by four distinct classes of attributes, namely, the attributes of its components, or inert matter's laws; and those attributes peculiar to it as an organizing atom, or, in other words, its organic laws, general and specific. And as on the inanimate laws, general and specific, which control the operation of mineral atoms, are constructed our sciences of physics and chemistry; so, in the organic laws, general and specific,

which regulate the action of organizing atoms, we recognise the objects of physiological research." (pp. 25-7.)

Now if our author, instead of the hypothetical "organizing atom," had used the term "cell-germ," we apprehend that he would have been much nearer the truth. Indeed, the apparent novelty of his] views in most instances results from his having simply expressed in abstract terms what others have worked out in the concrete form; and we think that he would have done well to acknowledge his obligations to the various physiological writers, by whose hints or more formally-expressed doctrines he has obviously profited largely. In the idea that the lower classes of animated being elaborate nutrient materials for the higher, and that in its entrance into the more complex organisms, the food is subjected to a series of elaborating processes by which it is gradually prepared for entering into the composition of the solid textures of the fabric,—ideas which constitute essential parts of his system,—our readers will not find anything very novel. Nevertheless we repeat that the treatise will be read with pleasure by those who feel interested in speculations of this nature; new relations being developed among ideas that were previously familiar, and various suggestions being thrown out which may be profitably pursued.

ART. XIV.

On the Treatment of Ulcers on the Leg without Confinement. With an Inquiry into the best Mode of effecting the permanent Cure of Varicose Veins. By H. T. CHAPMAN, F.R.C.S., &c. &c.—*London*, 1848. 8vo, pp. 156.

THE object of this work is to point out the benefits to be derived from the use of water-dressing in the treatment of ulcers of the leg, mild compression being employed at the same time. The more practical portion is preceded by some sensible observations on the characters peculiar to ulcers on the lower extremity, and the sources of their intractability, the physiological questions being resolved by reference to Mr. Liston's well-known paper "On the Arrangement of the Intermediate Vessels on Surfaces secreting Pus," and to the preparations of injected ulcers by Mr. Quekett, of the College of Surgeons. Passing over these questions, which have been repeatedly discussed in the pages of our predecessors when treating on the subject of inflammation in general, we proceed to give a short account of the treatment advocated by Mr. Chapman.

He divides ulcers into two classes,—those which are intractable from constitutional, and those which are intractable from local, causes. In the remarks upon the first class we find nothing worthy of extract. The following paragraph will explain the views of the author on the treatment of the second:

"Thus, the great majority of ulcers of the leg are of the simply indolent kind, of which the varicose is but an accidental, although perhaps an aggravated, modification, and the callous ulcer the extreme result; the supervention of inflammation will convert the atonic ulcer into an inflammatory, phagedenic, or sloughy sore; and where great excitability of nerve exists, the character of morbid sensibility will be further impressed upon it.

"Assuming, from the premises laid down in a former section, that the chief local sources of intractability in these cases are, either debility, both of the vessels and nerves of the part, or debility of the vessels, combined with extreme morbid sensibility of the nerves, the remedies which have obtained an established reputation for the relief of these conditions will naturally resolve themselves into two classes: first, such as stimulate the feeble contractility of the weak vessels, and, at the same time, excite healthy nervous action; and, secondly, those which subdue excessive sensibility, (morbid or perverted nervous action,) and, at the same time, stimulate contractility.

"The first class of remedies, namely—*Those which stimulate, or afford succour to, the feeble contractility of the vessels, and excite healthy action of the nerves,*—will best fulfil the indications on which the treatment of indolent ulcers is based." (pp. 44-5.)

Some observations follow on rest, horizontal position of the limb, and the use of bandaging, when the circumstances of the patient, or the state of his general health, oblige him to move about, or take air and exercise; with an account of the different kinds of strapping and dressing which have been most generally used. Water-dressing is the subject of an especial section, in which the author purposes to determine the effects of water applied locally, at certain specified grades of temperature. We might complain of an evident want of precision in this inquiry; but give the leading statements without further comment.

"The mode of action of temperature upon the body consists of an abstraction of heat from, or a communication of heat to, any part of the surface; an important influence being thus exercised over its vital functions—in other words, over its sensibility, contractility, and the evolution of animal heat.

"The great power possessed by water of conducting and diffusing heat, enables us to regulate this influence, by its intervention, with more facility and uniformity than could be accomplished by any other medium.

"Fixing, then, upon the two grades of cold and tepid water, I shall proceed to examine the actions on the sensibility and contractility of healthy tissue peculiar to each.

"Action of cold water—under which designation may be included a range of from 40° to 70° of Fahrenheit.

"*On the nerves:* stimulant; augmenting sensibility, and rousing into extraordinary activity those vital functions by which the evolution of animal heat is effected to resist the invasion of external cold, or rather, to supply the loss occasioned by the unusually rapid abstraction of heat. As long as the part retains sufficient vitality to enable it to respond thus to the stimulus of cold, it excites healthy action; when vitality is reduced below this point, its depressive or destructive action commences.

"*On the blood-vessels:* constringent; producing an increased exertion of their contractility.

"In considering the action of cold upon the peripheral circulation, the degree to which the capillaries may be effected by the vital contractility of tissue, particularly of the skin, must not be overlooked.

"On discontinuing or interrupting its application, reaction takes place; the constringent influence of cold being removed, the capillaries relax and admit a larger quantity of blood than in their previous condition, and animal heat is evolved in excess.

"These phenomena are usually evanescent; but the vitality of the part may be so far reduced as to leave it in a state of congestion or chronic inflammation, of which result chilblain is a familiar example.

"The stimulant action of cold upon the nerves may likewise provoke violent reaction in excitable subjects, or under peculiar circumstances, notwithstanding the continued application of a low temperature." (pp. 71-4.)

"Action of tepid water; or water at a temperature of from 80° to about 95°.

"*On the nerves:* sedative throughout its application; allaying undue excitement, maintaining the part as much as possible in a state of ordinary sensibility, and aiding them in keeping up the animal heat where the vitality of the part, or of the constitution, is at a low ebb.

"*On the blood-vessels:* gently constringent, in a degree proportionate to the difference between the temperature of the water and that of the blood circulating in the part.

"Bearing in mind the distinct effects produced by these two grades of temperature upon the nerves and vessels of healthy tissue, and their pathological conditions in the several varieties of ulcer on the leg, cold water obviously ranks with that class of remedies which stimulate feeble contractility, and excite healthy nervous action; tepid water with such as tend to subdue excessive sensibility, the result of actual lesion of nerve." (pp. 76-7.)

Ulcers, which are indolent and irritable from a morbid condition of the capillaries of the ulcer, are to be treated by cold-water dressing and compression; "the grade of temperature calculated to stimulate the contractility of the dilated capillaries without depressing too much the vitality of the part, having been ascertained," that temperature is to be uniformly maintained. The system of irrigation beneath a sheet of oiled silk, so long practised, is, of course, the most effectual means of doing this; but it requires confinement to the bed or sofa. Mr. Chapman generally uses the water-dressing, as most surgeons have done for the last dozen years, covering wet lint with a piece of oiled silk, to favour reaction after renewal of cold applications, by preventing undue reduction of temperature. His plan is described as follows:

"As soon, therefore, as all inflammatory action has been subdued by emollient and antiphlogistic measures, these may be advantageously exchanged for cold-water dressing and support, applied in the following manner. The sore being dressed with a compress of lint dipped in cold water, folded once, twice, or three times, according to the degree of compression which the surgeon may deem suitable to the case, three or more moistened strips of linen or calico, about two inches and a half in width, are to be carried smoothly round the leg. These strips must be applied precisely in the same manner as the strapping recommended by Mr. Baynton; the middle of the first strip being placed upon the back of the leg, with its upper edge opposite to the lower margin of the ulcer, the ends are brought round to the front, drawn firmly, and laid down smoothly one over the other; the second strip must cover the upper third of the first; and the same proceeding must be followed with as many strips as the size of the ulcer may require. Over the strips a calico bandage is applied, the greatest attention being paid to its equable adjustment, so that the compression be distributed evenly over the entire surface, and its amount regulated by the sensibility of the ulcer. Where the leg is slender above the ankle, the roller is apt to fall into plaits, and furrow the skin; to guard against this inconvenience, the hollows immediately above the malleoli should be filled up by compresses of lint. The whole is then to be soaked with cold water, and the moistened bandage enveloped with a sheet of oiled silk, reopening it from time to time, to renew the cold affusion. The wet strips of linen adhere to the limb, even before the application of the bandage, almost as closely as adhesive strapping, and are capable of affording a support, scarcely inferior to that derived from it. In the case of a gentleman, whose legs were extremely bulky, and where they were applied from the toes upward, after the mode of strapping the leg followed by Mr. Scott, the patient was in the habit of removing the roller in the afternoon, and, drawing a silk stocking over the strips of linen, trust solely to the support afforded by them until the following morning. If the ulcer secretes abundantly, it is better at first to repeat the dressing daily, although the lint will absorb much of the discharge; very shortly, however, under the action of cold, large, shining, semi-transparent granulations become compact and red, and a thin and copious secretion diminishes in amount and improves in quality, rendering a daily renewal of the dressings quite unnecessary; and, after a time, this necessity becomes still more rare; in several of the cases hereafter recorded, an interval of three, four, or even five days sometimes elapsed between each dressing, without any interruption to the onward progress of the ulcer.

"The frequency with which the cold affusion is practised must be regulated by the temperature of the part, and the state of the patient's feelings; heat, uneasiness, and irritability being at once relieved by it; the age and temperament of the patient, as well as the season of the year, must also be taken into consideration. In proportion as the ulcer advances towards cicatrization, it is required much less frequently, and in the last stage of the cure may often be altogether dispensed with.

"In the early management of very deep ulcers, I have found it requisite to modify the water-dressing as follows: unless the cavity be filled up to the level of the surrounding skin, and its surface participate in the support given to the rest of the limb, granulations rise very slowly from the depth of the sore; successive layers of lint, according to Mr. Whately's practice, or scraped lint—the *charpie rapée* of French surgeons—were not, I found, well adapted either to absorb the discharge, or to imbibe the water, and convey it freely to the deeper portions of the surface; in fact, such a mass soon became as impenetrable as an unctuous dressing.

"Instead of lint, therefore, in such circumstances, I make use of soft sponge, torn up into very small shreds, and soaked in water; these are dropped lightly into the ulcer, and covered with a single layer of lint, over which the bandage is carried, as in shallow sores; gentle support being thus conveyed to the entire surface, and tone communicated to the minute vessels, granulations spring up uniformly and vigorously, and fill the hollow of the ulcer, often with surprising rapidity, of which Case XVII is an example. The sponge acts, as far as its compressing power is concerned, on the same principle as the wax-dressing poured into deep ulcers, in the manner suggested by Mr. Stafford; according to my experience, however, it is not only a more convenient mode of effecting the same object, but accomplishes it more speedily and completely. The shreds of sponge should be well soaked, and lightly distributed, in order to avoid any ill consequences from too much pressure by their subsequent expansion.

"Under this simple plan of treatment, I am satisfied that the granulation and cicatrization, in a large majority of cases of indolent ulcers of the leg, of long standing, even when attended with a high degree of irritability, will proceed more favorably and expeditiously, and occasion less inconvenience to the patient, than under any other method whatever." (pp. 81-5.)

This extract contains everything that can be considered at all original in this little book; the whole of the remainder consisting of clear and sensible, though rather commonplace, commentaries upon the methods of other surgeons. One of these methods, however, is novel, and has not attracted the attention it deserves. We allude to the employment of galvanism as a means of hastening granulation. We believe that this agent was first employed for this purpose in Germany, and has been for several years adopted in the hospital at Corfu, as may be learned from a paper by Mr. Spencer Wells, which was read before the Medico-Chirurgical Society in January last, having been written in October 1847. This paper will be found in the 'Medical Gazette' of May 26, 1848, and contains the results of numerous observations and experiments made at Corfu. A similar method has been adopted at Guy's Hospital, as we find from the following extract from a lecture by Mr. Bransby Cooper:

"At a convenient distance from the indolent ulcer, (not less than five or six inches,) a portion of cuticle is raised from the cutis by the application of a piece of emplast. lyttæ, of the size of a half-crown. The cuticle is detached by a pair of scissors, and on the exposed cutis a piece of zinc-foil is placed, of the same size as the denuded spot. A plate of silver-foil is laid on the original ulcer, and the two plates connected by means of a thin copper wire. The size of the silver plate is immaterial. Both zinc and silver are now covered with pieces of moistened lint and oilskin, and the apparatus, as recommended by Dr. Bird, is complete.

"In a few hours the surface beneath the zinc becomes white, and a slough begins to form, which in a short time is thrown off, leaving a healthy ulcer behind. In the meanwhile a great change has taken place in the original ulcer; it has thrown aside its indolence; granulations are sprouting and contracting; new skin is forming at the margin, and the whole surface looks healthy and animated.

"The rationale of the action of this contrivance is simple enough; there occurs a decomposition of the water and chloride of sodium naturally existing in the blood and tissues; the result of this is the formation of soda (oxide of sodium), and the evolution of hydrogen and chlorine; the former escapes at the silver plate, while the latter mingles with the zinc to form chloride of zinc, which produces the above speedy escharotic effect." (pp. 91-2.)

Instead of the foil, Mr. Wells uses two small plates of the metals connected together by a wire soldered to each, so that the little apparatus is always ready for immediate application. In the plan he described, also, vesication by cantharides was conveniently replaced by a small puncture or abrasion of the cuticle; and we have been informed that he has latterly found the cuticle become a sufficiently good conductor, when it is simply moistened with a little vinegar, the inconvenience of the eschar produced by the zinc being thus avoided, while all the good effects are as certainly obtained. Mr. Wells advocates the method, as affording the most regular and uniform action of galvanism, and a much better imitation of the natural currents of vital electricity than any battery, the action of which is powerful, and can only be temporarily employed. We have ourselves been astonished at the rapidity with which indolent excavated ulcers assume a healthy surface, and become filled up by fluid granulations, under its use; and we hope that practical men will not neglect a field of observation, in which the labours of Matteucci may be turned to such valuable account.

After some remarks upon tepid water-dressing, the bandage, and tepid affusion, in the treatment of ulcers in a state of morbid sensibility, and also in persons whose "general vitality is low," we have some remarks on the treatment of varix. Mr. Chapman merely gives a very short account of some of the methods which have been proposed to obliterate varicose veins, appearing to lean to the treatment by caustic potash, or the adoption of galvano-puncture, the mode of producing a solid obstructing clot so much practised of late years in Italy. As Mr. Chapman says nothing on this subject from his own observation, we may add our own opinion—an opinion in which we believe most experienced men will agree—that all operations upon veins are attended with danger; that their result is extremely uncertain, a permanent cure very seldom being obtained; and that where simple palliative means relieve all the real evils of varix, as they will in nine cases out of ten, any operation for radical cure is altogether uncalled for, and unjustifiable. We were formerly connected with a northern institution, in which forty cases of varicose veins were successively treated by caustic potash with safety, and more or less success. There followed two deaths from phlebitis, and the plan was very properly abandoned. So much for Mr. Chapman's argument, that the patient is thus not exposed "to the risk of phlebitis," because M. Bonnet was successful in fourteen cases. We happen to know that M. Bonnet himself has lost cases from phlebitis, although, with a feeling not altogether peculiar to French surgeons, he has not brought these cases so prominently forward as his successful ones. Simple pressure by a truss will cure a varicocele as well, and far more safely, than any other operative proceeding; and all the advantages to be obtained from obliteration of the veins of the leg may be gained by the equable pressure of a laced stocking or elastic bandage. Even if obliteration of some veins be obtained by compression or otherwise, others are very apt to become varicose; and we are convinced, from a tolerably extensive experience, that the less veins are operated on, the safer will it be for the patient, and for the reputation of the surgeon.

We have now only to say, that while we consider Mr. Chapman's book almost destitute of novelty, it is clear, well written, and likely to be useful.

Art. XV.

1. *A Practical Treatise of Chemical Analysis, including Tables for Calculations in Analysis.* By H. Rose. Translated from the French and Fourth German Edition, with Notes and Additions, by A. Normandy. In Two Vols. Vol. I. *Qualitative.* Vol. II. *Quantitative.*—*London,* 1849. 8vo, pp. 887.
2. *Practical Introduction to H. Rose's Treatise on Chemical Analysis, illustrated by Synoptic Tables and numerous Formulas.* By A. Normandy. —*London,* 1849. 8vo. Paragraphs 742.

Nineteen years have elapsed since the first edition of Rose's Analytical Chemistry made its appearance in Germany, and eighteen since it was translated into our language by Mr. Griffin; and, notwithstanding that the original work was held in the very highest estimation on the Continent, that M. Rose occupied a place among the first of European chemists, and that there was not such another treatise in this country at the time of its publication, yet it is remarkable that the translation of it by Mr. Griffin fell almost dead-born from the press, so little was it sought after here, and so few were the cultivators of the science of which it treated. But no sooner did the powers of chemistry begin to manifest themselves, and the utilitarian to perceive that they might be advantageously applied to almost every branch of our industrial occupations, than the science assumed a new aspect; and now, instead of having but one, and that an almost unconsulted treatise upon its practical pursuit, we have the works of Faraday, Rose, Griffin, Liebig, Parnell, Normandy, Mitchell, Wills, Noad, and others; and still the wants of all inquirers are not completely satisfied. We cannot, for example, point to any treatise which can be said to meet the necessities of the medical student. He has still to seek for instruction in the practical chemistry of physiology, pathology, therapeutics, and toxicology, from among *all* the works to which we have alluded. And here, although the labours of M. Rose are presented to him in two thick octavo volumes, together with an introductory volume by Mr. Normandy, it can hardly be said that they are very suitable to his particular purpose. In fact, these volumes have been composed to meet the wants of the mineralogist and the assayer, to whom they will be found to be of the utmost value; for they contain the most accurate and complete information on the various processes which are necessary to the detection and quantitative determination of the several constituents of minerals, alloys, and saline combinations. Nor is the information contained in them altogether limited to the analysis of mineral substances; for the second volume of Rose contains ample instructions for the performance of organic analysis, and for the examination of gases and mineral waters, the study of which will, no doubt, be of some service to our sanitary chemists. We observe, also, that M. Rose has given some useful logarithmic tables, for the correction of the changes to which the volumes of gases are liable when they are exposed to different temperatures. These tables, which are founded upon the researches of M. Rudberg, exhibit a relation which is rather different from that which has been hitherto received by chemists, on the authority of Gay-Lussac; for example, Rose's tables show that 1000 volumes of *dry* air at 32° become 1365 at 212°, and not 1375 as Gay-Lussac, Dulong, and Petit state. It must be remarked, however, that the latter number is quite applicable to the expansion of *moist* gases. Again, at the end of the volume, there are numerous tables which have been introduced for the purpose of calculating the proportions of elements in given weights of the several combinations in which they are likely to be recognised. These tables are far more correct, and far more applicable to the purposes of the analytical chemist, than the scales of equivalents or the tables of logarithms which have hitherto been employed. Finally, the work contains an account of the several new elementary and compound substances, with which science has been enriched by Rose and others, since the appearance of the last edition of this treatise. We have no hesitation, therefore, in recommending the work to all who are engaged in the study of practical assaying or mineral chemistry; the present edition having fully sustained the reputation which its predecessors have enjoyed for the last nineteen years.

We ought to remark, also, that Mr. Normandy has accomplished his part of the task, namely, its translation, very creditably, though we are of opinion that it would have been advantageous to the reader if he had adopted the plan of heading each paragraph with a short title, this plan having been found very useful in the old translation by Mr. Griffin. We think, moreover, that there has been sufficient time for the introduction of Rose's new process for the quantitative determination of the alkaline bases, when they exist in combination with the metallic acids, &c. This process is founded upon the property which sal ammoniac possesses, of decomposing many of the metallic oxides, sulphurets, &c. at a high temperature; but as it does not fall within our province to discuss the merits of it, we have only alluded to it, and shall refer our readers to Rose's original papers on the subject, in the 'Chemical Gazette' for 1848, p. 166-411.

The object of Mr. Normandy's Introduction is to explain, as briefly as possible, the rationale of the processes upon which the analysis of compounds is based, to give an epitome of the reactions of general and particular substances on each other, and to refer to the various precautions which are necessary to the proper performance of a chemical analysis; in fact, his object is to furnish a guide to the comprehension of the more elaborate treatise of M. Rose. In the performance of this task he has been, in our opinion, very successful; and we can recommend his treatise as an excellent guide to the student who is entering upon a course of analysis, whether inorganic or organic.

PART SECOND.

Bibliographical Notices.

Art. I.—*Surgical Anatomy.* By Joseph Maclise, Surgeon. Fasciculus I. Folio. Four Plates, with Descriptions.—*London*, 1848.

We are heartily glad to welcome Mr. Maclise back to his own field of labour. Of his claims as a philosophical anatomist, we felt compelled to express an opinion which was far from flattering. As a surgical anatomist he has probably no superior. As a philosophical writer we found him obscure, sometimes almost incomprehensible, labouring to express ideas which probably had a definite form in his own mind, but which he wanted power to convey to others. But substitute the pencil for the pen, the drawing-board for the writing-desk, the real dissection for the abstract archetype; and Mr. Maclise then displays the graphic power of the accomplished artist, united with the truthful exposition of the pains-taking demonstrator.

Mr. Maclise's work bids fair to redeem our country from the stigma of possessing no original work on surgical anatomy, that could take rank in regard to completeness and beauty of execution with many continental productions that might be named. The part now before us contains four plates, each containing two drawings nearly of the size of life. The subjects illustrated are the surgical anatomy of the thoracic cavity, and of the neck and face; the dissections of which are so carefully made and truthfully depicted, as to serve to those who have practically familiarised themselves with these regions (which every student ought to do) as a safe and convenient *refresher*, whenever they find it desirable to renovate their acquaintance with them. We particularly approve of the manner in which the parts are brought into view by a succession of plates; no effort being made to show too much, but those parts only being displayed which are brought into view at one time by a good dissection. Thus, as a guide to the dissector, these plates will be far more valuable than such as endeavour to display the whole anatomy of a complicated region in one or two drawings. The purchaser will be no sufferer by this multiplication of the plates; but, on the contrary, a gainer; for the work is produced at such a marvellously low price, as to bring it within the reach of every one. As we feel assured that nothing but a very extensive sale can make it remunerative, we trust that our readers will show their sense of what is due to the author and publisher, by possessing themselves of it forthwith.

Before parting with Mr. Maclise, we have a word of friendly advice to give him with respect to the literary portion of his production. Nothing can be better than the descriptive portion of his commentaries. But when he tries to edge in a bit of philosophical anatomy, the old leaven displays itself; his language becomes obscure, transcendental, and un-English; and his ideas, if not really confused, have every appearance of being so. Thus in adverting to the fact that the irregularities of arterial distribution in the human subject often correspond with the regular modes of arrangement among the lower animals, he thus expresses himself:

> "It will, however, sound more euphoniously with reason, and at the same time, I believe, be found not altogether unrelated to the useful, if, when such conditions as the 'anomalies of form' present themselves, we can advance an interpretation of the same, in addition to the dry record of them as isolated facts. Comparative anatomy, which alone can furnish these interpretations, will therefore prove to be no alien to the practical, while it may lend explanation to those bizarreries which impede the way of the anthropotomist."

So in the preface, Mr. Maclise cannot satisfy himself with exalting his own subject, but he must run a tilt against every one who does not come up completely to his notion of what constitutes the philosophy of organization. Thus he tells us,—

> "The physiologist of the present day is too little of a comparative anatomist, and far too closely enveloped in the absurd jargon of the anthropotomist, ever to hope to reveal any great truth for science, and dispel the mists which still hang over the phenomena of the nervous system. He is steeped too deeply in the base nomenclature of the antique school, and too indolent to question the import of pons, commissure, island, tænia, nates, testes, cornu, hippocamp, thalamus, vermes, arbor vitæ, respiratory tract, ganglia of increase, and all such phrase of unmeaning sound, ever to be productive of lucid interpretation of the cerebro-spinal ens."

Now we need scarcely tell our readers that the whole character of modern physiological research, as distinguished from that of older date, has been precisely the opposite of that described by Mr. Maclise. The physiologist scarcely ever mentions the greater number of the terms which Mr. Maclise has cited with ridicule, except to define to the mere anthropotomist what are the parts of which he is speaking; and is not in the least hampered by them in his search for those homologies which it is his aim to discover under the guidance of comparative anatomy. For proof of this we need only refer to the writings of Mr. Owen, Mr. Solly, and Dr. Carpenter, with the character of which Mr. Maclise could scarcely have been acquainted when he wrote the paragraph we have cited.—We are somewhat at a loss to comprehend the precise nature of Mr. Maclise's estimate of Microscopy, expressed in the following sentence:

> "Originality has outlived itself; and discovery is a long-forgotten enterprise, except as pursued in the microcosm on the field of the microscope, which, it must be confessed, has drawn forth demonstrations only commensurate in importance with the magnitude of the littleness there seen."

If Mr. Maclise means to say that the general views of life and organization opened out to us by the microscope, to say nothing of the multitude of special results of its application, are of a value proportioned to the littleness of the objects of microscopic research, we must beg to tell him that, if he had made himself acquainted with them, we are sure that he would not speak in such disparaging terms of them: and that it ill becomes a man to talk against the merits of anything which he does not understand. If, on the other hand, this is *not* his meaning, and his estimate is intended to be the very reverse of the interpretation we have put upon it, we would counsel him to express himself more clearly in future.—We trust that in our subsequent notices of this work, we shall not have to comment upon any interpolated blemish that in the least detracts from the merits of the admirable delineations and the excellent descriptions with which Mr. Maclise has presented us.

THE MEDICAL PRACTITIONER'S AND STUDENT'S LIBRARY.

ART. II.—1. *Elements of General Pathology.* By ANDREW STILLÉ, M.D., Lecturer on Pathology and the Practice of Medicine in the Philadelphia Medical Association.—*Philadelphia,* 1848. Small 8vo, pp. 483.

2. *The Principles and Practice of Midwifery.* By DAVID H. TUCKER, M.D., Professor of the Principles and Practice of Medicine, &c. &c. With numerous Illustrations.—*Philadelphia,* 1848. Small 8vo, pp. 405.

THESE volumes are the first of a series recently commenced in Philadelphia, evidently in imitation of Mr. Churchill's 'Manuals', but in a somewhat different form, and at a marvellously low price. The typography and illustrations are, however, very inferior to that to which we are now accustomed in this country; and judging from the two specimens before us, the scientific and literary character of the series will not be above a decent mediocrity.

That an elementary work on General Pathology was a desideratum in English medical literature, we consider that Dr. Stillé is quite justified in affirming. We have ourselves pointed out the deficiency in Dr. Williams's valuable treatise, on which he comments; but we think that in making semeiology and morbid anatomy constitute the chief part of his own production, he has gone into the opposite extreme. The Second Part, embracing the general phenomena, theory, and classification of diseases, is meagre in the extreme; and might have been written twenty years ago, so little indication does it present of an acquaintance with the progress of opinion on these subjects. The author has evidently taken the works of Chomel, and of Hardy and Béhier, as his chief guides; and does not appear to have thought out any one subject for himself. Nevertheless, this treatise will supply the American student with much valuable information in a compendious form; and we trust that it will tend to improve his acquaintance with the science of medicine. We need scarcely say that we fully agree with Dr. Stillé in the following sentiments; and that we shall rejoice in finding them become the general creed of the profession, more especially of those who regulate the qualifications for admission into it.

> "He is convinced that among the most serious defects in medical education are an almost total neglect of logical analysis, and a tendency where generalization is at all encouraged, to speculate on fanciful analogies, rather than to extract truth from facts. How fatal these influences must be to a successful study of disease, and to that confidence in medicine which experience imparts to the philosophical physician, is too apparent to require illustration. The author is persuaded that the view of medical science which it is the province of general pathology to afford, is eminently adapted to correct these evils, by encouraging a more healthful discipline, and infusing a more hopeful spirit, amongst those who are preparing for medical practice." (p. viii.)

The treatise on Midwifery is a compilation from several approved physiological and obstetric works. The author has, upon the whole, executed his task fairly; and the book will be found useful to the student. We are glad to acknowledge the fairness with which the author cites the works and opinions of others; and the principal fault we have to notice in him, is that he has few well-formed opinions of his own. The physiological part of the book contains many of the more recent investigations on the subject of menstruation, conception, and development; although they are rendered very vague and uncertain to the student from the want of illustrations. The author has not worked out these interesting physiological matters for himself; and he does not know some few of the more important modern discoveries. In describing the organic relation which subsists between the maternal and fœtal part of the placenta—he gives long extracts from Wagner's and Dr. Carpenter's Physiology, of the opinions of Weber and Dr. Reid, which the student will find it difficult to comprehend; and he appears to be ignorant of Mr. Goodsir's most valuable researches on this difficult subject, which appear to us to clear up much of its obscurity, and of which he would have found a full account in Dr. Carpenter's last edition.

There is nothing in the practical part of the book which is worthy of notice, beyond the fact that the author has quoted very largely from Dr. Simpson's communications on placenta prævia; and from Dr. Radford's article on galvanism, the whole of which, in fact, is transcribed. There is no glaring deficiency in this section of the treatise, and the precepts are in general sound enough; but the whole wants the life which a thoughtful experience can alone give, and the absence of which is but poorly compensated by a lavish use of the opinions of others.

ART. III.—*Kurt Sprengel's Versuch einer pragmatischen Geschichte der Arzneikunde.* Vierte Auflage, mit Berichtigungen und Zusätsen versehen von Dr. JULIUS ROSENBAUM, &c. &c. Erster Band. *Aelteste Geschichte der Medicin bis zur empirischen Schule.—Leipzig,* 1846. 8vo.

Sprengel's History of Medicine. Fourth Edition, with Corrections and Additions by Dr. JULIUS ROSENBAUM, &c. &c. First Volume, containing the *History of Medicine from the earliest Times to the School of the Empirici.—Leipsic,* 1846.

WE meant to have reserved our notice of this work till it was finished; but, as it seems uncertain how soon this may be, we will no longer delay bringing it before the notice of the English public. Of Sprengel's work itself, it is indeed unnecessary to speak, as both its merits and defects (of which there is no lack,) have been often canvassed. It was Gibbon (if we remember right,) who ventured to prophesy that his great history would be read and abused a hundred years hence; and Sprengel's work (however far inferior to Gibbon's) seems not unlikely to share the same fate, as will perhaps appear from the following sketch of its history since its first appearance. The first German edition was published 1792-9 in 4 vols.; the second 1800-3 in 5 vols.; the third 1821-8 in 5 vols. There are two French translations, one by C. F. Geiger, Paris, 1809-10, in 4 vols., the other by A. J. L. Jourdan, Paris, 1815-20, in 9 vols.; and there is also one Italian translation by R. Arrigoni, Venice, 1812-16, in 11 vols., of which a second edition appeared under the superintendence of F. Freschi, Florence, 1839-42 in 5 vols. 8vo maj. We think it will be admitted that a work of this size, of which, fifty years after it first appearance, a fourth edition is published of the original, and also a second edition of an Italian translation, must either be a book of no ordinary merit in itself, or must at any rate be supplying a desideratum in medical literature, which many authors have attempted to supply, but hitherto without success. We think both of these alternatives are true; it certainly does appear to us to be a work of great (though not of the highest) merit, and we have no hesitation in saying, that, in completeness and excellence combined, no other medical history can be compared to it: so that upon the whole it is still an indispensable work for the student of medical literature and antiquities, and seems likely long to continue such.

But it is not with Sprengel's work itself that we are now concerned, but with Dr. Rosenbaum's new edition of it. The Editor is well known to the literary portion of the medical profession by several historical and bibliographical works, and he had previously shown his respect and admiration for Sprengel by collecting together his 'Opuscula Academica,' (Lips. 1844. 8vo.) and prefixing a biographical account of the author, and a list of his numerous writings: he was therefore one of the fittest persons that could have been selected for the task of editing Sprengel's History of Medicine. It would be manifestly out of place here to enter upon a detailed examination of the work; but we may state generally that the editor appears to have bestowed very great care upon it, and to have executed his laborious task with eminent success. Indeed the amount of time employed in it must have been immense, and we can well believe the editor's statement (p. xiv.) that the preparation of a single sheet not unfrequently took him a whole month.* Accordingly the four parts (or forty sheets) of which this volume consists, appeared at distant intervals (viz. Nov. 1843, Aug. 1844, Sept. 1845, and Oct. 1846), and we are sorry to find that there is now but little probability that the work will (at least at present,) be completed. Dr. Rosenbaum has preserved the author's original text, but has inserted between brackets sundry short additions and notes of interrogation: those passages which cannot be set right by these simple means he occasionally discusses at greater length in his notes. He has verified and corrected Sprengel's references, and has sometimes quoted the words of the original author at full length, whenever there seemed to be any occasion for so doing: he has also added various passages taken from works published since the appearance of Sprengel's History, which serve to illustrate or corroborate the statement in the text. He has prefixed (pp. 10-24) a copious catalogue of works relating to medical biography, literature, and history, (which, by the way, though useful in itself, is perhaps more fitted for a professed bibliographical work, than for a history,) and has added at the end of the volume a chronological table and three good indices. We have not examined the work throughout, but we have referred to several passages in which we believed Sprengel to be mistaken, and we have found almost all of them noticed and corrected by the editor;[*] so that we think we can conscientiously recommend the book to every student of medical history, and the more so as we cannot believe that so valuable a work will eventually be allowed to remain unfinished. With reference to its temporary suspension, Dr. Rosenbaum has since published a little pamphlet, entitled, "Neun Jahre aus dem Leben eines Privatdocenten. Ein Beitrag zur innern Geschichte der medicinischen Fakultät zu Halle." Leipzig, 1847; which, however, contains too much merely personal and private matter to be generally interesting to the English reader.

---

* Perhaps it may be interesting to English literary men to learn that the remuneration which he received for this amount of labour was four thalers (or about twelve shillings) per sheet. (p. x.)

ART. IV.—*American Medical Biography: or Memoirs of Eminent Physicians, embracing principally those who have died since the publication of Dr. Thacher's work on the same Subject.* By STEPHEN W. WILLIAMS, M.D., &c. &c. &c. With Engravings.—*Greenfield, Mass.* 1845. 8vo.

WE ought sooner to have brought this work before our readers; but, as we have not seen it noticed in any contemporary journal, and as we have never met with any copy but our own, we have reason to believe that it is still unknown to many members of the profession in this country. It is intended as a continuation of Dr. Thacher's 'American Medical Biography,' which was published in 1828, and contains an account of more than a hundred medical men, principally those who have died since that date. It consists chiefly of a condensed collection, alphabetically arranged, of such scattered notices of these individuals as have already appeared in different periodicals, whether medical or otherwise; in other cases it contains original communications from the friends of the deceased. Certainly we should be inclined to say that many of the individuals mentioned in this volume were hardly deserving of that honour, and can by no possible extent of charity come under the category of "*eminent* physicians." Our trans-atlantic brethren are perhaps rather inclined to err in this respect, and appear to consider, that, as each man in the United States has an equal right to vote for a Member of Congress, so each has an equal right to have his name preserved from oblivion in a Biographical Dictionary. However, this is only an amiable failing which is perhaps natural and inevitable in a *young* country, and which will be outgrown when the Republic is one or two centuries older.[†]

In point of style the volume has no very great literary merit; but this was perhaps unavoidable in a work compiled from so many different sources. It seems, however, (as far as we are able to form an opinion at a distance from the country to which it relates,) to be respectably edited, and it is certainly useful and interesting, in giving us some idea of the private life and station of medical men in the United States. Many of the persons mentioned in this volume lived in the stirring time of the American Revolution, and became army surgeons; while some relinquished the practice of their profession altogether, took an active part in public affairs, and rose to high civil or military rank. Indeed it may be remarked in general that physicians and surgeons in America seem to take a greater share in political life, than is ordinarily done by the members of our profession in Great Britain. Another point which the inspection of this work brought prominently before us was the amount of fatigue and danger occasionally undergone by medical men in the United States in their night journeys through their pathless forests, which seem far to have surpassed anything of the kind ever experienced in our own, or perhaps any European country. Upon the whole we consider this to be a valuable work, (though perhaps hardly so much so as Dr. Thacher's,) and one which well deserves a place in at least every *public* library in this country.

---

[*] In the French translation of Sprengel's work (tome i. p. 288,) it is stated that there is no *chronological* difficulty in the way of receiving the story of Hippocrates being sent for by Perdiccas II, king of Macedonia, and discovering by certain external symptoms that his sickness was occasioned by his having fallen in love with his father's concubine. M. Littré also finds no difficulty in the dates (*Œuvres d'Hippocr.* tome i. p. 38), nor apparently does Dr. Rosenbaum, as he takes no notice of Sprengel's statement (p. 332.) It has however been examined at length in Smith's *Dict. of Greek and Roman Biography*, vol. ii. p. 483, art. HIPPOCRATES; and also in the third volume of Henschel's *Janus;* and we think that, if Dr. Rosenbaum will look at either of these works, he will probably consider that the dates do form an insurmountable objection against the reception of the story in question.

[†] It is not only among the members of the medical profession, that this feeling appears. In Allen's 'American Biographical and Historical Dictionary' there are numerous articles which are probably far too insignificant to have been admitted into a similar work published in any country in Europe.

It is not, however, entirely on account of the interest of this work in itself that we notice it at present, but also because we think it affords an example which we might do well to follow in this country. Surely, when any one remembers the names of Abernethy, Baillie, Beddoes, Cooper, Gooch, Halford, Heberden, Jenner, Knighton, Liston, &c. &c., he will see at once how very interesting a volume might be made, by giving an account of all the eminent medical men of Great Britain who have died since the beginning of the present century. In those cases where a detailed life has been already published (as for instance, Beddoes, Cooper, Jenner, Knighton, &c.) there are ample materials for the biographer, and nothing but a careful abridgment would be required; in other cases, probably the short biographical sketches that have appeared in our medical journals contain all that is known (or that deserves to be told) of the deceased; while in others again, though no account of the individual has hitherto appeared, it is not yet too late to obtain information from surviving friends and relatives. We throw out this suggestion, not at all in a spirit of rivalry against our American brethren, but in approbation and imitation of them. We heartily hope that some one will consider it worth adopting; in which case, as we have now nearly reached the middle of the century, the present seems to be a favorable time for putting it into execution, and then perhaps the next generation fifty years hence may think the plan worthy of continuation.

Art. V.—*Clinical Midwifery: comprising the Histories of Five Hundred and Forty-five Cases of Difficult, Preternatural, and Complicated Labour. With Commentaries.* By Robert Lee, M.D., F.R.S., &c. Second Edition.—*London*, 1848. Fcap. 8vo, pp. 228.

The value of this little work has already been sufficiently recognised by the profession, to render it unnecessary for us to do more than announce the publication of a second edition, containing the histories of 145 additional cases of difficult, preternatural, and complicated labour, which have occurred in the author's practice during the last five years.

ART. VI.—1. *The Student's Clinical Memorandum Book, or Medical Practitioner's Remembrancer, and Vade-Mecum.—London,* 1848. Oblong 8vo.

2. *The Clinical Repertory, Synoptical Tabular Form.—London,* 1848. Double folio.

WE are glad to welcome any attempts to facilitate and encourage the practice of recording cases, on the part both of the student and the practitioner, and we therefore take the first opportunity of noticing these productions. The first is intended as a portable Case-book, to be carried in the pocket, and filled up on a tabular system. The second is a large sheet, better adapted to be kept at home, and capable of receiving, under its several heads, more than most busy practitioners will have time to record in it. Neither seems to us likely to be as useful as the Case-book noticed by us in our last Number. We are altogether opposed to the system of minute classification, in making the first record of a case; for this ought to present, day by day, a faithful portrait of the patient's condition, and is very likely to be curtailed as to what is really most important, by the restriction of the entries to specified formulæ. The tabular arrangement can only be employed with advantage in working-up the results, for the purpose of drawing from them any general conclusions. Nevertheless, each of these records has its points of value, and we would particularly recommend the large sheet as well adapted for use in hospitals, where economy of space is less an object, and where every case ought to be recorded in a manner as convenient as possible for reference.

ART. VII.—1. *School Chemistry; or Practical Rudiments of the Science.* By ROBERT DUNDAS THOMSON, M.D., Lecturer on Chemistry in the University of Glasgow, &c. &c. With numerous Wood-engravings.—*London,* 1848. Fcap. 8vo, pp. 232.

2. *An Introduction to Practical Chemistry, including Analysis.* By JOHN E. BOWMAN, Demonstrator of Chemistry in King's College, London. With numerous Wood-engravings.—*London,* 1848. Fcap. 8vo, pp. 280.

THE two works before us, although very similar in purpose, size, and general aspect, are very different in their characters. They are both intended to familiarise the beginner with the elementary truths of the science, in such a manner as to prepare him for higher attainments; and both agree in making it a primary object to communicate this knowledge chiefly by a consecutive series of experimental illustrations, which the student should work out for himself. Here, however, the resemblance ends; and we must notice each treatise separately.

After a couple of Introductory Chapters, treating of the "Mode of determining the physical or external characters of bodies," and "Of the internal or chemical properties of bodies," Dr. Thomson plunges at once into all the mysteries of the science; and the remainder of the book seems to us very little else than an abridgment of any ordinary system of chemistry, made, we are sorry to say, with marvellously little consideration for the capacities and wants of the learner, and with a slovenliness of diction which is peculiarly objectionable in a chemical treatise,—both because it tends to confuse the learner's scientific ideas, and also because it directly antagonises the intellectual benefit for which we especially value the study as a means of mental discipline. Examples which we might cite in justification of our criticism occur in almost every page of the book. Thus if we take Chapter II, in which the student is initiated into the mysteries of testing by the blow-pipe and by liquid reagents, instead of a few well-devised and simple experiments, that should give to the learner a notion of what *testing* means, we have little else than a set of tables, including the rarest elementary bodies along with those of most frequent occurrence, and a dry catalogue of unconnected rules and results, with scarcely any attempt to explain their rationale. We defy any student having no previous knowledge of chemistry, to obtain half a dozen definite ideas from the perusal of this chapter. In the second page of it we are told that "to reduce an oxide to the state of metal," is "to cause the oxygen of the oxide to combine with the flame!" With similar inappropriateness of language we are afterwards told (p. 31.) that "when we burn a candle in common air, it is oxygen which combines with the tallow, and contributes to give out light and heat." We do not recollect ever to have heard either of *oxide of flame*, or of *oxide of tallow*. It is true that Dr. Thomson does not directly employ these terms; but they are the legitimate expressions of his accounts of the processes in question. Further on (p. 42) we actually find the process of bleaching by chlorine thus explained;—Cl, HO, and colour become H Cl + *oxide of colour!*" In the succeeding pages we find described among the compounds of chlorine, *hypochlorous* or *protochlorous* acid; *hypochloric* or *quaterchlorous* acid; *perchloric* or *septachloric* acid; *chlorochloric*, and *chloroperchloric* acids. So, again, under the head of cyanogen, Dr. Thomson is not satisfied with bringing in *mellon*, *melam*, *melamine*, *ammeline*, *ammelide*, &c., but we must have also *hydrosulphomellonic* acid, *mycomelinic* acid, &c. &c., compounds whose very names are enough to repel the beginner, and of which the introduction is utterly useless to him. We could go on thus for hours, citing similar instances of inaccuracy and irrelevancy; but this would only be a tax upon our readers' patience and our own.

We regard Mr. Bowman's treatise as far superior, both in plan and execution, to the preceding, and as realizing, in fact, almost everything to be desired in an Introduction to Practical Chemistry. It is so arranged as gradually to lead the student from what is simple to what is complex, and to put him in possession of all the instructions requisite to enable him to educe both facts and principles from his own operations,—a method of teaching for which chemistry is peculiarly adapted. We cannot but think, however, that, to most beginners, the way would have been smoothed by a preliminary chapter, containing some general views as to the nature of chemical action, and the substances of which the great mass of organized structures and inorganic bodies is composed; after which the introduction, in which the use of symbols is explained, would have very appropriately followed. The facilities derived from the employment of these symbols might then have been at once rendered obvious; and some leading ideas would have been gained, which would have served the learner in the arrangement of his facts as he successively acquired them. Taking it as it is, however, the treatise is by far the best adapted for the chemical student, of any that has yet fallen in our way.

ART. VIII.—*The Periodoscope: with its Application to Obstetric Calculations, and the Periodicities of the Sex.* By W. TYLER SMITH, M.B. Lond., Obstetric Lecturer in the Hunterian School of Medicine.—*London*, 1848. 8vo, pp. 48.

THE Periodoscope consists of a disc of card, the edge of which is so graduated as to form a sort of circular Calendar, marked with the divisions of the months and days of the month. This disc is attached at its centre, but in such a manner as to rotate with facility, to a larger card; on which is a circle somewhat larger than the disc, the inner part of which is graduated so as to mark the lunar months, whilst on the outer part of the circle is marked at 0 the word *conception,*—at 1, 2, and 3, the word *abortion,*—at 4, *quickening* and *induction of miscarriage,*—at 5, *induction of miscarriage,*—at 6, *hemorrhage in placenta prævia,* and *induction of miscarriage,*—at 7, *fœtus viable,* with *hemorrhage in placenta prævia,* and *induction of premature labour,*—at 8 and 9, the two latter titles repeated, —and at 10, *parturition.* By setting the disc to the date of conception, the date of parturition will be at once made out, as will also the dates of those various accidents of pregnancy, which, in the author's opinion, are connected with the periodical excitement of the sexual system; and, consequently, the times when the patient should most carefully guard against these, as well as the dates at which the induction of premature labour may be practised with the greatest facility and probability of success. The table also serves to indicate the periods of recurrence of the catamenia in the non-pregnant state; for the disc being turned so as to correspond with the date of any one period, all the rest are shown that may be expected to recur within the whole year.

Our readers, if they have understood our description, will be able to judge for themselves as to the utility of this instrument; which is obviously capable of saving a great deal of troublesome calculation to those largely concerned in obstetric practice. It cannot afford any increased certainty, however, to such calculations; since the real difficulty is to get at the exact data on which they are to be founded. Thus, if the date of conception be not accurately known, which it very seldom is, the whole succession of periods, on which Dr. T. Smith lays such particular stress, is thereby rendered equally uncertain. We cannot understand why Dr. T. Smith should have thought it necessary to divide his disc into 364 instead of 365 parts, giving to December only 30 days, and thus requiring the addition of a day whenever that month enters into the computation. He tells us (p. 14) that to have done otherwise "would have vitiated the whole instrument;" but it appears to us that the difficulty would have been very easily overcome.

We abstain, at the present time, from entering upon the discussion of sundry novel doctrines advanced by Dr. T. Smith, in the letter-press attached to the Periodoscope, and more fully developed in his recently-published Lectures; since he announces a formal treatise 'on Parturition and the Principles and Practice of Obstetrics,' the publication of which will afford us a better opportunity of doing so. All we shall at present remark is, that the applicability of the Periodoscope to the wants of the obstetrician is by no means dependent upon the correctness of the author's theories.

ART. IX.—*Chemical Technology; or Chemistry applied to the Arts and Manufactures.* By Dr. F. KNAPP, Professor at the University of Giessen. Edited, with numerous Notes and Additions, by Dr. EDMUND RONALDS, Lecturer on Chemistry at the Middlesex Hospital, and Dr. THOMAS RICHARDSON, of Newcastle-on-Tyne. Vol. II. Illustrated with 214 Wood-engravings, and one Steel-plate.—*London*, 1848. 8vo, pp. 492.

WE had the pleasure of noticing in a former Number the first volume of this excellent work, and of expressing our high sense of its value. We need say little more, therefore, of its continuation, than that it fully sustains the character of its predecessor, both in regard to the value of the original treatise, and the number and importance of the additions which have been made to it by the English editors. The subjects treated in it are the manufactures of glass of all kinds; of alum and vitriol; of clay-wares, pottery, and porcelain; of bricks and tiles; together with the various applications of lime and baryta, especially in cements. On each of these subjects a large quantity of information is brought together respecting the processes in use on the Continent, which cannot but be useful to our manufacturers at home; whilst in every department in which English science or operative skill has kept us in advance, the editors have amply supplied the deficiencies of the original. We are glad to learn that Dr. Knapp is engaged in preparing a volume on Articles of Food; and that his editors are about to produce an independent work upon Metallurgy.

ART. X.—*Remarks on the Extension of Education at the University of Oxford.* By HENRY WENTWORTH ACLAND, M.D., F.R.S., Lee's Reader in Anatomy, &c. &c.—*Oxford*, 1848. 8vo, pp. 40.

WE are rejoiced to learn that the recent movement in the University of Cambridge will not improbably be followed by a similar extension of the plan of education at Oxford. Among other indications that the matter is undergoing grave consideration on the part of those with whom it rests to regulate the studies of the University, we have great pleasure in welcoming this fearless and judicious pamphlet of Dr. Acland's. The two points which he desires to press on the earnest attention of all who have any influence on the matter, are:

"1st. The duty of introducing the elements of certain branches of Natural Knowledge into the list of studies necessary for all persons taking the degree of Bachelor of Arts.

"2d. The necessity of a reconsideration of our responsibilities and prospects, as a University having the privilege of granting degrees in Medicine, and giving license to practise."

On the first point, we suppose that our readers need no argument to convince them. Dr. Acland's remarks, however, are addressed to the learned Grecians of Oxford, many of whom look upon science of all kinds as an abomination; and the first portion of them is directed entirely to the attempt to prove, that there is a mental discipline to be attained from the study of natural knowledge, which the most diligent classical training cannot of itself impart; and that such study has a practical value in almost every walk of after-life. Moreover, he urges that as the University has accepted, and now possesses, foundations for the furtherance of knowledge, in anatomy, botany, chemistry, natural philosophy, &c., it is of considerable importance to its character to show that it makes *some* reasonable use of these foundations. And he proposes that each candidate for the B. A. degree should be required to have attended three courses, of twenty-four lectures each, on natural philosophy, chemistry, and physiology respectively; and to produce a certificate of having passed an examination on each of these subjects, that shall be satisfactory to the lecturer.

The second point urged by Dr. Acland is that which more immediately concerns the medical profession. Low as was our estimate of the importance of Oxford as a medical school, we were not prepared to learn that, for the last twenty years, Oxford has not granted on the average more than two degrees of M.D. in a year: and that there are not, at the present time, more than thirty-five persons on the books of all the different Colleges and Halls in Oxford, with the degree of M.D., nor fifteen with that of M.B. The University not merely possesses the power of conferring medical degrees, but also that of granting a license to practise out of the metropolis. Now, Dr. Acland suggests that, looking to the present aspect of the Medical Reform question, and to the capabilities of the University and city of Oxford for sustaining a really efficient medical school, all idea of their giving a complete medical education should be abandoned, and that (as a consequence) the power of conferring the license to practise should be voluntarily relinquished. On the other hand, he is for making efficient use of the means already existing, for giving the theoretical part of a physician's education; and would have the Oxford student, after taking his degree in arts, to study anatomy, physiology, comparative anatomy, chemistry, botany, the principles of pathology, and hygiène, in which he should be examined by the board constituted by the University, and for proficiency in which he should receive the M.B. degree. The completion of the medical education must then be sought in some of the great metropolitan schools of practical medicine and surgery, after which the license to practise would be sought from the College of Physicians, and the M.D. degree from the University. Our only objection to this plan is, that by conferring the M.B. degree upon a student who had only passed an examination in theoretical subjects—pretty much equivalent to the *first* examination for the degree of M.B. in the University of London, and for that of M.D. in the University of Edinburgh—a wrong value would be placed upon that degree, the possession of which is at present held to indicate that a *complete* medical education has been obtained. We do not see the use of conferring any degree whatever at this stage in the student's career; but if a title be given, it should not be one which may delude the public.

If all our medical reformers were as liberal and unselfish, and as free from class interests, as Dr. Acland, there would be little difficulty in framing a scheme for the common advantage of the profession and the public.

Before quitting the subject, we may add that we have reason to believe that we have undervalued the medical examinations at Oxford and Cambridge (Vol. II, p. 315), the latter especially, in which that *clinical* examination is in actual use, of which we spoke as a great desideratum in our metropolitan systems of testing the professional acquirements of candidates.

Art. XI.—*Hand-Book of Physiology.* By William Senhouse Kirkes, M.D. Assisted by James Paget, Lecturer on General Anatomy and Physiology at St. Bartholomew's Hospital.—*Lond.* 1848. 12mo, pp. 706.

This work was originally intended to be an abridgment of Dr. Baly's translation of Müller's 'Elements of Physiology;' but as the author proceeded with his task, he found that the science of physiology had made such rapid progress during the seven years which have elapsed since the publication of that work, that the original plan could only be carried into execution in the Chapters on Motion, Voice and Speech, the Senses, Generation and Development. The primary object the author had in view in writing the present work, was "to give such an account of the facts and generally-admitted principles of physiology, as may be conveniently consulted by any engaged in the study of the science, and, more especially such a one as the student may most advantageously use during his attendance on lectures, and in preparing for examination." The author of this book has, in fulfilling his undertaking, evidently availed himself of all the latest and the best sources of information, and has spared no efforts to render it worthy of public approbation. Though we are of opinion that he has fallen into some errors in matters of detail, has overlooked one of the general doctrines of physiology in his account of the circulation of the blood, has passed over in too cursory a manner the principles of the science, and has not followed the best arrangement in the order in which the several great functions of the body are discussed, yet the work, as a whole, reflects much credit upon him, and leads us to hope that, as he has begun his professional career so auspiciously, he may be enabled, if life and health be granted him, to extend the boundaries of that branch of medical science to which he appears to have, in a special manner, devoted himself. We have been much pleased with the section on the Development of the Blood-corpuscles, and the chapter on Nutrition and Growth, especially the former, which is an abstract of part of the Lectures on the Life of the Blood, delivered by Mr. Paget at the College of Surgeons, in May, 1848. Mr. Paget has arrived at the conclusion that—

> "In all the vertebrata, two sets of red corpuscles are developed at different periods of life: a first set, which exists alone in the blood till lymph and chyle begin to be formed; and a second set, which are formed from the lymph- and chyle-corpuscles, and gradually supersede the first set. The corpuscles of the first set are, in the first instance, part of the embryo-cells, which form the mucous or vegetative layer of the embryos in mammalia and birds, and the whole inner surface of the vitelline membrane in the embryos of fish and reptiles. In the latter class, certain of these cells are laid out in the plan of the future heart and chief blood-vessels before the walls of these organs are yet formed, and before the blood has begun to move." (p. 64.)

These cells, out of which the first set of blood-corpuscles are developed, are large, colourless, and of a spherical form, full of yellowish particles of a substance like fatty matter, and each cell contains a central nucleus. Mr. Paget has observed, as Kölliker, Cramer, and others have done, a process of multiplication in these cells, by bipartition of the central nucleus, each half of which, either by appropriating half the cell, or by developing a cell round itself, becomes the central nucleus of a new cell, differing from the parent-cell, from which it escapes, in little except in being smaller and more generally circular.

The source from which the first set of blood-corpuscles are derived soon fails; and when, in the progress of the development of the embryo, the lymph and chyle begin to be formed and added to the blood, as well as ever afterwards during the life of the individual, the blood-corpuscles are developed out of the lymph- and chyle-corpuscles.

In the account of the circulation of the blood, not only the propulsion of the blood along the arteries, but the relative distribution of the quantity sent to the different organs, are described as being effected entirely by the contractions of the heart, and the actions of the muscular coats of the smaller arteries. At p. 110 it is stated:

> "And thus, while the ventricles of the heart determine the total quantity of blood to be sent onwards at each contraction, and the force of its propulsion, and while the large and merely elastic arteries distribute it and equalise its stream, the smaller arteries, with muscular coats, add to these two purposes that of regulating and determining the proportion of the whole quantity of blood which shall be distributed to each particular part."

How the muscular coat of the smaller arteries could dilate these tubes, and cause an increased flow of blood to pass through an organ, is far beyond our comprehension. That there is a *vis a fronte*, developed by the chemical changes that take place between the blood and the tissues in the processes of nutrition and secretion, drawing the blood into these tissues, aiding the more powerful *vis a tergo* of the heart in the higher animals, and regulating the quantity of blood that flows through each tissue and organ, is, in our opinion, one of those general doctrines in physiology placed beyond dispute. As, however, we had so lately occasion (Vol. II, pp. 144-8) to direct the attention of our readers to this subject, we need not here pursue it further. We shall merely remark that we look upon this general doctrine as one of the most important in physiology; since we believe that all attempts to unravel the phenomena of inflammation, and many other morbid actions in the body, must be based upon a knowledge of the forces which create and regulate this *vis a fronte*, before they can lead to any satisfactory results.

At pp. 288-9, we find it maintained that a certain influence of the nervous system is a *condition essential* to healthy nutrition. Now, we feel satisfied that, if our author will take the trouble to reconsider the evidence he adduces in proof of this statement, he will perceive that it by no means proves that the influence of the nervous system is a *condition essential* to healthy nutrition. For, what is meant by the expression, a *condition essential*, but that, without this influence of the nervous system, healthy nutrition is impossible? The facts, however, adduced, are not at variance with the general doctrine in physiology, that nutrition is a property inherent in each tissue, though it may, especially in the higher animals, be influenced by causes acting through the nervous system. It is absolutely necessary to separate those conditions which are essential, from those which are accidental or accessary, if we wish to avoid falling into serious errors.

We remarked above that our author had also fallen into some errors of detail; but these, considering the nature of the work, are comparatively few. At p. 444, it is stated that Biffi and Morganti, in their experiments upon the effects of irritation of the glosso-pharyngeal nerve within the cranium, observed no contractions of the muscles of the pharynx; whereas, in their paper detailing their experiments, contained in Müller's Archives for 1847 (vide Vol. I, p. 268 of this Journal), they describe so many muscles of the pharynx and palate as being thrown into contraction on irritating the glosso-pharyngeal within the cranium, that we suspect that their experiments are not free from error.—We would direct the attention of the author to the section treating of the "Effects of the suspension and arrest of the circulation," as it appears to us confused and unsatisfactory, arising, in a great measure without doubt, from his unwillingness to admit the influence of the chemical changes between the blood and atmospheric air in promoting the circulation through the lungs. Part of it seems to furnish a good example of what is termed reasoning in a circle. The obstructed passage of the blood through the lungs, is, he says, dependent on the blood being charged with carbonic acid and refusing to pass freely through the capillaries; yet the experiments of Mr. Erichsen show that the dark blood continues to pass freely through the lungs and back again by the pulmonary vein, when the heart's action is unimpaired; "therefore the fatal result is due, in some measure, to the weakened action of the right side of the heart, in consequence, probably, of its over-distension by blood continually flowing into it, which it is not able to discharge quickly through the lungs." In other words, the stagnation of blood in the lungs takes place because the heart's action is weakened, and the heart's action is weakened because the stagnation of blood in the lungs has taken place.

These are the chief points on which we differ from the author; and we shall only add, that the work is particularly adapted to those who desire to possess a concise digest of the *facts* of human physiology, without troubling themselves too much about its *principles*, or seeking for their elucidation in the variations of the plan on which the functions are performed in other animals.

Art. XII.—*Elements of Chemistry, including the Applications of Science in the Arts.* By Thomas Graham, F.R.S., V.P.R.S., Corresponding Member of the Institute of France, Professor of Chemistry in University College, London, &c. &c. Second Edition, entirely revised and greatly enlarged; with additional Wood-engravings. Parts I, II, III.—8vo, pp. 544.

The position which Professor Graham now enjoys in the estimation of all scientific men, as one of the most philosophical and learned chemists of the day, renders it unnecessary for us to do more than announce the issue of a portion of the long-promised new edition of his 'Elements of Chemistry.' That the rapid advance of chemistry as a science, and the extension of its application in the arts, should render great additions requisite, was quite to be expected; and a comparison of the parts before us with the corresponding portions of the former edition, has fully satisfied us that the author has spared no pains in introducing whatever novelties he thought deserving of special notice. When the work is complete, we shall examine its leading features in more detail.

ART. XIII.—***Wonders displayed by the Human Body in its Endurance of Injury.*** From the Portfolio of DELTA. With Etchings by the Author. —*London*, 1848. 8vo, pp. 40.

THE title of this pamphlet sufficiently explains the nature of its contents, which consist of a collection of curious cases in which complete recovery has taken place, or life has been prolonged for a considerable time, after injuries which might have been expected to be immediately fatal. A perusal of it may not be without its use in impressing the reader with an idea of the conservative and reparative powers of the system; but as few references are given to the authorities from which the materials are derived, and as several very questionable cases are mixed up with those undoubtedly genuine, and this without the least hint of any difference in the trustworthiness of the several narratives, we cannot regard this production as having any scientific value.

ART. XIV.—*A Dictionary of Medical Science; containing a concise Explanation of the various Subjects and Terms; with the French and other Synonymes; Notices of Climate, and of celebrated Mineral Waters; Formulæ for various Officinal and Empirical Preparations, &c.* By ROBLEY DUNGLISON, M.D., Professor of the Institutes of Medicine, &c., in Jefferson Medical College, Philadelphia. Seventh Edition, carefully revised and greatly enlarged.—*Philadelphia*, 1848. 8vo, pp. 912.

THAT this work, first published in 1833, should have now reached a *seventh* edition, is a sufficient proof of its felt utility in the country which has produced it; and we have much pleasure in calling to it the attention of our readers, as a work equally needed on this side of the Atlantic. Its title, however, does not correctly express its nature, for it is rather a Dictionary of Medical *Terms* than of Medical *Science.* Its purpose is to give a precise definition of every technical term employed in medical literature, so that no reader need be checked in his progress by meeting with a word whose import he does not comprehend; whilst under each of the more important terms the author has endeavoured "to afford a condensed view of its various medical relations." Each successive edition of the work has been greatly augmented, by the incorporation of the various terms of modern coinage, and by the addition of older words that had been previously overlooked; the sixth edition is stated by the author to have been thus enlarged by the introduction of nearly two thousand five hundred subjects and terms not contained in the one immediately preceding; and in the present edition the author has added between *six and seven thousand.* Whence they have been all derived we find it rather difficult to imagine; we can only say that, after looking for every new and strange word we could think of, we have not been disappointed in regard to more than a few of most recent introduction, such as those designations given by Professor Owen to the component elements of a vertebra. But we must also say that we find many of the definitions capable of much improvement. Thus we find *cell* described merely as "a small cavity;" and yet we find, immediately afterwards, as the meaning of *cell-life,* "the life which is possessed by the separate cells that form the tissues, and by which the nutrition of the tissues is presumed to be effected." What can be the *life of small cavities* we cannot comprehend. So again, under *cellular tissue*, we are told that "Anatomists do not agree regarding its intimate structure." Now we do not know that any difference of opinion exists among anatomists worthy of the name, as to the *structure* of this tissue, however they may differ as to its origin. So, again, under *decidua* we find a variety of opinions entertained by Chaussier, Granville, and others, as to its nature; but there is no notice of the recent investigations of Sharpey or Goodsir. We are surprised to find *shaggy chorion* and *spongy chorion* among the synonymes of *decidua;* having always believed the two things to be quite distinct, although it is quite true that the cellular layer of the decidua is continued over the tufts of the chorion. Turning to *placenta,* we find the description equally imperfect. Our friend Dr. Dunglison must remember that the addition of six or seven thousand new terms is by no means sufficient without the revision of the old; and we would recommend him to make this a special object in his next edition. We question if the introduction of the pharmaceutical formulæ contained in all ordinary dispensatories is desirable, since no practitioner is without access to them elsewhere. And there are many terms which are now so completely obsolete, being contained only in the writings of authors which scarce any one reads, that they might be omitted without disadvantage, so as to make room for longer explanations of others of more importance. We make these remarks, not in disparagement of the volume as it at present stands, which is an honorable monument of Dr. Dunglison's learning and industry, but by way of suggestion for its improvement. We believe that the work may be readily obtained in this country; and we are sure that a large proportion of our readers would find it a very useful acquisition.

ART. XV.—*Medical Jurisprudence.* By ALFRED S. TAYLOR, F.R.S., Licentiate of the Royal College of Physicians, and Professor of Medical Jurisprudence and Chemistry in Guy's Hospital. Third Edition.—*London.* 1849. Fcp. 8vo, pp. 849.

HAVING had recent occasion to point out certain defects in Dr. Taylor's work on Toxicology, we have the more pleasure in expressing our hearty coincidence with the general verdict of the two professions, medical and legal, in favour of this admirable treatise; which, like the one just mentioned, although printed in the Manual form, is really the most elaborate work on the subject that our literature possesses, and will unquestionably hold its ground as the standard of medical jurisprudence in this country, so long as it shall be kept by its author so completely up to the mark as it now is. In the present edition, eight new chapters have been added, and a large portion of the volume has been rewritten, the total quantity of matter having been increased by more than a hundred pages. The table of contents also has been augmented; and the index—that most invaluable appendage to a work of this description—has been made much fuller. We have carefully examined the portions of the work in which the chief additions have been made; and we have no hesitation in saying, that they fully bear out the high character of the original treatise, and that the manner in which the new matter is incorporated with the old, gives evidence that the author has spared no pains in rendering his work as perfect as possible.

ART. XVI.—*Portraits of Diseases of the Skin.* By ERASMUS WILSON, F.R.S. Fascic. IV.—*London,* 1848. Folio. Four Plates.

IN announcing the publication of another Part of this truly magnificent work, we can but repeat all that we have said on preceding occasions in its praise. In the combination of fidelity in the representation of the objects they depict, and of artistic beauty of delineation, we are sure that these portraits have never been equalled, and we much doubt if it would be possible to surpass them. The first two plates represent, from the same subject, two varieties of *Lichen syphiliticum,* which is considered by Mr. Wilson as "Depositive inflammation of the derma." We are somewhat perplexed by the account of the case which accompanies the plates; since it would appear from this that the eruption, although "having the character of syphilitic lichen," was not the result of syphilitic contamination, but merely of irritation of the genital passages, occasioned, in a young woman newly married, "by the awkwardness of her husband." The third Plate is a very characteristic portrait of a case of *roseola syphilitica,* which, as Mr. Wilson justly observes, does not differ in any essential points from idiopathic roseola; this affection he considers as "Congestive inflammation of the derma." The fourth Plate is one of the most elaborate specimens of art we have seen. It represents the entire back of a man suffering from *Lichen simplex,* the whole surface being covered with minute pimples. We have calculated that at least *eight thousand* pimples are here separately drawn and coloured, each with such remarkable definiteness of character, that it could scarcely be mistaken for anything else. This will indicate that, in order to render the work what it aims at being, neither pains nor expense are spared by those concerned in its production.

ART. XVII.—*The Natural History of Man; comprising Inquiries into the Modifying Influence of Physical and Moral Agencies on the Different Tribes of the Human Family.* By JAMES COWLES PRICHARD, M.D., F.R.S., M.R.I.A., Corresponding Member of the Institute of France, &c. &c. Third Edition, enlarged, with Fifty coloured and Five plain Illustrations on Steel, and Ninety-seven Wood-engravings.—*London,* 1848. 8vo, pp. 678.

IT gives us much pleasure to learn, from the continued demand for this valuable work, that public attention is now in some degree directed to the subject of Ethnology. Nothing can be more vague and imperfect than the notions that have been prevalent, even among well-informed persons, in regard to the physical and mental characters of the several races of mankind, and the degree in which their mutual relations have been elucidated by the profound researches of modern philologists; and at the present political juncture, the question whether the characters of races are necessarily transmitted from parent to offspring through all time, or whether they are susceptible of modification under the influence of external conditions, is one of vast importance to the welfare of mankind. Of these subjects, a most lucid and comprehensive view is given by Dr. Prichard in the work before us, which is admirably adapted to afford to those, who do not desire to make ethnology an object of special study, a general view of the *results* that have been attained by the most recent investigations, the details of which are contained in Dr. Prichard's larger work.

As a general account of Dr. Prichard's labours was given not long since in the pages of one of our predecessors (Brit. and For. Med. Rev., Vol. XXIV, pp. 49 and 441), we do not intend, on the present occasion, to do more than notice some very interesting additions which have been made to the original treatise (and which may, we believe, be obtained separately by those who possess this), chiefly relating to the ethnography of Southern Africa; respecting which a large amount of new information has been obtained from various sources during the last few years. Some of the most important of this information is that which has been derived from the study of the south African languages; distinct proof having been thereby obtained that the diversity of tongues is much less than had been supposed, and that the relationship of the different nations extending over very wide areas is much closer than was imagined,—a result which is in perfect harmony with that which had been attained by similar investigations in Australia and in America. To those who have not attended to the progress of philological science, it is necessary to state that it is in most cases quite possible to distinguish such community of language as arises from intercourse, conquest, &c., from that which is primitive and primordial, growing out of the original development of the several languages. It is doubtless very easy to deny the possibility of this, and to throw overboard every particle of philological evidence as utterly valueless in ethnology when compared with that derived from physical conformation.* But we are confident that no one who has attentively studied and compared *both* kinds of evidence, putting aside all reference to the *consequences* which will result from the preference of either, will hesitate in admitting the superior value of that now afforded by glossology. In the case of the South African nations, we have a remarkable example of physical diversity, so great as to require the admission of *several* distinct originals, if *any two* races be supposed to have descended from distinct stocks; and yet of community of language, evinced in such striking peculiarities of construction as to forbid the idea that it is anything else than original and fundamental. The races thus grouped together extend nearly up to the equator, both on the eastern and western side of the continent.

The information obtained by Dr. Andrew Smith respecting the origin of the Bushmen is of peculiar interest. It has long been known that they are to be regarded as degraded Hottentots; and it has been customary to regard this degradation as one of the results of European colonization. According to Dr. Smith, however, almost all the South African tribes which have made any advances in civilization are surrounded by barbarous hordes, whose abodes are in the wildernesses and fastnesses of mountains and forests, and who constantly recruit their numbers by such fugitives as may have been driven by crime and destitution from their own more honest and thriving communities. Thus the Kafirs have their Bushmen as well as the Hottentots; although it is only the outcasts of the latter who are known to the Cape colonists. Of the difference in the languages of the Hottentots and Bushmen, on which many have relied as a proof of the entire distinctness of the races, Dr. Smith has given a very satisfactory account. He states that many of the bushmen hordes vary their speech designedly, by affecting a singular mode of utterance, and even adopt new words, in order to render their meaning unintelligible to all but the members of their own community. This modified dialect is more or less understood by the population belonging to each Bushman tribe; but not by the Hottentots, or by persons who know only the common language of the race. As Dr. Prichard justly observes, these curious facts are extremely valuable in their bearing on other ethnological questions.

---

* See Ethnological Journal, edited by Luke Burke, No. vi, p. 283.

It seems probable that many of the diversities of human speech may have had their origin in the voluntary adoption of a new jargon by some small separated community; and that many tribes as distinct in their physical and moral characters as the Hottentots and Bushmen now are, may have originally stood in the same relation to each other. Indeed there are many instances in ancient history, of tribes who were probably of kindred origin with their masters, being reduced and kept in a state of vassalage for many generations, and treated with the greatest cruelty, so as at last to exhibit a complete degradation in physical and moral characters. The subject thus elucidated is a most interesting one under every aspect, physiological, moral, and social.

ART. XVIII.—*Pathology of the Human Eye.* By JOHN DALRYMPLE, F.R.C.S. Fasciculus I. Four Plates. Small Folio.—*London*, 1848.

MR. CHURCHILL seems determined to verify the old adage, "It never rains but it pours." As if the two beautiful illustrated works which we have already noticed, were not enough, he now places before us a third no less admirable, the value of which can scarcely be over estimated. With a few honorable exceptions, the representations of the various morbid conditions of the eye already before the public are worse than useless, being not only imperfect and slovenly as works of art, but deceptive to the practitioner. And yet it is obvious that there is no organ in the body so well calculated to afford a series of objects for delineation, which shall be valuable not merely as a guide to the recognition of the various diseases to which it is itself subject, but also as affording the means of following the progress of inflammatory or other morbid operations, of witnessing the effects of remedies, and of observing the mode in which the diseased gives place to the healthy action. Our limited time and space alike prevent us from doing full justice to the merits of this beautiful production on the present occasion; we shall take the first opportunity, however, of recurring to it, and of pointing out in more detail its peculiar features, and shall at present only say that it realizes all that we believe it possible for Art to effect in the imitation of Nature. The fasciculus contains four plates; and each plate includes six figures. The subjects illustrated are, Chronic Ophthalmia, Tinea Ciliaris, Lippitudo, Ectropium, Trichiasis, Entropium, Pterygium, Symblepharon, Anchyloblepharon, Hordeolum, and Tumor Tarsi.

PART THIRD.

Original Reports.

REPORT ON PULMONARY PHTHISIS,

AS OBSERVED AT

THE HOSPITAL FOR CONSUMPTION, BROMPTON.

By Walter Hayle Walshe, M.D.,

PROF. CLIN. MED. UNIV. COLL. LONDON, PHYSICIAN TO THE HOSPITAL FOR CONSUMPTION, ETC.

The following Report has been undertaken in obedience to a desire expressed by the Committee of Management of the Hospital for Consumption, that some statement of the results obtained within its walls should be made public. It embraces, as a first contribution, numerical inquiries into the four following points: *The course of the disease, considered generally, while under observation; its alleged hereditary character; the relative progress of tuberculization in the two lungs; and the laws of hæmoptysis.* These inquiries are prefaced by a few considerations on the sex, age, and social ties of my patients.

SEX, AGE, AND SOCIAL TIES.

§ 1. Of 629 patients, in whom the existence of phthisis was clearly established, 321 were males, 308 females.

Of the 321 males, 130 were in-patients; of the 308 females, 81 were in-patients: the remainder were out-patients.* These proportions, however, give no accurate notion of the relative frequency of application for relief by the two sexes; they simply show how the sexes stand in this Report.

§ 2. The subjoined table shows the ages of 629 patients at the period of observation.

Ages of 629 Phthisical Persons at Time of Observation.

Ages.	Males.	Females.	Totals.
0 and under 10	12	13	25
10 „ 15	12	14	26
15 „ 20	35	47	82
20 „ 25	61	69	130
25 „ 30	63	67	130
30 „ 35	47	32	79
35 „ 40	33	21	54
40 „ 45	20	22	42
45 „ 50	17	19	36
50 „ 55	13	3	16
55 „ 60	6	0	6
60 „ 65	1	1	2
65 „ 70	1	0	1
Totals	321	308	629

It follows from it, that the decennial period "20 and under 30," furnishes

---

* Formerly seen by me when the establishment was at Chelsea; a few of my in-patient records are also derived from that institution.

260 of the entire number, or 41·34 per cent.; the twenty years, comprised between the ages of 15 and 35, supply 421 cases, or 66·93 per cent. of the whole.* The age of 35 once passed, a rapid decrease takes place in the numbers belonging to each quinquennial period. The proportions here exhibited, in all probability, represent pretty accurately the frequency with which the population of this country at large suffers from phthisis at different ages; we must not, however, conclude that these numbers consequently exhibit the relative tendency to the disease at different ages, or, in other words, teach us the *influence of age on its generation.* In order to establish the amount of this influence (for that age does exercise some such influence is indubitable), the number of phthisical persons should be compared with the total number living at each corresponding age; 629 cases are insufficient to justify the serious labour of the comparison.

Up to the 50th year, the accordance of the numbers in the two sexes is most striking; the difference from 50 to 70 (21 males to 4 females) probably depends upon the small number of cases met with during those years of life. Age, then, obviously exercises a paramount influence on the generation of phthisis (though, as admitted, the precise character of this influence does not flow from the table) —an influence superior to that of sex, and of all agencies connected with occupation, social state, &c. The converse of this is true of cancer—wherein sex seems to master age, the frequency of the disease at different ages being very different in the two sexes.

§ 3. Whether the states of *marriage or celibacy* really exercise any influence on the generation of chronic *general* diseases is doubtful, and, at all events (for reasons, many of them easily divined), very difficult of demonstration. In the instance of cancer, I have shown that the *age* of the sufferers seems more closely connected, than their married or single state, with the nature of their disease; and the same is in all probability true of phthisis. But suppose, in regard of phthisis, the question is reversed; and it be inquired what influence the existence of, or tendency to, phthisis exercises on the contraction of marriage ties, we have an inquiry opened of no slight social import, and one which statistical evidence (on a large scale) can alone answer: as a small contribution, the following table of the numbers of married, single, and widowed persons among 629 patients may be offered:

Ages.	Males, 321.			Females, 308.		
	M.	S.	W.	M.	S.	W.
0 and under 10	0	12	0	0	13	0
10 „ 15	0	12	0	0	14	0
15 „ 20	0	35	0	2	45	0
20 „ 25	9	52	0	12	57	0
25 „ 30	28	35	0	27	38	2
30 „ 35	30	14	3	19	13	0
35 „ 40	28	3	2	18	3	0
40 „ 45	16	1	3	13	8	1
45 „ 50	14	3	0	12	1	6
50 „ 55	8	3	2	1	0	2
55 „ 60	5	1	0	0	0	0
60 „ 65	1	0	0	0	0	1
65 „ 70	1	0	0	0	0	0
Totals......	140	171	10	104	192	12

Excluding from consideration all persons under the age of 15, there remain 297 males, and 281 females; and we obtain the following—

---

* Hippocrates fixed from the age of 18 to that of 35 as the most common period for phthisis.

Table, exhibiting the Mean Ages and Per centages of Married, Single, and Widowed *Phthisical* Persons.

	Males, 297.	Females, 281
Mean Age	31·28	29·19
Per cent. Married . .	47·14	37·01
Per cent. Single . .	49·49	58·72
Per cent. Widowed .	3·36	4·27

In itself, however, this table signifies little; to give it any value, we must be able to compare it with a similar one of *non-phthisical* persons. With this view, I have had 578 cases taken from my books at University College Hospital (hence from a population similar in worldly condition, &c. to that furnishing the previous table),—no selection being made except such as is entailed by the exclusion of phthisis:

Ages.	Males, 297.			Females, 281.		
	M.	S.	W.	M.	S.	W.
15 and under 20	0	29	0	0	47	1
20 „ 25	7	33	0	15	54	0
25 „ 30	20	22	2	7	21	0
30 „ 35	20	11	1	11	14	1
35 „ 40	23	7	1	16	6	6
40 „ 45	28	5	2	13	6	7
45 „ 60	48	13	13	16	6	14
60 „ 65	5	0	1	1	1	4
65 „ 70	2	0	0	2	0	6
70 „ 75	2	0	1	1	0	4
75 „ 80	1	0	0	0	0	1
Totals. . .	156	120	21	82	155	44

From these absolute numbers may be calculated the next—

Table, exhibiting the Mean Ages and Per centages of Married, Single, and Widowed *Non-phthisical* Persons.

	Males, 297.	Females, 281.
Mean Age	36·90	30·92
Per cent. Married . .	52·52	29·18
Per cent. Single . .	40·40	55·16
Per cent. Widowed .	7·77	15·66

Hence:

(1.) The mean age of non-phthisical patients was greater than of the phthisical by about 5 years in males, and 21 months in females.

(2.) In both series the ratio of single females was higher than of males.

(3.) The proportion of non-phthisical persons *single* is less than of phthisical persons;—a fact probably depending on the greater mean age of the former, for that there apparently exists a *greater proneness to early marriage among the phthisical than the non-phthisical* would follow from the subjoined—

TABLE, showing the Proportion of the Total Number Married who were under ætat. 35.

	Phthisical.		Non-phthisical.	
	Males.	Females.	Males.	Females.
Per centage of total number married, who were under ætat. 35 . . .	47·86	57·69	30·13	40·24

ON THE COURSE OF THE DISEASE.

§ 1. In respect of the course followed by the disease during the stay of the patients at Brompton,* their cases may be arranged under the following heads:

CLASS I.	CLASS II.	CLASS III.
A. All symptoms removed; physical signs passive.		F. Worse.
B. All symptoms removed more or less completely; physical signs active.	E. Stationary.	G. Seriously worse. H. Sinking.
C. Much improved generally.		
D. Improved.		I. Death.

K. Condition doubtful (either in fact, or from imperfection of records).

In 211 cases (130 males, 81 females) the numbers, belonging to each of these three heads, are as follows:

		A	B	C	D	E	F	G	H	I	K
Both Sexes .	211	9	7	39	56	24	14	1	5	44	12
Males . . .	130	7	6	25	28	16	8	1	2	30	7
Females . .	81	2	1	14	28	8	6	0	3	14	5

Hence:

(1.) In 9 of 211 cases (or 4·26 per cent.) of phthisis, without distinction of stage, or of severity of the primary or secondary morbid changes, a condition of health, as respects all perceptible evidence of disease, was obtained; all *symptoms* had disappeared, and the physical *signs* showed that no active changes existed in the lungs.

(2.) If we take no account of slight activity of some of the physical signs, (such as continuance of cavernous *rhonchus,*) we get a proportion of 7·58 per cent. of cases (16 of 211) in which all symptoms had disappeared,—the physical signs alone indicating the absence of health.

(3.) The ratio of cases of improvement (Class I, 111 of 198)† is 56·06 per 100; that of cases in which the disease was prevented from advancing (Class I and II, 135 of 198)† is 68·19 per 100; that of cases in which the disease advanced in spite of all management (Class III, 64 of 198)† is 32·33 per 100; that of cases of actual death‡ (I, 44 of 211) is 20·85 per 100.

(4.) The small number of patients belonging to the category (G) "seriously worse," is easily accounted for: patients feeling that they grow worse, would either leave the Hospital before such serious change had actually been effected; or remain on, and, dying there, appear in the category I.

---

* Throughout the remainder of this Report, in-patients alone are referred to, and simple emphysematous cases with chronic bronchitis completely excluded.

† The 12 "doubtful" cases are here omitted for obvious reasons.

‡ Some of these patients were actually moribund on admission.

(5.) Of a given mass of patients entering the Hospital in all stages of the disease, and in every variety of general condition—between the actually moribund state and that of but slight constitutional suffering—the number leaving it, on the one hand, *improved* or *unadvanced* was more than double that, on the other hand, leaving it in *a worse state* or *dying within its walls* (the exact ratio is 67·84 : 32·16). If the cases, in which death was actually imminent at the period of admission, were excluded, the result would be very materially more favorable than this.

§ 2. Distinguishing the sexes, the following inferences flow from the above table.

(1.) Complete removal of symptoms was more frequently effected in the male than in the female, in the ratio of 10·00 : 3·69 : (such removal having been effected in 13 of 130 male cases; in 3 of 81 female cases.)

(2.) But, on the other hand, the general results were slightly more favorable in females than in males; thus:

	A, B, C, D, E. Improved or *Statu quo.*	F, G, H, I. Worse or Dead.
123 Males . .	82, or 66·67 per cent.	41, or 33·34 per cent.
76 Females .	53, or 71·05 per cent.	23, or 31·58 per cent.

§ 3. The connexion of the stage of the disease with its course in the two sexes, while under observation, may be gathered from the following table:

		CLASS I and II. A, B, C, D, E. Improvement or *Statu quo.*		CLASS III. F, G, H, I. Worse or Death.
Both Sexes. 199*	1st stage . .	59	1st stage . .	0
	2d do. . .	75	2d do. . .	0
	3d do. . .		3d do. . .	64
Males . . 123*	1st stage . .	35	1st stage . .	0
	2d do. . .	46	2d do. . .	0
	3d do. . .		3d do. . .	41
Females . . 76	1st stage . .	24	1st stage . .	0
	2d do. . .	29	2d do. . .	0
	3d do. . .		3d do. . .	23

Hence it follows, that—

(1.) In Class III (worse or death) every patient had reached the third stage.

(2.) All patients still in the first stage of the disease left the Hospital either improved or having had a *statu quo* kept up, (of the latter the number is only 7 of 59.)

(3.) Nine of 16 patients in the categories A and B ("symptoms removed") had reached either the second or third stages when admitted.

(4.) Of 139 patients admitted in the third stage, 75 (or 53·96 per 100) figure in Classes I and II; 64 (or 46·04 per 100) in Class III: hence improvement is somewhat more probable than the reverse, even in a mass of patients who have reached the third stage on admission.

The general bearing of these four results is assuredly very satisfactory.

§ 4. But the time the disease had existed, rather than the stage it had reached, at the period of the patient's admission, seems likely *à priori* to be an important element in the benefit of a sojourn at the Hospital. The analysis of 175 cases gives the following results in regard of this point.

---

* Stage of one male case not known.

Table, exhibiting the Mean Duration of the Disease, *at the Time of Admission*, in 175 Cases, in connexion with the eventual condition of the Patient at cessation of Observation.

		A	B	C	D	E	F	G	H	I	K
Both Sexes.	No. of Cases .	8	7	33	43	20	13	1	4	37	9
	Mean duration in months .	6·50	29·57	26·66	20·47	28·20	17·00	40·00	22·00	19·27	27·78
Males.	No. of Cases .	7	7	20	19	13	7	1	2	24	4
	Mean duration in months .	7·29	29·57	26·70	19·79	18·75	22·00	40·00	4·50	21·71	40·50
Females.	No. of Cases .	1	0	13	24	7	6	0	2	13	5
	Mean duration in months .	1·00		26·69	21·00	45·86	11·17		39·50	14·77	17·80

Hence:

(1.) The mean duration of the disease at the period of admission, in cases where its suspension was effected (A), had been materially less (6·50 months) than in any other category of cases.

(2.) Setting aside the instances of suspension of the disease (A), I find that in 103 cases belonging to the other categories of "improvement" (B, C, D, E) the mean duration of symptoms previously to admission had been 24·58 months, whereas in 55 cases belonging to the categories "worse" (F, G, H, I) that mean duration had been only 19·31 months. This result gives some idea of the amount to which an original tendency to rapid course will interfere with the occurrence of benefit from a sojourn at the Hospital. In different degrees the same influence prevails in both sexes, as shown in the next table.

	Duration of Symptoms (in months) previous to admission.	
	B, C, D, E.	F, G, H, I.
Males . .	22·69	21·29
Females . .	28·99	16·09

(3.) But suppose that we do not exclude the category of "suspension" cases (A), the number of these is insufficient to modify notably the general result,—the mean duration in the categories A, B, C, D, E (111 cases) being 23·28 months. So that, in a given mass of cases, the chances of favorable influence from sojourn in the Hospital will be greater, in a certain (undetermined) ratio, as the duration of the disease previous to admission has been greater,—in other terms, natural tendency to a slow course is a more important element of success in the treatment of the disease, than the fact of that treatment having been undertaken at an early period. Here is one instance more, in which observation overturns the doctrines of speculative writers.

§ 5. In 186 cases, the mean length of the patients' *stay in hospital* was 70·23 days;* 63·66 days in those of 117 males; 81·38 days in those of 69 females. This mean excess of nearly three weeks in the length of stay of females, perhaps, depends upon the greater importance of the efforts of the male in supporting his family, rather than upon any given amount of change being more rapidly

* By a regulation, passed by the Committee in 1847, a governor's letter only *entitles* its holder to a stay of three months; this period may, at the recommendation of the physician, be extended to four. A new letter is required for any more lengthened stay.

effected in men than in women, or than upon the fact of the former sex being more readily satisfied with a given amount of improvement than the latter.

The length of stay under each head is shown in the following table, in days:

	A	B	C	D	E	F	G	H	I
Both Sexes	76·59	110·80	91·32	86·63	52·29	63·67	42·00	35·50	52·73
Males . .	68·17	142·60	89·01	76·96	25·13	52·00	42·00	38·00	40·37
Females . .	85·00	79·00	93·62	96·29	79·34	75·34		33·00	65·08

The general tenor of the influence deducible from this table is better shown by comparing the cases of improvement, *statu quo*, and worse, thus:

	No. of Cases.	Mean Sojourn in Days.
A, B, C, D—Improvement .	103	91·34
E—*Statu quo*	21	52·29
F, G, H, I—Worse and Death	62	48·35

Hence, then, the length of stay in the most favorable class of cases nearly doubled that in the least favorable. Some of the latter class, of irritable temper, who left the hospital almost at once, because the sudden improvement they had anticipated had not immediately occurred, would in all probability have eventually won places in Class I; but the total *possible* number of such cases is only twelve.

The *statu quo* class of cases remained a medium time in hospital between the two other classes.

Fatal cases, and those rapidly sinking, had a mean stay of 40·08 days; in several of the former the patients had been but 14, 9, 8, 7, 4, and even 2 days within the institution when death occurred,—they were admitted when utterly beyond human aid. The low average stay in these two categories of cases indicates the tendency of the disease, at the period of admission, to rapid course.

The longest time of stay was, among males, 350 days; among females, 223 days. In both these instances marked improvement was effected; the male patient, who had large cavities, on admission, in the left lung, was seized with pericarditis (with effusion reaching to the clavicle) while in hospital, and struggled successfully through this superadded disease.

§ 6. *Connexion of trade or occupation with result of sojourn.* There are several points of view under which the character of the daily occupation of phthisical patients, previous to admission, might be supposed to affect the frequency and degree of benefit obtainable by a stay at Brompton. I find 191 cases (122 males, 69 females) available for this inquiry.

First. Taking as a basis of division of trades, the degree to which their pursuit requires confinement in-doors, we may separate them into (*a*) confined, (*b*) open air, and (*c*) medium; the proportional numbers belonging to each division turn out to be as shown in the next table:

Trade.	Males.	Females.	Both.
Confined .	51	18	69
Open-air .	44	0	44
Medium . .	27	51	78

The absolute amount to which each of these varieties of trade respectively supply the three classes of "improvement," "statu quo," and "worse or death," is shown in the next table:

Trades.	A, B, C, D. Suspension and Improvement.			E. *Statu quo.*			F, G, H, I. Worse or Death.		
	M.	F.	Both.	M.	F.	Both.	M.	F.	Both.
Confined 69	23	8	31	5	3	8	23	7	30
Open-air 44	23	0	23	9	0	9	12	0	12
Medium 78	20	30	50	2	5	7	5	16	21

From these absolute numbers may be calculated the following—

TABLE, exhibiting the Relative per cent. Frequency with which three Varieties of Trades supplied the Classes of "Improvement," "Statu quo," and "Worse," (without distinction of sex.)

Trades.	A, B, C, D.	E.	F, G, H, I.
Confined.	44·93	11·59	43·34
Open-air.	52·28	20·45	27·28
Medium .	64·11	8·98	26·92

Hence the inferences that—

(1.) Improvement was effected about 14 per 100 more frequently in persons following "open-air" and "medium" occupations, than in those whose trades were "confined."

(2.) That *death* or *deterioration* was very closely as frequent as *improvement* in those who followed "confined" trades; whereas the former were not half as frequent as the latter in those who followed "open-air" or "medium" occupations.

(3.) The chances, then, were evidently more favorable for those who had wholly or partially pursued their trades out of, than those who had laboured within, doors. How is this explicable? Are we to suppose that out-of-door labourers, relieved by the hospital from exposure, gain proportionally more than those who have not been so exposed; or that the pursuit of a "confined" trade gives such a character to the disease, that it is less mitigable than when developed in the pursuit of an open-air occupation? Researches upon a very large scale would be necessary to decide this question.

Secondly. Dividing all occupations into those *physically** laborious and non-laborious, I find that the cases arrange themselves thus:

Trades.	Males.	Females.	Both.
Laborious . .	60	41	101
Non-laborious	62	28	90

The absolute numbers furnished by each of these varieties of trades to the three classes of improvement, &c., are as shown beneath:

Trades.	A, B, C, D. Improvement.			E. *Statu quo.*			F, G, H, I. Worse or Death.		
	M.	F.	Both.	M.	F.	Both.	M.	F.	Both.
Laborious . . . 101	33	23	56	10	4	14	17	14	31
Non-laborious . . 90	33	15	48	6	4	10	23	9	32

---

* The reader must bear in mind this qualification *physically;* thus, the occupation of a dressmaker, though so laborious in some points of view, would figure, in this arrangement, in the non-laborious class, because no expenditure of *physical* force is required in its pursuit.

From these absolute numbers may be calculated the following—

TABLE, exhibiting the relative per cent. Frequency with which Laborious and Non-laborious Trades supplied the Classes of "Improvement," "Statu quo," and "Worse," (without distinction of sex.)

Trades.	A, B, C, D.	E.	F, G, H, I.
Laborious. .	54·45	13·86	30·69
Non-laborious	53·34	11·11	35·56

Hence it would follow that the results were not affected by the exercise of a laborious or of a non-laborious trade.

§ 7. *Influence of age on result.* I find 199 cases in which the age of the patient and the result are both noted. The mean age in these 199 cases was 28·73 years: 30·17 years in 123 males; 26·36 years in 76 females. These means are compared with the results in the three great classes of "course of the disease" in the following table:

	All Cases.		A, B, C, D.		E.		F, G, H, I.	
	No. of Cases.	Mean Age.	No. of Cases.	Mean Age.	No. of Cases.	Mean Age.	No. of Cases.	Mean Age.
Both Sexes .	199	28·73	111	28·53	24	27·54	64	29·47
Males . . .	123	30·17	67	30·91	16	29·56	40	29·18
Females . .	76	26·36	44	24·91	8	23·50	24	29·96

The general inference from this table is, that age had little influence upon the result; the mean age in the categories of "worse and death" being scarcely nine months more than the general mean. The two sexes do not precisely agree in regard of this point; the mean age of females in F, G, H, I being below, and of males somewhat above, the general mean.

The mean age in 9 cases of suspension of the disease was 21·12 years; in 45 cases of death, 29·96 years.

§ 8. *Influence of previous place of residence.* It appears fair to imagine that the influence of a sojourn at Brompton would be different upon patients supplied by the provinces and by London and its suburbs; varying views might, however, be taken of the probable nature of the difference. In the subjoined table the absolute shares of 170 cases, belonging to the nine categories of result, are exhibited in connexion with the habitual places of abode.

Habitual Place of Abode.		A.	B.	C.	D.	E.	F.	G.	H.	I.	Totals.
Both Sexes	London* .	4	4	16	14	8	5	0	2	21	74
	Suburbs .	1	2	14	17	5	4	0	1	10	54
	Country .	1	0	6	16	7	3	1	2	6	42
Males†	London* .	4	3	10	5	5	4	0	2	15	48
	Suburbs .	0	2	9	8	1	2	0	0	6	28
	Country .	1	0	4	12	6	1	1	0	3	28
Females	London* .	0	1	6	9	3	1	0	0	6	26
	Suburbs .	1	0	5	9	4	2	0	1	4	26
	Country .	0	0	2	4	1	2	0	2	3	14

* Or some large town,—in one instance, New York.

† One male, "stoker of a steam-vessel," lived at sea, and left "much improved."

The entire number of these 170 patients furnished by London and its suburbs, then, was 128; while 42 came from the country. Crediting the numbers from each of these sources of supply to Classes I and II, on the one hand, and to Class III, on the other, we obtain the following table:

Habitual Abode.	Improvement or *Statu quo.* A, B, C, D, E.	Worse or Death. F, G, H, I.
London or Suburbs	85	43
Country . .	30	12

From this table it may be calculated that 66·41 per cent. of persons from town and its outlets improved, and 33·59 per cent. the contrary; while 71·43 per cent. of those from the country improved, and 28·57 per cent. the contrary. Hence the chances of improvement are about 5 per cent. greater, and of growing worse, 5 per cent. less, in persons coming from the country, than in townspeople. The advantage does not lie on the side that might have been anticipated,—for Brompton would, *à priori*, seem more likely to benefit those coming from the atmosphere of town, than from the purer air of the country.

This difference does not depend upon any excess of the sex, most frequently benefited, having come from the provinces. For, whereas the general chances of females are somewhat more favorable than of males (see p. 229), only about one quarter of the females, and about one third of the males, were inhabitants of the country. Possibly town-atmosphere, habits and conditions of life in some way modify the disease, so as to render it more difficult of management; or, yet more probably, the destitution of the phthisical town-population is greater than of the phthisical country-population.

§ 9. Does *the period of the year*, at which patients happen to be admitted into the hospital, exercise any influence on the resnlt of their stay? I find 202 cases which may be employed in answering this query. In these 202 cases admission occurred in the different months as follows:

COLD MONTHS.			TEMPERATE MONTHS.		
	Males.	Females.		Males.	Females.
October	9	4	April	19	9
November . . .	13	5	May	10	6
December . . .	12	12	June	15	9
January	13	7	July	6	2
February . . .	7	6	August	4	5
March	14	8	September . . .	4	3
	68	42		58	34

Hence 110 patients were admitted during the colder, 92 during the warmer half of the year: this proportion, however, must not be regarded as representing the amount of demand for the shelter of the hospital comparatively in the two seasons; for, as is well known, there are constantly on the books a long list of patients (passed as fit objects for admission) waiting their turns for vacancies.

As far as these cases go, the greatest share of male entries occurs from January to June (namely, 61·91 per 100, 38·09 remaining for the other six months); so likewise of female entries, the ratio being as 59·21 per 100 to 40·79 per 100: winter and its consequences probably!

As regards the results of their stay in the hospital, according to the months of their admission, the following table may be consulted:

Sex.	Total Number of Admissions.	Number of cases in A, B, C, D, E. Improvement or *Statu quo.*	Number of cases in F, G, H, I. Deterioration or Death.
Both Sexes	October to March, 110	72	38
	April to September, 92	65	27
Males .	October to March, 68	43	25
	April to September, 58	41	17
Females .	October to March, 42	29	13
	April to September, 34	24	10

Taking both sexes collectively, the subjoined per centages may be calculated from this table:

	Per centage "Improvement" cases.	Per centage "Deterioration" cases.
October to March	64·45	34·55
April to September	70·65	29·35

But the more favorable influence of the warmer months, doubly exhibited here, is doubtless in harmony with a general law of the disease; and has no special connexion with the habitation of Brompton.

IS PHTHISIS AN HEREDITARY DISEASE?

§ 1. Popular feeling has so long and so absolutely replied to this query in the affirmative, that it may appear an empty labour to submit the question to investigation. But in point of fact (as those versed in the natural history of the disease are well aware) *proof* has never yet been afforded of the justness of the general conviction. The matter is yet open to inquiry; and as a step towards it settlement, I have analysed, in the first place, the family history of 162 phthisical persons.

§ 2. The first general results of this analysis may be thus tabularly expressed:

	87 Males.			75 Females.		
	Phthisical.	Non-phthisical.	Information deficient.*	Phthisical.	Non-phthisical.	Information deficient.*
Father . .	10	60	17	10	52	13
Mother . .	7	65	15	15	51	9
Both Parents	2	51	—	3	41	—

Hence it follows, that:

(1.) Of 162 phthisical patients, 42 (25·94 per 100) sprang from either a phthisical father or mother.†

(2.) Of 87 males, 17 (19·54 per 100), and of 75 females, 25 (33·34 per cent.), were born of a tainted father or mother: females were consequently about 14 per 100 more frequently of tuberculous origin than males.

(3.) Two of 87 males (2·29 per 100), and 3‡ of 75 females (4 per 100), were the offspring of parents, both of them consumptive.

(4.) In the parents of 92 of 162 phthisical persons (57·41 per 100) no evidence of tuberculous disease could be traced.§

(5.) In the parents of 51 of 87 males (58·62 per 100), and of 41 of 75 females

---

* Information altogether wanting, or vague in its character.

† Living or dead at the time of observation of the offspring.

‡ Two of these three females were sisters.

§ Perhaps a very small per centage should be allowed here for cases of latent phthisis among parents.

(54·67 per 100), no such evidence could be traced.* Proposition 4 corroborates proposition 2.

(6.) In males having an hereditary taint, this existed in the father in 11·49 per 100 of the cases; in the mother in 8·05 per 100. In females, the diseased parent was the father in 13·34 per 100 of the cases, the mother in 20 per 100. So that it would follow, a tainted parent is more likely to have children of the same, than of the opposite, sex similarly affected.

Do these propositions suffice in themselves to prove the reality of hereditary influence in the generation of the disease? I think not. They show that of a given generation (*b*), about 26 per 100 come (under certain ascertainable conditions) of a tuberculous parent (generation *a*). But this ratio of 26 per 100 might be, and probably is, no higher than that of the tuberculized portion of the population generally. In other words, it might be predicated of any mass of individuals taken in hospitals (and of whose history nothing is known, the non-phthisical and the phthisical mixed therefore), that about 26 per 100 of the generation from which they sprang were tuberculous. Hence there would appear to be nothing in the mere fact of 26 per 100 of my patients having had a tuberculous parent, to prove that their disease arose under hereditary influence. Hereditary transmission would be rigidly demonstrated, were the 26 per 100 furnished, generation after generation, by linear descendants of an original tuberculized stock; but I need scarcely observe that the existence of such linear transmission has not only not been demonstrated, but not even suspected, to prevail.

§ 3. The question, it occurs to me, may be examined under another aspect. The ages of the tuberculous portions of any two succeeding generations (*a* and *b*) are (it is legitimately to be assumed) the same. Now we learn, first, from a previous table (p. 226) that 23 only of 629 phthisical persons in a given generation (*a*) marry under twenty-five years of age; secondly, in the next generation (*b*), we find, according to the same table, that of 629 phthisical individuals there will be 263 under that age; now, thirdly, 240 of these 263 do not marry,† and hence do not propagate phthisis; yet in the next generation (*c*), the total proportional number of phthisical persons to the population will be just the same as before,—and, consequently, there has sprung up in this generation (*c*) an equal amount of phthisis from a less amount of phthisical parentage. Hence, either phthisical stock has a great tendency to multiply, or much phthisis is, in each generation, non-hereditary. Now the latter alternative is the correct one: no proof has ever been given that such stock is specially prolific.

§ 4. Further, some aid may be obtained from examining the condition of the brothers and sisters of phthisical persons; as far as my records go in the 162 cases referred to, the results may be tabulated thus:

	One or more Brothers, or Sisters, or both.		
	Phthisical.	Non-phthisical.	Information deficient.
87 Males . .	13	43	31
75 Females .	22	33	20

So that 20·98 per 100 of these phthisical patients had one or more brothers or sisters, or both, similarly diseased: among the males the proportion was 14·94, and among the females 29·34, per 100. On the other hand, 46·91 per 100 of the number were free from taint in brother or sister.

§ 5. The degree to which phthisis existed in the past and present generations of the families of persons, themselves phthisical, may be gathered from the following table:

---

* See note §, p. 235.

† A small correction should be made for illegitimate children.

		Brothers, or Sisters, or both.		
		Phthisical.	Non-phthisical.	Information deficient.
Males . . 64*	Father, or Mother, or both phthisical	2	9	4
	Neither Father nor Mother phthisical	6	30	13
Females . 63*	Father, or Mother, or both phthisical	9	7	7
	Neither Father nor Mother phthisical	12	19	9

Hence:

(1.) Of 127 phthisical people, there were 11 (or 8·07 per 100) whose parents and brothers or sisters suffered from the same disease.

(2.) Of the same 127 persons, there were 49 (or 38·58 per cent.) in whose families neither generation appeared affected.

(3.) It is probable that the 8·07 per 100 of such double taint is greater than would be found among non-phthisical persons.

(4.) It is probable that the proportion of these phthisical patients having no taint in either generation (38·58 per 100), is lower than would be found among non-phthisical persons.

(5.) Both these probabilities are in favour of the hereditary character of the disease: the degree to which they are so, can only be determined by a similar analysis of the family histories of non-phthisical patients.

§ 6. Of collateral relatives† I have had but little information; such as it is, it may be presented thus:

	Collateral Relatives.		
	Phthisical.	Non-phthisical.	Information deficient.
Males . . . 87	3	13	71
Females . . 75	13	9	53

So that of 38 phthisical persons, of whose collateral relatives I had any information, there were 16 (or 42·11 per 100) having a collateral taint in their families,—apparently a very high proportion.

§ 7. In estimating the next table, it is to be remembered that the vast proportion of cases in which I had no account of collateral relatives interferes much with its value.

	Father, Mother, Brother, Sister, and Collateral Relatives.	
	All Phthisical.	All Non-phthisical.
87 Males . .	0	9
75 Females .	2‡	4§

§ 8. The general bearing of the facts, now analysed, in favour of hereditary influence would be powerfully supported, were it found that the mean age, at the period of outbreak of the disease, were materially less in those who had, than in those who had not, phthisical progenitors. I have examined the cases in this respect, omitting those in which information is deficient or imperfect, with the following results:

---

* Cases in which information was deficient about *parents*, are omitted in this table.

† Uncles, aunts, cousins.

‡ Two sisters.

§ Two of these patients also sisters.

TABLE, showing the Mean Age, at the Period of Outbreak of the Disease, of Phthisical Persons having, or not having, a Phthisical Father or Mother.

		Phthisical.		Non-phthisical.	
		Father.	Mother.	Father.	Mother.
Males.	No. of cases . . .	10	6	56	62
	Mean age in years . .	25·12	24·60	26·80	27·65
Females.	No. of cases . . .	9	14	48	49
	Mean age in years . .	18·56	21·67	23·48	23·59

Hence in males the malady exhibited itself at a mean period of about two years earlier,—and in females at a mean period of about three years and a half earlier, when there was, than when there was not, a parental taint. Five only of my patients were tainted on both sides (see Table, p. 235),—a number too small to furnish any inference worthy of trust; still it may be as well to mention that the mean age of these five persons, on the outbreak of phthisical symptoms, was eighteen, the lowest mean I have met with in these calculations.

It would appear, then, that where adults are the subjects of observation, the disease is earlier developed when parental taint exists, than when it is absent; and hence it is legitimate to conclude that, were observation extended to phthisical children, evidence of this kind, yet stronger, of hereditary influence would be found.

§ 9. But this inquiry cannot be considered complete in the stage to which I have now brought it. I have simply shown, so far, that phthisical persons spring from a phthisical source with a certain amount of frequency; and that freedom from taint in parentage is *probably* more rare, and the existence of such taint *probably* more common, in phthisical than in non-phthisical persons. To convert probability into certainty, it becomes necessary to examine the parentage of patients themselves non-phthisical; for this end I have had the notes of the parentage of 328 patients, observed by myself, extracted, *without selection*, from my case-books at University College Hospital.* From the list I have cancelled every case in which the slightest suspicion of tuberculous disease existed during life, or the most trifling deposit even of gray granulations was detected after death. The total number of males is 161; of these, as there were 27 either clearly phthisical, or not demonstrably free from its signs, 134 only will figure in the following analysis; of 167 females, the number is, on the same principle, reduced to 150. The first general results from the analysis of these 284 cases are seen in the following—

TABLE, showing the absolute Number of Phthisical Parents furnished by 284 Persons, themselves free from all ascertainable evidence of Phthisis.†

	Males, 134.			Females, 150.		
	Phthisical.	Non-phthisical.	Information deficient.	Phthisical.	Non-phthisical.	Information deficient.
Father . .	7	81	46	21	82	47
Mother . .	9	87	38	10	104	36
Both Parents	1	63	—	5	71	—

---

* A laborious task, carefully performed by my assistant, Mr. William Tidmas.

† The value of this table, as balancing any incorrect results which may have entered into the similar table (p. 235), referring to persons themselves phthisical, is evident.

Hence:

(1.) Of 284 non-phthisical persons, 47 (16·55 per 100) sprang from either a phthisical father or mother.

(2.) Of 284 non-phthisical persons, 134 (47·11 per 100) were free from paternal or maternal taint.

§ 10. Collating these results with those already obtained (p. 235), concerning the parentage of persons themselves phthisical, we obtain the following—

Table, showing the per cent. Frequency of Parental Taint, or its Absence, in the Phthisical and in the Non-phthisical.

	Persons themselves Phthisical.	Persons themselves Non-phthisical.
Father or Mother phthisical	25·94	16·55
Both Father and Mother free from Phthisis . .	57·41	47·11

So that these results, obtained from the analysis of 446 cases, do not depose in favour of the hereditary character of the disease: for if, on the one hand, about 9 per 100 more phthisical, than non-phthisical, persons came of a consumptive father or mother; on the other hand, there were about 10 per 100 more phthisical than non-phthisical persons free from parental taint.

§ 11. The frequency with which phthisis existed in either brothers or sisters, or both, or was absent from these relatives, is shown in the next table.

	Brothers, or Sisters, or both.		
	Phthisical.	Non-phthisical.	Information deficient.
Non-phthisical Males, 134	16	71	47
Non-phthisical Females, 150	19	67	64

From this it appears that 12·32 per 100 (35 of 284) of non-phthisical persons had one or more phthisical brothers or sisters; and 48·52 per cent. of the number (138 of 284) had no relatives of that degree of consanguinity tainted. As the corresponding per centages in the instance of phthisical patients, are (p. 236) 20·98 and 46·91, it follows that these series of facts supply numerical evidence of hereditary influence to a slight amount.

§ 12. The degree to which phthisis existed in the past and present generation of the families of persons themselves non-phthisical, may be gathered from the following table:

		Brother, or Sister, or both.		
		Phthisical.	Non-phthisical.	Information deficient.
Non-phthisical Males, 77*	Father, or Mother, or both, phthisical	3	7	4
	Father and Mother non-phthisical . .	5	45	13
Non-phthisical Females, 94*	Father, or Mother, or both, phthisical	7	10	9
	Father and Mother non-phthisical . .	6	43	19

* Cases in which information was deficient about *parents* are omitted here.

Hence:

(1.) Of 171 non-phthisical persons, there were 10 (5·85 per 100) in whom parental and fraternal taints co-existed.

(2.) Of the same 171 persons, there were 88 (51·46 per 100) in whose families neither of those generations appeared affected.

§ 13. Now, from the collation of the corresponding results obtained from persons themselves phthisical, may be formed the following—

TABLE, exhibiting the per cent. Proportion of Phthisical and Non-phthisical Persons, either having, or free from, Phthisical Taint, in the present and next preceding Generations.

	Father, or Mother, or both, and Brother, or Sister, or both.	
	Phthisical.	Non-phthisical.
Phthisical Persons . . .	8·07	38·58
Non-phthisical Persons	5·85	51·46

These results tell, as I had anticipated (p. 237) they probably would, in favour of hereditary influence. But the degree to which they do so is very slight; phthisical people have the double taint in their families only about 2 per 100 more frequently than the non-phthisical; and the number of the latter positively free from these taints is about 13 per 100 greater than of the former.

§ 13. The final conclusion, then, deducible from this analysis of 446 cases is, that *phthisis in the adult hospital-population of this country is, to a slight amount only, a disease demonstrably derived from parents.* There is no reason to believe that the law differs among the middle and higher classes of society; but to warrant an absolute assertion, we should require an analysis of two series of cases (similar to those I have now adduced) which had been collected among those classes.

ON THE RELATIVE PROGRESS OF TUBERCULIZATION IN THE TWO LUNGS.

§ 1. The analysis of my cases in respect of the degree to which each lung was affected, allows of the construction of the following—

TABLE, exhibiting the Frequency of the three Stages* of the Disease in the two Lungs at Death, or the close of Observation, in 164 Cases.

	94 Males.		70 Females.		164 both Sexes.	
	Right Lung.	Left Lung.	Right Lung.	Left Lung.	Right Lung.	Left Lung.
First stage . . .	25	31	37	28	62	59
Second ditto . .	13	13	11	12	24	25
Third ditto . . .	46	38	15	24	61	62
Mention omitted of stage	2	2	1	0	3	2
No appreciable dis.	0	2	0	0	0	2
Records not sufficiently clear . .	8		6		14	

Hence:

(1.) In 150 cases, the right lung was certainly affected 147 times, the left 146; and the left certainly unaffected (as far as physical evidence went) in 2 cases only;† while similar absolute freedom of the right lung was not established in a single instance. The equality of stage in the two lungs is remarkable, and the result tends to reconcile some of the conflicting statements put forward by various persons on the matter.

---

* First stage of consolidation; second of softening: third of excavation.

† In one of these cases the right lung was only in the first stage; in the other, in the second, in connexion with old right empyema.

(2.) If the sexes be considered apart, however, this equality ceases to be so close. Examples of the first stage are more numerous on the left side in males, on the right in females; while the second and third stages appear in greatest number on the right side in males, on the left in females.

§ 2. The manner in which the stages were associated in the two lungs of both sexes in 143 cases is shown in the next table:

	Right Lung in 1st Stage.		Right Lung in 2d or 3d Stage.		Left Lung in 1st Stage.		Left Lung in 2d or 3d Stage.	
	Left lung. 1st Stage.	Left lung. 2d or 3d Stage.	Left lung. 1st Stage.	Left lung. 2d or 3d Stage.	Right lung. 1st Stage.	Right lung. 2d or 3d Stage.	Right lung. 1st Stage.	Right lung. 2d or 3d Stage.
Males . . .	20	4	11	45	20	11	4	45
Females . .	23	14	5	21	23	5	14	21
Both Sexes .	43	18	16	66	43	16	18	66

Hence:

(*a.*) The *right lung* being still in the first stage in 61 cases, the left had reached the second or third in 18, or in 29·51 per 100 of the number.

(*b.*) The *right lung* having reached the second or third stage in 82 cases, the left still remained in the first in 16; that is, in 19·51 per 100 of the whole.

(*c.*) The *left lung*, being still in the first stage in 59 cases, the right had reached the second or third in 16; that is, in 27·12 per 100.

(*d.*) The *left lung* having reached the second or third stages in 84 cases, the right remained in the first in 18; that is, in 24·43 per 100 of the number.

From these propositions it follows that the disease attains the softening point with not very unequal rapidity in the two lungs. Such difference as exists signifies that the point is more quickly attained in the left than in the right organ; for it appears that softening of the left lung, the right being still firm, was more common (under two distinct points of view) in the ratios of 29·51 to 27·12, and 24·13 to 19·51. I have, however, no means of ascertaining satisfactorily whether this depends on deposition being on an average earlier in the left lung than in the right, while the actual rapidity of subsequent destruction is equal in both; or from the destructive process being more rapidly effected on the left side, deposition being coeval on both sides.

§ 3. But, as will appear from the subjoined comparison, the laws regulating the disease in males and females probably differ in this respect: they certainly did in these 143 cases.

Tabular comparison of the condition of the two lungs (in 143 cases) of both sexes.

MALES.

(*a.*) *Right lung:* 24 in first stage, 4 in which the *left* had reached the second or third stage,—that is, in 16·67 per cent. of the number.

(*b.*) *Right lung:* 56 in second or third stage, 11 in which the *left* still remained in first stage,—that is, in 19·64 per cent. of the number.

FEMALES.

(*a.*) *Right lung:* 37 in first stage, 14 in which the *left* had reached the second or third stage,—that is, in 37·83 per cent. of the number.

(*b.*) *Right lung:* 26 in second or third stage, 5 in which the *left* still remained in first stage,—that is, in 19·27 per cent. of the number.

(*c.*) *Left lung:* 31 in first stage, 11 in which the *right* had reached the second or third,—that is, in 35·49 per cent. of the number.

(*d.*) *Left lung:* 49 in second or third stage, 4 in which the *right* still remained in the first,—that is, in 8·16 per cent. of the number.

(*c.*) *Left lung:* 28 in first stage, 5 in which the *right* had reached the second or third,—that is, in 17·86 per cent. of the number.

(*d.*) *Left lung:* 35 in second or third stage, 14 in which the *right* still remained in the first,—that is, in 40 per cent. of the number.

The propositions marked (*b*) do not tell in one direction or the other; those marked (*a*, *c*, *d*) depose strongly to the greater rapidity of evolution of the disease in the right lung of males, and in the left of females. Let it be remembered, too, that in the only two instances in which either lung was wholly unaffected, it was the *left* lung of *males* that had escaped. Why the disease should advance more quickly on the right side in men, and on the left in women, does not clearly appear: if it be suggested that the greater frequency of pneumonia of the right lung in the male will explain the fact of its becoming a more rapid prey to tuberculization, the difficulty is to explain why the left should be the earlier victim in females.* Besides, does pneumonia, occurring in a tuberculized lung, hasten the destructive course of the tubercular disease? Speculative considerations unhesitatingly reply,—Yes: analysed Observation has hitherto replied,—Either not at all, or in but a very small number of cases.

§ 4. By a comparison of cases in the two sexes in which either lung (both being in the same stage) was clearly more extensively affected with that stage than its fellow, corroboration of the above inferences is obtained:

		Lungs in same Stage.	
		Excess in right.	Excess in left.
Males, 46	First Stage .	10	3
	Second ditto	5	3
	Third ditto .	11	14
Females, 15	First Stage .	5	5
	Second ditto	—	—
	Third ditto .	1	4

Whence it follows that both lungs having reached the second or third stages, excess of disease existed in the male about as frequently in the right as left; but in the female four times as frequently in the left as in the right. The difference is more notable if cases of the first stage be compared.

§ 5. The relative condition at death, or at close of observation, of the two lungs of both sexes, *in connexion with age*, is shown in the following table:

Age.	Males.						Females.					
	Right Lung.			Left Lung.			Right Lung.			Left Lung.		
	1st.	2d.	3d.	1st.	2d.	3d.	1st.	2d.	3d.	1st.	2d.	3d.
10 and under 20	5	3	5	4	1	8	12	1	3	12	4	0
20 „ 30	8	4	20	14	4	14	19	5	9	14	5	15
30 „ 40	5	4	16	6	5	13	3	2	1	0	0	6
40 „ 50	5	1	3	6	2	1	2	1	2	0	2	3
50 „ 60	2	0	1	1	0	1	0	0	0	0	0	0

---

* I am aware it has been said, on loose grounds, that the left lung of females is more frequently inflamed than the right; but, as far as numerical evidence goes, the reverse is the fact.

Bearing in mind the law above established respecting tuberculization of the two lungs in the two sexes, we may infer from this table that *age* somewhat influences that law; for, taking the periods from æt. 10 to 30, and æt. 30 to 50, as comparable periods, we get the following absolute numbers:

Age.	Males.				Females.			
	Right Lung.		Left Lung.		Right Lung.		Left Lung.	
	1st Stage.	2d or 3d Stage.	1st Stage.	2d or 3d Stage.	1st Stage.	2d or 3d Stage.	1st Stage.	2d or 3d Stage.
10 and under 30	13	32	18	27	31	18	26	24
30 " 50	10	24	12	21	5	6	0	11

And from these absolute numbers may be had the following—

Table, exhibiting the per centage of Cases of Tuberculization, in which the Second or Third Stages had been reached at two periods of Life, by either Lung of both Sexes comparatively.

Age.	Males.		Females.	
	Right Lung.	Left Lung.	Right Lung.	Left Lung.
10 and under 30	71·11	60·00	36·73	48·00
30 " 50	70·59	63·64	54·55	100·00

Hence, in males, the excess of frequency with which the *right* lung had reached the second and third stages was within 2 per 100 the same before and after the age of 30; whereas in females, the excess of frequency with which the *left* lung was found in those stages was about 45 per 100 greater after than before the same age.

It will be well to test these results by the next

Table, exhibiting the Mean Age of Males and Females in the Three Stages of the Disease of both Lungs comparatively.

	Males.						Females.					
	Right Lung.			Left Lung.			Right Lung.			Left Lung.		
Stage of Disease	1st	2d	3d	1st	2d	3d	1st	2d	3d	1st	2d	3d
Number of Cases . .	25	10	44	31	12	37	37	9	15	27	11	24
Mean Age	30·96	28·00	30·37	29·77	30·93	27·81	23·22	27·56	26·67	19·63	25·56	30·00

From this it appears that the mean age of males having the right lung softened, was 29·19 years, and the left 29·37; while the mean age of females having the right lung softened was 27·13 years, and the left 27·78. This almost precise similarity of the ages of females in whom the right and the left lung had respectively softened (a similarity at first difficult to reconcile with the dissimilarity previously shown to exist in the number of cases of softening before and after the age of 30 in that sex) depends upon the total absence of examples of left lungs in the first stage after the age of 30,—an absence which, in any comparison made between cases of first stage on the one hand, and of the second and third stages on the other, necessarily raises to 100 per cent. the ratio of cases of softening.

§ 6. We may next consider how far any difference exists in the mean period of the disease at which softening occurs in each lung:

Table, showing the Mean Time of Illness (in months) at Death or close of Observation, in connexion with the Stage of the Disease in both Lungs of both Sexes.

	Males.				Females.			
	Right Lung.		Left Lung.		Right Lung.		Left Lung.	
Stage	1st	2d, 3d	1st	2d, 3d	1st	2d, 3d	1st	2d, 3d
Number of Cases . .	19	45	25	37	28	19	19	29
Duration in Months .	22·00	19·27	16·88	20·05	23·86	24·00	25·68	23·38

Hence it appears that the mean duration of the disease in males whose *right* lung was softened had been less by about a month, than in those whose left lung had undergone that change; whereas in females a difference of about three weeks existed in the converse direction. A comparison of the above mean durations in cases of consolidation and of softening in the two sides, and in the two sexes, still further goes to confirm the inferences already drawn as to the greater rapidity with which the right lung had softened in males, and the left in females.*

ON THE LAWS OF HÆMOPTYSIS.

§ 1. Among 165 recorded cases, no mention is made of hæmoptysis in 34; it is distinctly stated to have been present in 106, absent in 25, cases up to the period of death or of close of observation. Hæmoptysis therefore existed, to a greater or less amount, in 80·92 per cent. of these patients: it seems *probable*, however, that among the 34 cases in which no mention is made of the symptom, there were a greater number of instances of its absence than of its presence.

§ 2. The influence of sex on the mere occurrence of bloody expectoration does not appear very marked. Of 75 males 13 had no hæmoptysis; 62 (or 82·66 per cent.) had suffered from it: of 56 females 12 affirmed they had never spit blood in any form, 44 (or 78·57 per cent.) had been more or less severe sufferers.

§ 3. The amount of blood lost *on any particular occasion* varied widely. Ninety-nine cases, where the amount was noted with sufficient precision, may be arranged thus:

Amount of Hæmoptysis.		54 Males.	45 Females.
Slight. . .	Sputa tinged or streaked with blood†	10	12
	—up to ½ oz.	13	14
Medium. .	½ oz. to 1 oz.	5	5
	1 oz. to 4 oz.	6	3
Profuse . .	4 oz. to 16 oz.	10	11
	16 oz. and upwards . . .	10	0

A glance at this table shows that hæmoptysis to a medium amount is less common than to a very slight or to a profuse amount; the precise degree to which this is the case in the two sexes, is shown in the following table:—

---

* It is to be remembered, that these inferences are not based upon cases in which the moment, when softening commenced, had been ascertained; but upon cases in which that change existed at the close of observation or at death.

† No notice is, of course, taken here of cases in which a quantity of blood, too small to be seen by the naked eye, was present.

Hæmoptysis:	99 Total Cases.		Males, 54.		Females, 45.	
	Abs. No.	Per cent.	Abs. No.	Per cent.	Abs. No.	Per cent.
Slight and profuse .	80	80·80	43	79·63	37	82·22
Medium	19	19·09	11	20·37	8	17·78

It would appear also from these cases that very profuse hemorrhage from the lungs is less common in hæmoptoic females than in males; while 18·18 per cent. of the latter lost from one pint to three or nearly four pints of blood at a time, no example of such profuse discharge occurred among the former.

§ 4. In 105 of 131 cases of hæmoptysis, the frequency, with which the symptom recurred, is noted; the analysis of these cases may be exhibited as follows:

Sex.	No. of Cases.	Hæmoptysis.					
		Absolute Number of each Variety.			Per centage frequency of each Variety.		
		Once.	Twice.	Several times.	Once.	Twice.	Several times.
Males ...	61	21	5	35	34·43	6·91	57·38
Females..	44	23	3	18	51·82	6·82	40·91

From this table, it follows that *medium frequency* of recurrence of hæmoptysis is materially less common (and this in both sexes) than a single, or than repeated, attacks. Further, repetition of hæmoptysis is more common in males than females. Both these results tally precisely with those already set forth in respect of the *amount* of blood lost in attacks of hæmoptysis.

§ 5. It is a point of much practical interest to determine whether recurring hemorrhages, from tuberculization of the lungs, are disposed to increase or to decrease in violence with repetition: what I have been able to ascertain by the analysis of my cases on this matter appears in the following table:

Successive Hæmoptyses.	Males, 20.	Females, 17.	Total, 37.
Not materially unequal in amount	8	3	11, or 29·73 per cent.
First greater than subsequent	10	11	21, or 56·76 per cent.
Subsequent greater than first	2	3	5, or 13·52 per cent.

The number of cases here brought together is certainly too small to supply a certain inference; but the excess of instances, in which the first was greater than subsequent hemorrhages (in a ratio closely similar in the two sexes), is, nevertheless, very striking, and accords with what I have observed in regard of cancerous hemorrhages from various organs.

§ 6. The connexion of the stage of the disease with hæmoptysis is illustrated by the subjoined table, founded on the analysis of 128 cases.

Table, showing the frequency of Hæmoptysis in relation to the *Stage* of the Disease at Death, or at the close of Period of Observation.

	Total No. of cases.	Freq. of Hæmoptysis.	
		Absolute.	Per cent.
First Stage	39	28	71·79
Second Stage	20	18	90·00
Third Stage	69	57	82·61

Hæmoptysis is, it would follow, more frequently met with in persons who have reached the second and third stages, than in those in whose lungs softening has not yet occurred. This appears to hold good in both sexes, as may be gathered from the next table;—but it is more markedly true of males than of females.

	Males, 74.			Females, 54.		
	No. of Cases.	Hæmoptysis.		No. of Cases.	Hæmoptysis.	
		Absolute frequency.	Per cent. frequency.		Absolute frequency.	Per cent. frequency.
First Stage . . .	18	12	66·66	21	16	76·19
2d and 3d Stages	56	49	87·50	33	26	78·78

It may appear useless enough to make this a point of numerical investigation, softening and excavation would so plainly appear to be conditions much more conducive to perforation of vessels (whether on a large or a minute scale), than mere deposition of tubercle. But, per contra, the singular obliteration of the minute branches of the pulmonary artery, which occurs more or less completely around cavities, might be put forward as a reason why hemorrhage should, as the disease advances, become more rare and less abundant. So true is this, that it appears quite rational to suppose that some other coexistent condition (rather than the destruction of lung-substance) may have been the real cause of the excess of hemorrhagic cases among patients having reached the second and third stages; for instance, it will be well to see whether the *duration* of the disease in the two classes, where softening had and had not occurred, may not have had such influence: this point is examined in the next

Table, showing the Mean Duration of the Disease (*in months*) to Death or close of Observation, in Hæmoptoic Patients, either in the First, or in the Second and Third Stages.

	1st Stage.		2d and 3d Stages.	
	Cases.	Duration.	Cases.	Duration.
Both Sexes . .	27	26·55	68	26·28
Males	12	29·44	46	25·81
Females	15	24·25	22	27·27

Hence it appears that, up to the close of observation, the duration of the disease had been very closely the same in hæmoptoic patients who had reached the second or third stages, and in those who still lingered in the first; and it consequently follows that the conditions of softening and excavation do *per se* exercise an appreciable influence on the generation of hemorrhage,—especially as in hæmoptoic males having reached the second and third stages (those in whom the greatest frequency of hæmoptysis was noted), the mean duration had been nearly four months less than in those as yet unadvanced beyond the first.

§ 7. I have endeavoured, from the analysis of 131 cases, to ascertain whether hæmoptysis is more prone to occur from disease of either lung than of the other; limitation of the disease to one lung not having occurred in any of these hæmoptoic cases, they can only be compared in respect of the degree to which the disease had advanced in each.

Relative State of Lungs.	Males.		Females.		Both Sexes.	
	Hæm.	No Hæ.	Hæm.	No Hæ.	Hæm.	No Hæ.
Not sufficiently distinguished	8	3	8	1	16	4
Not materially unequal	8	1	12	2	20	3
Right further stage than left	9	2	3	1	12	3
Right more extensive in same stage	17	3	2	3	19	6
Left further stage than right	5	1	14	3	19	4
Left more extensive in same stage	14	3	6	2	20	5

From these absolute numbers it may be calculated that in 77·78 per cent. of the 40 cases of excess of disease on the *right* side there was hæmoptysis; and in 81·25 per cent. of the 48 cases of such excess on the *left* side that symptom had been present. This difference of 3·5 per cent. is too slight to be important,—especially as where both lungs appeared sufferers to a tolerably similar amount (23 cases), hæmoptysis occurred in nearly 87 per 100 of the number.

This refers to spitting of blood of all amounts; but the same statements apply to more or less profuse hæmoptysis considered in particular.

§ 8. The *ages* of 129 sufferers from hæmoptysis are exhibited in the following table:

Age.	73 Males.		56 Females.		129 Total.	
	Hæm.	No Hæ.	Hæm.	No Hæ.	Hæm.	No Hæ.
0 and under 10	0	0	0	0	0	0
10 „ 15	0	1	1	0	1	1
15 „ 20	6	1	10	2	16	3
20 „ 25	11	3	10	3	21	6
25 „ 30	14	3	10	6	24	9
30 „ 35	8	2	5	1	13	3
35 „ 40	10	2	3	0	13	2
40 „ 45	6	1	2	0	8	1
45 „ 50	4	0	3	0	7	0
50 „ 55	1	0	0	0	1	0
55 „ 70	0	0	0	0	0	0

The numbers here are small; but an inference not deficient in interest may be derived from comparing them, when assigned to two equal periods from ætat. 15 to 30, and ætat. 30 to 50. It follows, in truth, that of 79 persons, aged from 15 to 30, 61 (or 77·22 per cent.) had hæmoptysis; while of 47 others, aged from 30 to 50, 41 (or 87·23 per cent.) had suffered with that symptom. And the

increase of frequency of hæmoptysis with advancing years, thus shown
exists (as will be seen by the next table) in both sexes.

Age.	Males, 71.			Females, 55.		
	Total.	Hæm.	Per centage.	Total.	Hæm.	Per c
15 and under 30	38	31	81·58	41	30	7
30 — 50	33	28	84·84	14	13	9

The increase of frequency is more abrupt, it appears, in females
does this depend on catamenial influence? Taking 40 as the
decline in amount of that discharge, the cases may be arranged th

	Per cent. frequency of Hæmoptysis.	
	Males.	Females.
15 and under 40	81·66	76·00
40 — 50	90·90	100·00

Hence, while females were less disposed to hemorrhage f
males before the age of 40, they were very distinctly more s
latter, once the height of the catamenial age had been pass

§ 9. But the idea suggests itself, that the *duration of th*
of observation or of death) might have been the real cause
established, that the disease had been of longer duration in
age, and pari passu the cases of hæmoptysis had a long
persons independently of any special influence of age.
value of this surmise, we must ascertain the mean du
those who had had, and had not had, hæmoptysis at th
of observation.

TABLE, exhibiting the Mean Duration of the Dise
Death or close of Observation in 125 Cases,
and had not Hæmoptysis.

	Duration in
	Hæmop. present.
Males 74	26·42
Females . . . 51	25·70
Both Sexes . . 125	26·15

Those who had hæmoptysis, then, had been p
nearly eight months longer than those who ha
symptom. Scarcely, however, can the reality o
disease be considered to set aside the special in
of hæmoptysis;—especially as in infancy hæmo

§ 10. The period of the disease at which the
of hæmoptysis is seen in the following table:—

	52 Males.	35 Females.	Frequency of Hæmoptysis.		
			Absolute.	Per cent.	
First symptom	10	6	16	39·08	
Along with first symptoms	9	9	18		
Within first week . . .	2	3	5	10·04	
Within first month . . .	4	0	4		
Within six months . . .	6	6	12	31·04	50·57
Within twelve months . .	8	7	15		
Above twelve months . .	13	4	17	19·54	

Several inferences may be drawn from this table.

1. No period of the disease was exempt from the chance of hæmoptysis.

2. The most common periods for its occurrence were at the very outset (39·08 per cent.), or after the expiration of the first month (50·57 per cent.). Its frequency again appears to increase slightly from six months upwards.

3. It is very rare (occurring only in 10·04 per cent. of cases of hemorrhage) for hæmoptysis to occur within the first month, unless it has been absolutely the first or among the first symptoms.

4. Among these patients 88·57 per cent. of the women, and only 75·00 of the men, had their first hemorrhage within twelve months. Hemorrhage is more frequent as the first, or an attendant on the first, symptoms in females than in males.

§ 11. So far I have made no distinction of cases of trifling and of more or less notable hemorrhage, in connexion with the period at which this symptom first appears; this distinction is made in the following table:

	Males.			Females.			Both Sexes.		
	Streak or tinge.	Up to 4 oz.	Beyond 4 oz.	Streak or tinge.	Up to 4 oz.	Beyond 4 oz.	Streak or tinge.	Up to 4 oz.	Beyond 4 oz.
First symptom	0	2	8	0	4	2	0	6	10
With first symptoms . . .	3	2	0	4	3	1	7	5	1
Within week .	0	0	1	0	2	1	0	2	2
Within month	0	2	1	0	0	0	0	2	1
Within six months . . .	1	3	2	1	5	0	2	8	2
Within one year	1	4	3	0	3	2	1	7	5
One year and upwards . .	5	3	4	1	1	2	6	4	6

From this table it follows that—

1. In upwards of half the cases of notable hemorrhage (beyond 4 oz.) this occurs, or has occurred, as the first symptom, corroborating the inference before drawn (p. 245) as to the excess of amount of first over subsequent hemorrhages.

2. Hemorrhage of this amount is rare as a coexistence with other first symptoms (in $\frac{1}{13}$ of these cases) appreciable by the patient.

3. Streaked or tinged sputa are, on the contrary, of very common appearance among the earliest symptoms.

4. But streaked or tinged sputa are rarely (or never) *the* first symptom singly and alone.

In speaking of hæmoptysis as the "first symptom" of the disease, I simply mean that it is so accounted by the patients themselves,—that it was the first occurrence which led them to notice the state of their health particularly. I do not believe that hæmoptysis is *ever really the first symptom*; at least I have never yet failed by careful interrogation of patients, who alleged that it was so, to satisfy myself that others (slight cough, loss of flesh, anorexia, &c.) had led the way.

On the other hand, there can be no question that a sudden increase in the intensity of all the local symptoms often takes place immediately in connexion with hæmoptysis;—but whether the hæmoptysis is in some unexplained way the cause of this, or merely a coexistence with it, is yet matter for inquiry. I am inclined to the latter opinion,—hence (it will be inferred) I do not believe that hæmoptysis is ever the primary cause of tuberculization—in fact, as just mentioned, I have always discovered pre-existing symptoms.

§ 12. An individual, having had hæmoptysis of more or less severity, may have no recurrence for ten, fifteen, or twenty years; I have heard of such renewed attacks proving directly fatal,—but no instance of the kind occurs among these cases.

§ 13. It is matter of common belief that season exercises a rather marked influence on the occurrence of a first hemorrhagic attack; the analysis of my cases does not support this belief.

	Total Cases.	Streaked or tinged.	Up to 4 oz.	Beyond 4 oz.
Oct. to April . .	38	6	23	9
April to Sept. .	31	6	12	13

The excess of first attacks in the winter and spring months is slight; such as it is, it appears to depend upon the serious order of hemorrhages, rather than upon the trifling.

§ 14. The anatomical condition of the lung immediately leading to hemorrhage, is matter rather of inference than demonstration. My experience, such as it is, may be thus put. (1.) I have never yet seen coexistent tuberculization and pulmonary apoplexy (either nodular or diffuse) in a person who had suffered, more or less recently before death, from hæmoptysis. Nor can I even call to mind more than one case in which the same lung has been the seat of coexistent tubercle and pulmonary apoplexy.* Pulmonary apoplexy, then, is not the cause of phthisical hæmoptysis. (2.) I have never yet *seen* a ruptured vessel, which had been the cause of pulmonary hemorrhage;—but careful injection of the lungs would have been necessary to determine whether such vessel existed or not. (3.) It would follow that intense congestion ending in molecular ruptures (the old hemorrhage by exhalation) must be the common cause of hæmoptysis,—an inference supported by the fact that the physical signs and local and general symptoms of active congestion do sometimes precede the discharge of blood.

§ 15. Hæmoptysis, as a sign of tubercular disease, is only *clinically* valuable in the first stage; in the second and third stages the physical evidences of the disease (unless in singularly rare and exceptional instances) are, it may be assumed, conclusive.

It appears, then, that in 71·79 per cent. of my cases still in the first stage (p. 246) there had been hæmoptysis; but it is infinitely probable that the real ratio is higher than this, as the table quoted from refers to the stage of the disease existing either at death or at the close of observation, and not at the period of hæmoptysis having occurred. Further, it follows from the table at p. 249, that 39·08 per cent. of those probably† destined to have hæmoptysis, have it as the first, or among the first symptoms. Hence, in regard both of *stage* and *time*, the earliness, with which hæmoptysis frequently appears, is certain, and its occurrence absolutely valuable in diagnosis.

---

* On the other hand, I have met with nodular pulmonary apoplexy in several instances, where no particle of blood had been expectorated during life.

† I cannot say *certainly*, because all my cases were not observed till the occurrence of death.

But hæmoptysis is not a positive indication of tubercle, unless under certain conditions; and in examining these conditions I start with the postulate that the characters and manner of the hemorrhage prove that the blood is not furnished by the nares, mouth, throat, or stomach.

(*a.*) The *quantity* of blood voided, is the first point for consideration. It is commonly said that the expectoration of streaked or tinged sputa is utterly insignificant, because such are seen in *bronchitis*, &c. &c.; but no attempt has ever been made to decide numerically to what extent this is true. I find that in 25 cases (observed at Brompton, and at University College Hospital) of chronic bronchitis with or without marked emphysema (*but always without serious disease of the heart*), the absence or presence of such expectoration was noticed in the following proportion.

	Streaks of Blood in Sputa.	
	Absent.	Present.
Males . .	10	1
Females .	9	5

Now in *all* these six cases of streaked expectoration, there was more or less ground for suspicion that tubercles were to a slight amount present,—in two of them this was proved to be the fact by post-mortem examination.

While, then, bloody expectoration occurs in 71·79 per cent. (ante p. 246) of tuberculized persons in the first stage, it occurred in 24 per cent. of bronchitic people (free from serious cardiac disease); but in all of the latter there was either suspicion or certainty of the existence of tubercle to a *slight* amount.* The mean duration of the disease in the phthisical cases was 26·55 months, in the bronchitic, 49·50,—hence the significance (*quoad* tubercle) of hæmoptysis is greater even than the relative per centages above given would signify.

It appears (p. 249) that streaked or tinged sputa are rarely or never the "first symptom" of phthisis;—should they appear in this guise, then, they would probably be dependent on some other cause.

(*b.*) *Primary cancer of the lung and mediastina*, as I have elsewhere shown, from the analysis of a small number of recorded cases, is very frequently attended with sanguineous expectoration or pure hæmoptysis; in regard of this symptom the two diseases may be thus compared:

	Cancer of Lung and Mediastina.	Phthisis.
Ratio per cent. of Hæmoptysis of all amounts in cases of .	72·00	80·92
Ratio of cases of Hæmoptysis above 1 oz., to cases of Hæmoptysis generally in .	71·43	40·41 (p. 20, § 3.)

Hence, 100 cases of cancer of the lung will be attended nearly as often with hæmoptysis of all amounts, and greatly more often with hæmoptysis above an ounce in amount at a time, than 100 cases of phthisis. But, on the other hand, tuberculous is so vastly more frequent than cancerous disease of the lung, that the share of the population suffering at any time from cancerous hæmoptysis, will form but an insignificant fraction of that suffering from hæmoptysis of tuberculous origin. The distinction of the disease is, clinically, easy.

---

* Pathologically, these people were *latently* tuberculous persons, with superadded bronchitis; but practically, they could only be regarded as bronchitic.

(*c.*) It is not sufficiently insisted upon by writers, that *empyema* (become decidedly chronic), whether in the stage of dilatation or of retraction, does not give rise to hæmoptysis. In sixteen well-marked cases which I have had under my care within the last two years, for periods of three months and upwards, no single example of hæmoptysis occurred. But, more than this, empyema, established in a phthisical person, seems to be *preventive of hæmoptysis.* In seven cases of combined tuberculous excavation and empyema, carefully watched, and proved as to the diagnosis, either by dissection or by indubitable signs, no spitting of blood had ever occurred. All these seven persons were males. The pressure exercised on the lung by the contents of the pleura, might appear to explain the fact plausibly enough, especially as the excavations were on the same side as the empyema in six of the seven cases; and I have recently seen a curious exemplification of the apparent influence of excessively acute pleuritic effusion in arresting obstinate hæmoptysis. But, on the other hand, in one case, excavations existed in both lungs.

(*d.*) *Simple chronic consolidation of the lung* has not, in my experience, been attended with hæmoptysis to any amount.

(*e.*) I have never known *ulceration of the larynx* productive of discharge of blood to any extent; streaks are not uncommon. But ulceration of the larynx (proceeding from within outwards) seems not to occur as a primary affection; at least I have never seen it except in phthisis (which may be completely latent in regard of the lung), cancer, and syphilis: the diagnosis therefrom would not be difficult.

(*f.*) Hæmoptysis arising from *disease of the heart* can with difficulty be confounded, even in itself, with the severer forms of phthisical hemorrhage; while the physical signs of the cardiac disease will point to its true source in such cases of the slighter form. I have never once seen cardiac disease, of such kind as to cause hæmoptysis, coexistent with *phthisis*, using the term in its practical sense; but in a fair number of instances I have seen advanced cardiac disease in persons whose lungs contained *crude tubercles and gray granulations.* It may be, therefore, that the conditions of the system existing in heart-disease are unfavorable to the development of tubercle; but the infrequency with which the two kinds of disease are found together doubtless depends, in the main, on the difference in the periods of life at which each is especially prone to occur.

(*g.*) *Aortic aneurism*, opening into the trachea, may (without proving immediately fatal) give rise to hemorrhage, indistinguishable by its own characters from profuse pulmonary hemorrhage. The history of the case, the physical signs, the age of the individual, &c., commonly establish the diagnosis; but when the aneurism is small, and so placed as to elude percussion, and pressure-signs, both concentric and eccentric, are absent, the difficulty of *proving* the existence of aneurism may be insurmountable; the existence of the disease may be divined, but not demonstrated. It is to be remembered that the absence of notable signs of tuberculization does not justify the inference that the hæmoptysis is not phthisical, seeing that a tremendous pulmonary hemorrhage may occur when slight consolidation exists at one apex only, and that such consolidation might be supposed to depend on the presence of an aneurism.

(*h.*) It is matter of common belief that in women who menstruate imperfectly and irregularly, the expectoration of a small quantity of blood is insignificant. I think this *perhaps* true where streaks only are concerned; but in every instance I have had under my observation, where such succedaneous hæmoptysis reached an ounce or upwards, there has been either evidence, or ground for suspicion, of tuberculization. Similarly I have seen two cases of individuals presumedly in a state of perfect health, who, in a violent fit of passion, brought up a certain quantity of blood from the lungs: *both had latent tubercles.*

The tendency of my experience, then, is clearly to show the vast frequency with which hæmoptysis is in some manner or other an attendant on tuberculous disease. The fact, that individuals are occasionally met with who, after having had more or less profuse hæmoptysis, live on to a good age without exhibiting phthisical symptoms, does not invalidate this result: it simply shows that tuberculization tending to hæmoptysis may, as well as that not so-tending, undergo spontaneous suspension; and the consideration of the point becomes rather a question of prognosis than anything else.

§ 16. *Prognosis.*—Hæmoptysis may kill *directly* or *indirectly.* The entire series of 131 cases of phthisis furnish but two examples of such modes of death. In the one, death was *direct*—instantaneous, indeed; but it arose from asphyxia, not loss of blood; the trachea and bronchi, as far as traceable, were plugged with coagula. In the other, death was *indirect;* the system never rallied from the loss of blood, and death occurred from exhaustion within a few (five) days. Four other individuals (not belonging to this series, but who were at the time, or had previously been, under my care) have, to my knowledge, perished from hæmoptysis, one directly, three indirectly: there is a certain chance of death, then, from hæmoptysis. All six individuals were *males.*

(*b.*) A first hemorrhage having been severe, it is unlikely that a subsequent one will kill *directly;* for it is not often that a subsequent hæmoptysis is more severe than a first severe one, granting that they do not follow so closely upon each other as to appear a mere continuation of one and the same attack. One of the six cases referred to constituted an exception to this rule.

(*c.*) As it appears (p. 248, § 9) that hæmoptysis is frequent in proportion to the duration of the primary disease, the converse of the proposition cannot be evaded; and we are forced to conclude that *frequently-recurring hæmoptysis does not reduce the mean duration of life, after seizure with tuberculous symptoms, in any given mass of cases.* It is not repugnant to reason to admit, then, that in a certain number of instances hemorrhage from the lungs may act as a local therapeutical agent. No doubt the disease is sometimes observed to undergo an increase of rapidity after an attack of hæmoptysis; but this is rare, and the two things may be merely coincidences. It would be important to ascertain the relative duration of life (after the outset of phthisical symptoms) in those who have had slight, medium, and profuse hæmoptysis, and also in those who had such hæmoptyses at the outset, or at a later period, of the disease; but I have not a sufficient number of reports of fatal cases at command to furnish trustworthy inferences.

(*d.*) A first hemorrhage being moderate, subsequent ones are not likely to be severe. This follows from p. 245, § 5, and p. 249, § 11.

(*e.*) Sex is an element not to be disregarded in estimating the prognosis of hæmoptysis. The symptom is, in several points of view already set forth (ante p. 249, §§ 3 and 4; p. 253, § 16), of more serious import in males than females.

SUMMARY OF MORE IMPORTANT RESULTS.

§ 1. There apparently exists a greater proneness to early marriage among phthisical, than non-phthisical persons; this greater proneness exists in both sexes, and, in the cases analysed, prevailed to the same amount in males and females.

§ 2. Of a given mass of patients entering the hospital in all stages of the disease, and in every variety of general condition—between the actually moribund state and that of but slight constitutional suffering—the number leaving it, on the one hand, *improved* or *unadvanced* was more than double that, on the other hand, leaving it in *a worse state* or *dying within its walls* (the exact ratio is 67·84 : 32·16). If the cases, in which death was actually imminent at the period of admission, were excluded, the result would be very materially more favorable than this.

§ 3. In 4·26 per cent. of the cases, complete restoration to health, not only as regards apparent disturbance of the functions generally, but as regards local evidence of active pulmonary disease was effected.

§ 4. Complete removal of symptoms was more frequently effected in the male than in the female; but, on the other hand, the results were, on the whole, slightly more favorable in the latter than in the former sex.

§ 5. All patients whose condition grew worse, while they were in the hospital, had reached the stage of excavation on admission; and all patients, whose tubercles were yet unsoftened on admission, left the hospital either improved, or having had a *statu quo* condition kept up.

Improvement is more probable than the reverse, even where excavation exists on admission.

§ 6. In a given mass of cases, the chances of favorable influence from sojourn in the Hospital will be greater, in a certain (undetermined) ratio, as the duration of the disease previous to admission has been greater,—in other terms, natural tendency to a slow course is a more important element of success in the treatment of the disease, than the fact of that treatment having been undertaken at an early period. (Vide p. 229, § 4, 1, 2, 3.)

§ 7. The mean length of stay in the hospital in the most favorable class of cases, nearly doubled that in the least favorable.

§ 8. The chances of benefit are more in favour of those whose trades are pursued out of doors (wholly or partially), than of those who work altogether within doors.

§ 9. The results did not appear to be influenced by the laborious or non-laborious character of the trade individuals might have pursued.

§ 10. The age of the sufferers did not exercise any very material influence on the character of the results.

§ 11. Patients coming from the country have, on an average, a slightly stronger chance of improvement, than the residents of London and the suburbs.

§ 12. Patients admitted during the warmer half of the year, benefit by a sojourn at Brompton, to a slight extent, more than those received during the six colder months.

§ 13. From a comparative analysis of the family histories of 446 persons, 162 of them phthisical, 284 non-phthisical, the final conclusion flows:—*that phthisis in the adult hospital-population of this country is, to a slight amount only, a disease demonstrably derived from parents.* It is possible (nay, indeed, *probable*, for, in adults having a parental taint, the outbreak of the disease occurred a mean period of two years and nine months earlier, than in those free from such taint, vid. p. 238, Table,) that, were investigation extended to infancy, childhood, and youth, the ratio of cases of parental taint among the phthisical, would be proportionally greater than it proves where inquiry is limited to adults. But, on the other hand, there is no single valid reason for *supposing* (prior to actual experience) that the increase in that ratio would be of more than trifling amount. Again, whether the law differs in the adult portion of the middle and upper classes of society, from that holding in the humbler classes (those supplying hospitals,) can only be positively determined by an analysis of family histories collected among the former classes; meanwhile, it appears justifiable to doubt the reality of any such difference.

§ 14. If no distinction of sex be made, it appears that the softening point is attained with not very unequal rapidity in the right and the left lung; but such difference as exists signifies that softening is, on an average, more quickly accomplished on the left side than the right.

§ 15. But the law seems very clearly to differ in the two sexes in regard of this point: the disease is more rapidly evolved in the right lung of males, and in the left of females.

§ 16. Age exercises some influence over the course of the disease in the two lungs in the two sexes: in males, the excess of frequency with which the right lung had reached the second and third stages was within 2 per 100, the same before and after the age of 30; whereas in females the excess of frequency with which the left lung was found in those stages was about 45 per 100 greater after, than before, that age.

§ 17. Hæmoptysis is a symptom of extreme frequency,—occurring in about 81 per 100 of the cases.

§ 18. It was of very slightly (4 per 100) more frequent occurrence in males than females.

§ 19. Hæmoptysis to a medium amount is about four times less common than to very slight or to profuse amounts (both taken together). Very profuse hemorrhage from the lungs is more common in males than females.

§ 20. Medium frequency of recurrence of hæmoptysis is materially less common (and this in both sexes) than a single, or than repeated attacks. Further, repetition of hæmoptysis is more common in males than in females.

§ 21. It is materially more common for a first hemorrhage to be more profuse than subsequent ones, than for subsequent ones to be more profuse than the first.

§ 22. Hæmoptysis is more frequently met with (and this independently of any influence of duration of the disease) in persons who have reached the second and third stages, than in those whose lungs have not yet softened. This proposition is more markedly true of males than of females.

§ 23. There does not appear to be any notably greater tendency to hæmoptysis, where the right lung has reached a more advanced stage than the left, nor vice versâ, where the left has taken the lead. It seems improbable that either lung is more effective in causing hæmoptysis than its fellow.

§ 24. The frequency of hæmoptysis increases with advancing years in both sexes. The increase is more abrupt in females than in males, and in the former appears connected with the catamenial function.

§ 25. This greater frequency of hæmoptysis in persons of more advanced years, does not depend altogether on greater duration of the disease; for those, who had had hæmoptysis, had been phthisical for only a mean period of eight months longer than those who had not spit blood.

§ 26. The most common periods for the occurrence of hæmoptysis, were, at the very outset, or after the expiration of the first month; it is very rare for hæmoptysis to occur within the first month, unless it has actually appeared as the first, or among the first symptoms. The phrase "first symptom" here, is to be understood with the qualification already explained.

§ 27. In upwards of half the cases of notable hemorrhage (beyond 4 oz.) this occurs, or has occurred, as the "first symptom," corroborating the inference as to the excess of amount of first over subsequent hemorrhages.

§ 28. Hemorrhage of this amount is rare as a coexistence with other first symptoms (in $\frac{1}{13}$ of these cases) appreciable by the patient.

§ 29. Streaked or tinged sputa are, on the contrary, of very common appearance amongst the earliest symptoms.

§ 30. But streaked or tinged sputa are rarely (or never) *the* "first symptom" singly and alone.

§ 31. Season does not appear to exercise any marked influence on the occurrence of a first hemorrhagic attack.

§ 32. Hæmoptysis never appeared as the *bonâ fide* first symptom in these cases, the phrase being understood in its absolute sense without qualification.

§ 33. Pulmonary apoplexy is not the cause of phthisical hæmoptysis; the coexistence of pulmonary apoplexy and tubercles in a person who has had hæmoptysis, more or less recently before death, is at the least very rare. The common cause of hæmoptysis in tuberculous persons, is intense congestion ending in molecular ruptures.

§ 34. Expectoration of blood in persons labouring under chronic bronchitis, with or without emphysema, but without notable disease of the heart, justifies in itself a suspicion of the existence of latent tubercles.

§ 35. A given mass of cases of cancer of the lung will be attended nearly as often with hæmoptysis of all amounts, and greatly more often with hæmoptysis above an ounce at a time, than an equal mass of cases of phthisis. But cancerous disease of the lung is, comparatively speaking, so rare, that this proposition does not materially affect the diagnostic value of hæmoptysis in phthisis. Besides, the distinction of the diseases is, clinically, easy.

§ 36. Not only does chronic empyema not give rise in itself to hæmoptysis, but empyema, established in a phthisical person, appears to a certain extent prophylactic against the hæmoptysis which is almost an appanage of the latter disease.

§ 37. I have never once seen cardiac disease, of such kind as to cause hæmoptysis, coexistent with *phthisis*, using the term in its practical sense ; but in a fair number of instances I have seen advanced cardiac disease in persons whose lungs contained *crude tubercles and gray granulations*, which had been completely, or almost completely, *latent*.

§ 38. Contrary to common belief, it may be affirmed, that when the *nisus hæmorrhagicus* in women menstruating imperfectly, directs itself to the lung, and leads to the evacuation by that organ of a quantity of blood, amounting to or exceeding an ounce, there is motive for suspecting the existence of tubercles.

§ 39. All these propositions tend to exhibit in strong light the diagnostic signification of hæmoptysis *quoad tubercle ;* but it is to be remembered that they will only hold completely true, if applied to *latent* as well as *obvious* tuberculization.

§ 40. The diagnostic importance of hæmoptysis as a symptom of tuberculization of the lungs is, indeed, extreme,—even sputa, simply streaked or tinged with blood, are not without their significance under certain circumstances.

§ 41. Hæmoptysis is rarely "directly" fatal ; more frequently so in males than in females.

§ 42. Frequently-recurring hæmoptysis does not reduce the mean duration of life in any given mass of tuberculous cases.

§ 43. A first hæmoptysis having been severe, it is unlikely that a subsequent one will kill "directly ;" and a first hemorrhage being moderate, subsequent ones are not likely to be severe.

§ 44. The prognosis of hæmoptysis is materially more unfavorable in males than in females.

PART FOURTH.

Periscope.

ANATOMY AND PHYSIOLOGY.

On the Circulation in Insects. By EMILE BLANCHARD.

THE author of this paper is well known to comparative anatomists by his admirable 'Memoirs on the Organization of various Invertebrated Animals;' he has here given a detailed account of his researches on the circulation in insects, which will be found of peculiar interest both in an anatomical and a physiological point of view.

The previous state of our knowledge on this subject may be expressed in a very few words. It was admitted by every one, save by M. Léon Dufour, that the *dorsal vessel* is to be regarded as the central organ of the circulation, and that the blood is propelled through it, from behind forwards, by the successive contractions of its multiplied subdivisions. The anterior portion, which is contracted into a vascular trunk or aorta, subdivides in the head into several branches; but no one has succeeded in tracing the continued course of these. A movement of fluid has been seen in the wings of various insects during their development; and in a few even subsequently to their last metamorphosis. No system of vessels, however, has been detected; the only discoverable traces of vascular trunks, being the canals which deliver the blood into the posterior divisions of the dorsal vessel, and a tube which lies along the upper surface of the ventral nervous cord, and which has been termed by Mr. Newport the *supra-spinal* vessel. The course of the circulation has therefore been usually described as follows. The blood propelled forwards by the dorsal vessel is conveyed by the aortic trunk to the head; from the subdivisions of that trunk it passes into the *lacunæ* of the body, which it permeates, at the same time coming in its passage into frequent relation with the tracheal system, which is so extended through the body as to bring the air to meet the blood. From the lacunæ it is collected by efferent canals, or venous trunks, which return it to the posterior part of the dorsal vessel.

These views have been found by M. Blanchard to be correct as far as they go. He has carefully examined the sanguiferous system by means of injections; and he has found that, whilst these are readily transmitted through the dorsal vessel from behind forwards, so as to distend the aortic trunk and its ramifications, they soon become diffused through lacunæ destitute of distinct walls,—the parietes of the vessels by which the fluid is first transmitted, becoming gradually thinner and thinner, and at last disappearing altogether. He appears, moreover, to consider that the parietes of the venous canals, and of the supra-spinal vessel of Newport, are not proper vascular walls, but are only composed of areolar tissue more condensed than that which bounds other lacunæ.

M. Blanchard has added, however, a new and most important fact to the account previously given; namely, that the circulating fluid passes into that space between the internal and external walls of the tracheæ, in which the elastic spiral fibre lies.

This he ascertained by means of his injections, which he found to communicate their colour to the walls of the tracheæ, even to their minutest ramifications; and he gives ample reason for not regarding this coloration as the accidental result of imbibition. The passage of the circulating blood into this space has been further established by the observation of Mr. Newport, who has detected blood-corpuscles lying between the internal and external walls of the tracheæ. Thus it appears that the use of the spiral fibre is not merely to preserve the air-tubes themselves from being compressed; but also to keep open a space between their internal and external walls for the circulation of the blood. When the air-tubes undergo dilatation into vesicles, the spiral fibre loses its continuity; but a series of irregular passages is still found between the internal and external walls of these vesicles. The internal membrane of the tracheæ is continuous with the outer integument of the body, of which it is a reflexion; the external membrane, on the other hand, disappears near the entrance of the tracheæ, and the space between them seems there to come into connexion with the neighbouring lacunæ. Thus the tracheæ are ensheathed, even to their minutest ramifications, by the circulating fluid; indeed we might describe them as blood-vessels no less correctly than as air-tubes. If we could imagine the entire pulmonary arterial system of man penetrated by an extension of the bronchial tubes, which should occupy the central part of each canal, leaving a space around for the flow of blood, we should have a correct notion of the relation of the sanguiferous and respiratory apparatus in insects,—it being borne in mind, however, that the respiratory apparatus of the latter is not restricted to one part of the body, like that of man, but is diffused throughout the whole. We thus see that the air introduced by the tracheæ of insects, is brought into much more direct relation with their blood than has been generally supposed; and that it cannot have any influence upon the solid tissues (upon which some have imagined it to have a direct or immediate action), except through the medium of their blood.

The investigations of M. Blanchard have not yet enabled him to speak with certainty with regard to the course of the circulation: and as the colour of the blood affords no means of discrimination between that which has been oxygenated, and that which has been rendered venous by its circulation through the tissues, there is no obvious means of arriving at a positive conclusion in the absence of anything like vascular continuity. The following is M. Blanchard's own summary. "The blood propelled to the head diffuses itself in all the inter-organic spaces; at the same time it is directed into the lacunæ situated near the origins of the respiratory tubes, and then penetrates between the membranes of the tracheæ, in the space kept open by the spiral fibre. The nutritious fluid, thus conveyed to all the organs between the two tunics constituting the respiratory tubes, is separated by a single membrane only from the column of air; and it is thus subjected to re-oxygenation during its course. The tracheæ thus become, in their ramifications, true nutritive vessels. The blood, then falling back again into the interfibrillary spaces, and thence into the principal lacunæ, is brought back to the dorsal vessel by efferent canals, composed of areolar tissue, but destitute of membranous parietes."

As far, therefore, as we can gather from this and other passages, it is considered by M. Blanchard that the blood in the dorsal vessel is of mixed quality; being partly composed of that which has been transmitted direct from the inter-organic lacunæ, and partly of that which is returned from the parietes of the tracheal system.

We would venture to suggest a course, however, which seems to us more probable on analogical grounds, and more consistent with the facts stated by M. Blanchard. He remarks that the efferent or venous canals, which bring the blood back to the dorsal vessel, commence in the neighbourhood of the tracheal orifices, and that they appear to communicate with the lacunæ around the entrances of these tubes. Is it not probable, then, that they are strictly analogous to the branchio-cardiac canals of Crustacea and Mollusca, which convey back to the centre of the circulating system the fluid which has been just aerated in the respiratory organs; and that the blood which enters the dorsal vessel has been *entirely* oxygenated? And is it likely that, in the absence of any additional organ of impulsion, the feeble power of the dorsal vessel should so far extend itself through the system of lacunæ, as to serve to propel the blood which has passed from them into the sheaths of the tracheæ to the minutest ramifications of this complex system of tubes? Thus, then, it seems more probable that the blood propelled through the lacunar system of the body, by the action of the dorsal vessel, is already in an arterial condition; that in its course it becomes venous; and that it is then received by imbibition into the sheaths of the minuter tracheæ, which serve rather as *collecting* than as *distributing* vessels, and which deliver it, after oxygenation, into the sheaths of the larger tracheal trunks, from which it is carried back by the efferent canals to the dorsal vessel.

Whatever view we may take, however, of the bearing of his discovery, the fact announced by M. Blanchard, and confirmed by Mr. Newport, that the blood circulates in the sheaths of the tracheæ, is of the greatest anatomical and physiological interest. We shall add, for the benefit of such of our readers as may desire to prosecute any such inquiries for themselves, that the injection recommended by M. Blanchard, as superior on many accounts to that ordinarily employed, consists of the Prussian blue prepared for oil-painters, diluted with spirit of turpentine. Such is the freedom of communication between the different parts of the circulating system in insects, that an injection thrown into any part of the inter-organic spaces, will pass into the dorsal vessel and the sheaths of the tracheæ.—*Annales des Sciences Naturelles*, Juin, 1848.

Effect of Ether on the Phenomena of Respiration. By MM. VILLE and BLANDIN.

AFTER making a number of experiments, these gentlemen have found that in the state of complete insensibility and apparent death which follows the inhalation of ether, there is more carbonic acid produced by the lungs, than there is during the natural play of these organs; and they further add that, in the course of etherization, the amount of carbonic acid which is given out by the lungs is in a direct proportion to the insensibility.—*Comptes Rendus*, xxiv, p. 1016.

A Case of Dichromatic Daltonism. By Dr. DECONDE.

A SOLDIER applying for his discharge on the ground of the formation of cataract, attracted particular attention on account of the cyanic colour of his sclerotica; and on inquiring into his power of distinguishing colours, it was found that all those of the spectrum were confounded in two fundamental colours, yellow and blue. Dark red, bright red, rose, orange, yellow, green in which yellow predominated, gray-white, were all perceived as different shades of yellow. Blue, green, and white, with a bluish-shade, were perceived as *blue*. All the very deep colours were confounded with black, and all the very light ones had a whitish appearance, although the individual did not seem to have cognisance of white, properly so called. Light decomposed by a prism, appeared to him of a uniform blue.—*Annales d'Oculistique*, tom. xx, p. 52.

ORGANIC CHEMISTRY.

On the Composition of the Bile.

DRS. GUNDERLACH and STRECKER have recently examined the bile of the pig; and they say that its principal constituent is a new acid, which they have named *hyocholic*. This acid, they say, is somewhat like choloidic, for which it was mistaken by Gorup-Besanez, but differs from it in many of its essential properties, and in the circumstance of its containing nitrogen, its composition being (C^{54} H^{43} NO^{10}). These authors have carefully studied the many reactions of this acid, and the properties of several of its salts. They state that it exists in the bile of the pig, in combination with soda, and that it constitutes about 75 per cent. of it. Moreover, their investigations go to show that the bile of the pig does not contain any sulphur, and that it cannot, therefore, yield taurine.—*Annal. der Chem. und Pharm.*, lxii, p. 205.

Upon reviewing, therefore, the different opinions which have been entertained with respect to the constitution of bile, it is clear that the discrepancies have been occasioned by the facility with which this substance undergoes decomposition, so as to become broken up into numerous secondary groups. Berzelius was the first to allude to this, and to clear the way to our comprehension of the subject, by showing—1. That pure bile, when it was freed from fat and colouring matter, consisted principally of a body which he called *biline.* 2. That biline was decomposed with great facility, and that when acids were present it was resolved into *fellinic acid*, *cholinic acid*, *ammonia*, and *taurine.* 3. That when the salts of lead were added to bile, a precipitate of *bilifellinate* and *bilicholinate* of lead was formed, and that these resulted from the decomposition of biline. Mulder has adopted almost the same view, but he endeavours to show that biline contains a *peculiar molecular group* (C^{40} H^{36} O^{6}), together with the elements of *taurine, ammonia,* and *water;* and he refers to the relations which exist between the composition of this group and of dyslysin, cholinic, fellanic, fellic, and choloic acids, in order to explain their formation, and to establish the truth of his hypothesis.

Dr. Strecker, however, is of opinion that bile consists essentially of *two* soda salts, viz. of a salt of a nitrogenous acid, which is free from sulphur, and of a salt of a sulphuretted acid, which is free from nitrogen; and every variety of bile, according to Dr. Strecker, is but a mixture of these two salts, the relative proportions of which may vary in different classes of animals, but, perhaps, remain almost the same in individuals of the same class. He states, moreover, that the composition of the nitrogenous acid (cholic C^{52} H^{43} NO^{12}) may also undergo an alteration in different animals, for he has found that it is displaced by hyocholic acid (C^{54} H^{43} NO^{10}) in the bile of the pig.—*Annal. der Chem. und Pharm.*, lxv, p. 1-37.

So that it appears now to be pretty well established, that bile is not a very complex substance; but that it either consists of one salt, *bilate of soda* (Mulder), or of two salts *cholate* and *sulpho-choloate of soda* (Strecker), together with colouring matter (*biliphaein*) and variable proportions of *fat, cholesterine,* and *mucus.* Future investigations must determine which of these two opinions is the correct one.

Sulphur in bile. Dr. Bensch has determined that the following proportions of sulphur exist in the 100 parts of dried bile.—7·2 in the serpent; 6·46 in the fish; 6·46 in the sheep; 6·38 in the bear; 6·21 in the dog; 5·62 in the calf; 5·57 in the chicken; 5·56 in the fox, 5·55 in the goat; 5·03 in the wolf; 4·0 in the ox; 0·32 in the pig.—*Annal. der Chem. und Pharm.*, lxv, p. 194.

Blue Coloration of the Dressings of Ulcers.

M. SCHLOSSBERGER attributes the blue coloration, which is sometimes seen upon the dressings of old ulcers, to phosphate of iron (vivianite), and not, as many chemists suppose, to prussian blue. He founds his opinion upon the following fact: that in the stomach of an ostrich he found three iron nails impacted in a fetid black mass of fat and dried blood; some days after its exposure to the air, this mass became blue in the neighbourhood of the nails; and on analysis it yielded 25 per cent. of water, and 50 per cent. of peroxide of iron, together with a considerable quantity of phosphoric acid.—*Annal. der Chem. und Pharm.*, lxii, p. 382.

Nature of the Acid in Gastric Juice.

THERE is even at the present time a difference of opinion among chemists, with respect to the nature of the acidifying principle of the gastric juice. Some imagine that the acidity of this liquid is due to lactic acid, others to hydrochloric acid, and a third party attributes it to the presence of the superphosphates. M. Schmidt, without making any direct experiments upon the subject, is inclined to think that the acidity is occasioned by a complex acid, which, under the influence of heat, is decomposed into hydrochloric acid and pepsine. Liebig, in common with some other chemists, denies the existence of free hydrochloric acid in the stomach; and he believes that when it is obtained by the simple distillation of the gastric juice, it is formed by the reaction of the lactic and phosphoric acids, which are present in it, on the chlorides; and that they thus give rise to the production of free hydrochloric acid. Lehmann has recently made a new examination of this subject; and he finds that when he operates upon the stomachs of dogs, which are placed in a vacuum, in such a manner as to cause the vapours from the gastric juice to traverse a tube containing a solution of nitrate of silver, there is not any indication of free hydrochloric acid, until the concentration of the liquid has been carried so far as to permit the action of the lactic acid on the earthy chlorides. His observations, therefore, confirm the remark of Liebig, as well as the results of Bernard and Barreswill, as to the nature of the acid in the gastric juice.—*Archiv der Pharm.*, c, p. 79.

On the Existence of Lactic Acid in the Animal Fluids.

IT would not be advantageous to refer to *all* the circumstances which have tended to hinder the development of this subject; but it is very possible that some of them might have been dependent upon the uncertainty which has hitherto clouded the reactions of lactic acid and its salts;—thus, for example, it was stated by Pelouze, in 1844, that lactate of copper was not completely decomposed by milk of lime, and that this fact would serve to distinguish the lactate of copper from the acetate. Almost directly afterwards, M. Boussingault took advantage of this property, in order to detect the presence of lactic acid in the urine of the herbivora; and M. Gobley also made use of it, when he discovered lactic acid in the yelk of egg; while Bernard and Barreswill adopted it as a means of recognising this acid in the gastric juice. M. Strecker, however, has since confidently stated, that the presence of lactic acid does not altogether prevent the precipitation of oxide of copper by milk of lime, but that it merely delays it; and he, moreover, asserts that many other organic substances, as sugar, gelatine, glycocoll, impure lactate of lime, fibrine, albumen, and caseine, will act in the same way.—*Annal. der Chem. und Pharm.*, lxi, p. 126.

Again, it does not appear that we can place any reliance upon the characters which are furnished by the lactate of zinc; nor are we even sure that lactic acid may not, under the influence of a putrefying body, be converted into some other compound; indeed, the recent observations of Engelhardt and Maddrell seem to show, that even the crystallized lactate of lime may become converted, when a nitrogenized substance is present, into a butyrate.—*Annal. der Chem. und Pharm.*, lxiii, p. 83.

At the present time, therefore, there really is some considerable doubt hanging over the reactions and properties of lactic acid. In fact, the authors, to whom we have just alluded, have declared, after a careful examination of this subject, that they have sought in vain for some positive character by which they could distinguish the presence of lactic acid.—*Ibid.*

On the Chemical Properties of Saliva. By M. BERNARD.

WITH the view of determining the part which saliva plays in the act of digestion, M. Bernard has made a great number of experiments upon dogs, and he finds that, while the mixed saliva, containing mucus, &c., from the mouth of these animals, has the power of transforming starch into sugar, the saliva taken at once from the parotid, the sublingual, or the submaxillary ducts has not this power. M. Bernard concludes, therefore, that the digestive principle (salivary diastase) results from the action of the saliva on the mucus of the mouth; and that the chemical influence of the salivary secretion is almost *nil* in the act of digestion—that, in fact, it merely serves as an agent to assist in the process of mastication and deglutition.—*Journ. de Pharm. et de Chim.*, 3e série, xi, p. 125.

Existence of Urea in the Vitreous Humour.

M. Millon observes that he has constantly detected the presence of urea in the vitreous humour of the eye; and that, in fact, it constitutes so large a proportion as from 20 to 25 per cent. of the dry residue. He has also detected it in the hyaline membrane, and in the fluid of the anterior chamber of the eye.—*Comptes Rendus*, xxvi, p. 121.

Millon's observations have since been confirmed by Wöhler and Marchand.—*Annalen der Chem. und Pharm.*, lxvi, p. 128.

Non-existence of Urea and Uric Acid in the Juice of Flesh.

M. Liebig states that he has made every effort to determine the presence of urea or of uric acid in the fluid of muscle; and he believes that he should have succeeded in detecting it, even if there had been but a millionth of these substances present. His researches, therefore, exclude urea from the number of the constituent principles of the juice of flesh; once, however, he obtained a body which presented all the characters of uric acid, but he has not succeeded in again obtaining it therefrom.—*Annuaire de Chimie*, 1848, p. 425.

On the Clinical Detection of Iodine.

It may be often of service to determine when certain medicinal substances have penetrated into the system; and as regards iodine, M. Rayer adopts a very simple procedure at La Charité. A strip of starched paper is moistened with the saliva or urine of the patient, and is then touched with nitric acid, with the effect of producing a more or less intense blue. As regards the urine, however, if the proportion of iodine employed has been very slight, the fluid requires to be evaporated to a 15th, 20th, or more of its volume. So treated, M. Rayer has detected iodine in the urine of a patient in whose case a simple iodine injection had been but once thrown into the knee-joint.—*Bulletin de Thérapeutique*, tom. xxxv, p. 410.

PATHOLOGY AND PRACTICAL MEDICINE.

On Pectoral Undulation, or Thoracic Vibration in Health and Disease.

By M. Monneret.

By thoracic vibration is meant the sensation imparted to the hand placed on the chest of a person while speaking aloud or singing. He should pronounce some words in a loud and measured tone, which he will best do by being told to count some numbers. The linen or flannel shirt should be alone left on, and the patient seated while the lateral and posterior portions of the thorax are examined, and laid on his back while the anterior portions are so. The hand is then to be rapidly passed over the different regions of the chest, with a sufficient degree of pressure; and it is essential to remember that these normally furnish different intensities of vibration. Thus, the right side oscillates far more strongly than the left; and the following is the order in which the intensity decreases in the various parts. 1. The larynx and the trachea, as far as the sternum. 2. The four last cervical, and four or five first dorsal vertebræ. 3. The right sub-clavicular and sterno-mammary regions. 4. The same on the left side. 5. The space comprised between the spinous processes of the cervical and dorsal vertebræ and the inner edge of the scapula. 6. The posterior and lateral regions. 7. The sternum and supra-spinal fossæ. 8. The anterior and inferior parts, i. e. the præcordial and hypochondriac regions. The thorax is not the only part which transmits this vibration freely, for it is felt along the whole dorsal and part of the lumbar spine. Its intensity is very great at the cephalic extremity of this, especially the sinciput. The vibration hed if the chest is covered with thick muscles, or if there be deposits of fat or serum, and is very feeble in the mammary region in women. In those who have long, thin chests, or who have fallen into a state of marasmus, it is very perceptible.

Among the diseases in which the *intensity of the vibration is increased, pneumonia* may be first mentioned; and so marked is the increase in this disease, that it becomes an immediate means of distinguishing it from pleurisy. In 50 cases carefully noted, the vibration was always found remarkably increased when the inflammation occupied an entire lobe, or implicated the middle or lower lobes. The undulation indeed was just as great in pneumonia of the upper lobe; but, as in the normal state, the pectoral regions corresponding to this part of the lung vibrate more forcibly than at other parts, it requires more attention and a more accurate comparison of the two sides. Under the clavicle, in the axilla and the supra-spinal fossa, the vibration is often twice as strong as on the sound side. If the pneumonia (or tubercle) occupy the summit of the left lung, as the vibration is here normally more feeble than on the right side, we now find the two sides becoming alike in this respect. After a few weeks' practice in gaining familiarity with the normal differences prevailing at different portions of the thorax, the detection of pneumonia becomes very easy, the vibration increasing in intensity with its progress, and diminishing again as it is relieved.

If, however, there is any coexisting *effusion into the pleura,* the vibration will be proportionally feeble, or even become imperceptible if that increases, becoming thus in such cases a valuable means of diagnosis. So, too, in those cases of *general bronchitis,* when the symptoms are difficult to distinguish from those of pneumonia, the absence of vibration is conclusive. In respect to *children,* also, whose cries render auscultation so difficult, this means becomes of valuable assistance. It likewise often serves for the detection of *passive congestions* occurring in the course of fever and other general disease. In *phthisis,* the vibration also becomes much more intense when considerable masses of tubercles are deposited in the lung, and even a small amount of these suffices to produce an augmentation; and it has several times happened to the author to have the disease thus indicated to him, when all the ordinary signs were absent. As in its progress the tubercle becomes surrounded by denser tissue, the amount of vibration increases, while if excavations are formed, it becomes diminished. Although in *pleurisy, with effusion,* the vibration is *nil,* yet if false membranes are formed, and firm adhesions contracted between the lungs and thorax, it becomes very distinct. When the lung decreases in density, and air becomes interposed, the vibration is diminished (though less than in pleurisy with effusion), as in the case of emphysema; and in large excavations opposite the cavities we may find the vibration diminished, while at their condensed circumference it is increased. In four cases of pneumothorax, the vibration was absent. —*Rev. Méd. Chir.,* tom. iv, pp. 128-39, 195-204.

On the Injection of various Substances into the Veins.

By Drs. Manzolini and Quaglino, of Milan.

The municipality of Milan having placed at the authors' disposal a large number of stray dogs, they took the opportunity of injecting various substances into the veins; as pus of various descriptions, blood abstracted for different diseases, and a variety of putrefying animal substances. They then experimented with a variety of poisonous and medicinal agents, as arsenic, corrosive sublimate, iodine, digitalis, quinine, &c. The following are the principal conclusions:

1. *Pus,* whether syphilitic, variolous, or tubercular, does not induce corresponding specific affections in the organism of brutes, but constantly determines a gastro-enteritis and enlargement of the mesenteric glands. 2. Recent blood, though abstracted from patients with fever, smallpox, &c., exerts no effect on the health of animals. 3. The inorganic irritating poisons exert their action on the alimentary canal, inducing generally a gastro-enteritis. 4. This predilection for the intestinal tube seems to be due to the facility with which it becomes an eliminatory organ,

by reason of its vast superficies and its easy communication with the exterior. 5. The action excited by pus transported into the circulation explains the colliquative diarrhœa and other gastro-enteric disturbances which individuals suffering from large and old suppurating wounds, open cancer, phthisis, empyema, phlebitis, and gangrenous typhus are liable to. 6. Putrefying substances exert the same effects upon the canal as the irritant poisons. 7. Gastro-enteritis is not met with in those animals which die soon after the injection of these bodies. 8. All poisonous substances, organic and inorganic, whether introduced into the circulation by the veins, or by the digestive apparatus, constantly effect an alteration in the crasis of the blood. 9. That these poisons may exert their action upon the nervous system, it is requisite they should be transported into the circulatory current, the affection of the nervous centres being only secondary; in fact, when the stomach has been deprived of the influence of the par vagum by section, the same results are obtained as when this is uninterfered with. 10. *Vegetable poisonous substances* and their alkaloids, however, attack the nervous system in preference to the digestive tube. Some especially affect the brain, as opium; others the cerebellum and medulla spinalis, as the ethers; and others the medulla spinalis alone, as strychnia, quinine, *lolium tomulentum*, &c. 11. Two substances acting upon the same portion of the nervous system, but producing phenomena peculiar to each, when they are given simultaneously, do not confound their actions, but simultaneously manifest their proper phenomena. Thus by giving strychnia to a dog labouring under the tremors produced by *lolium tomulentum*, we engraft upon these the tetanic spasms peculiar to the alkaloid. 12. The venous and arterial tree is always found normal, whatever be the substance injected by the veins or administered by the mouth. 13. The remedies known as the narcotics and narcotico-acrids of authors (as aconite, belladonna) never induce gastro-enteritis, however introduced. 14. Dilatation of the pupil may be induced by remedies which act on the encephalon, as well as by those which act on the medulla spinalis. In the first case, it is due to the torpor which the narcotic substances produce in the nerves of the third pair destined for the motion of the circular fibres of the iris, by which these antagonize the radiated fibres receiving their motor power from the cervical nerves. In the second case, the midriasis depends upon the spasm or excess of excitement of these last nerves.—*Gaz. Medica di Milano*, 1847, No. 46.

Case of Hydrophobia Spontanea. By Dr. Mombert.

Such is the appellation bestowed by Dr. Mombert upon a case recently published by him, of a most interesting character in its details, though unfortunately imperfect, from the absence of a post-mortem examination.

Dr. Mombert was sent for early on the morning of the 24th July to see a lad (æt. 12), who had been indisposed for a day or two before, apparently from having become chilled while hot. He found him with a slow and feeble pulse, pain in the head, contracted pupils, and difficulty of deglutition; his secretions, tongue, and skin being in a normal condition. While quietly talking, he suddenly started from the arms of his parents, struck his head with the rapidity of lightning, screamed aloud, and distorted his eyes and limbs in the strangest manner. In a few minutes all was calm, and he continued his narration. This paroxysm produced little effect upon his pulse, and was repeated with the same vehemence every few minutes, the attacks differing from epilepsy, in the retention of complete consciousness. Sinapisms to the feet, warm clysters, and calomel, were ordered.

Two hours after, the author found the parents vainly engaged in trying to administer a powder in water, the child declaring he could not swallow, and raging frightfully when they attempted to make him do so. He crushed the spoon between his teeth, and the fluid flowed out at the angles of the mouth. The same occurred when water or chamomile tea was offered. He ate an apple, however, thick potato-soup, and the like, with appetite. He declared his willingness to take anything, but that he could not get fluids down, and shuddered at their mere sight.

No affection of the throat existed. The paroxysms of convulsions, screams, and raging continued, so that several strong men could scarce restrain him. As the carotids beat strongly, the pulse had become hard and quick, and the pain in the head was very great, cold was ordered to the head, and a venesection at the foot. But when the patient attempted to place his foot in water, the hydrophobic symptoms redoubled in intensity; and when, by the exertions of four men, he was made to do so, the ghastliness of his features evidenced the anguish he endured. His respiration became short; he could neither swallow nor spit out his saliva; and now commenced biting those about him. For a while after the V. S., the paroxysms seemed less intense, but towards the afternoon, they returned as badly as ever; his propensity to bite those about him, or even himself, increasing. When those who held him slackened their hold, he, in the full possession of consciousness, entreated his friends to keep at a distance, lest he should bite them. No mitigation after this occurred, and he died at three in the afternoon. The most careful external inspection showed no traces of any bite, and his relatives felt certain he had not been bitten; and yet the symptoms were not to be distinguished from those which the author had witnessed in true hydrophobia. All the persons bitten by him continued well.—*Walther and Ammon's Journ.*, Band viii, p. 304.

A Case of Epileptica Peripherica. By Dr. STUMKE.

A CASE is here detailed, which, although occurring in a young woman, seems from the author's description to have been of a truly epileptical, and not hysterical, character. The paroxysms came on every week or fortnight. An unpleasant feeling in the toes, proceeding upwards along the leg, always preceded the attack. After a while, the muscles of the entire limb became convulsively attacked; she then fell down, lost her consciousness, and the ordinary symptoms of an epileptic attack manifested themselves, a deep sleep terminating the fit. The peculiar kind of *aura* having directed the author's attention to the foot, he found a *clavus* there in an inflamed and painful condition. The patient was confined to her bed, and by various applications the corn was in a few days softened and removed; and from that period to the present (more than three years), the attacks, which had latterly occurred nearly daily, have never returned.—*Casper's Wochens.* No. 35.

On Muscular Rheumatism. By M. VALLEIX.

THE essential character of this affection is *pain*, and no anatomical lesion belongs to it, unless it become complicated with other affections. It possesses various analogies with neuralgia, and the two affections may be easily transformed into each other. If the pain remain concentrated in the nerves, we find isolated, characteristic, painful spots—a *neuralgia*, properly so called. If it spread to the muscles, the contractions of these are especially painful, and *muscular rheumatism* is present; while if it extend to the skin, we have a *dermalgia*. All these forms of the same affection may unite together, or by two and two.

The *diagnosis* of *acute* muscular rheumatism from inflammation is generally easy, owing to the absence of the well-known signs of the latter. It becomes more difficult when the muscles over a joint are those affected; and on no account can the identity of muscular and articular rheumatism be admitted. In respect to its diagnosis from neuralgia, it may be observed that it is much more rare to mistake a rheumatism for a neuralgia, than the reverse. In rheumatism, the pain and tenderness are more diffused, and are found rather at the attachments of muscles than in the course of the nerves. Muscular action causes an amount of suffering altogether disproportionate to the other spontaneous or excited pains; while in neuralgia the reverse is generally observable.

Prognosis. Chronic muscular rheumatism is more obstinate than chronic neuralgia; while exactly the reverse is true of the *acute* form. However severe, it is rare to see it continue longer than a week; while neuralgia may persist, with all its original severity, for weeks or months. Rheumatism is generally a far less serious affection than neuralgia; it does not produce the same perturbation of the economy, and is more easily dissipated. The same is true of the *chronic* form, for we only now and then see paralysis of one or several muscles result; while the subject of neuralgia not unfrequently continues to lead a miserable existence, deprived of the use of his limbs.

Treatment. These two diseases have too often been treated alike, and with great mischief to the subjects of them; for while bleeding may benefit rheumatism, it is very hurtful to neuralgia; and flying blisters and intercurrent cauterization, so useful in acute neuralgia, are of little avail in rheumatism. In *chronic* cases, hydrotherapeia, shampooing, and thermal waters are more useful in rheumatism than in neuralgia: but in this stage the treatment of the two diseases approximates much more than in the acute one.

Among the different species of muscular rheumatism may be noticed that which affects the *muscles of the head;* sometimes the occipito-temporal, the masseters, or temporals, and at others the muscles of the eyes or cheeks being those which suffer. This pain is distinguished chiefly by the exacerbation which is produced by causing the sudden movement of the muscles affected; furnishing a far stronger contrast with the spontaneous pain than is the case with other pains of the head, as well as by being limited to the muscles in question. This form best yields to the external application of the *cyanide of potassium;* and of all forms of cephalalgia it is that which is least benefited by blistering the nape, so indiscriminately resorted to. This rheumatism of the head is found also generally worst on rising in the morning, especially in *moderately* cold and damp weather. This is attributable to the parts having been exposed to the cold during the night (from which they would have been protected in *very* cold weather), and the simple precaution of covering them with a cap or handkerchief has not unfrequently relieved pains which have long obstinately persisted.

Besides a *torticollis,* rheumatism may show itself in the cervical region in the form termed by the author *cervicodynia,* in which there is a very variable amount of dull pain felt along the cervical region, and even extending to the epicranial muscles. It is much aggravated by stretching the head backwards, or keeping it bent forwards. It is easily transformed into a neuralgia, and may become confounded with this, as it may with congestion of the brain, when it extends to the pericranial muscles. The most successful means are, sea-water baths, cold affusion, cupping, and acupuncture. When the affection becomes chronic, it is very obstinate.

One of the most obstinate forms of muscular rheumatism, as also one of the most important, as it may lead to *paralysis of the deltoid,* is *rheumatism of the muscles of the shoulder.* In its acute form, it is distinguished with difficulty from articular rheumatism or acute arthritis. In its chronic form, it sometimes induces the above-named paralysis. Several cases are related by authors, in which paralysis supervened on painful affections of the shoulder; but these are, for the most part, too imperfectly reported to enable us to judge whether this arose from chronic muscular rheumatism or inflammation. Others are more explicitly detailed, and two of these are quoted by the author, in which acupuncture, after the failure of other remedies, effected a cure.—*Bulletin de Thérapeutique,* tom. xxxv, pp. 296-307, 385-96.

Case of Rheumatic Blindness. By Dr. CRAMER.

THE winter of 1847 was both long and severe, and during its course there occurred to the author several nervous affections, which seemed only due to exposure to cold, such as facial neuralgia, spinal irritation, St. Vitus's dance, ischiatica, &c. &c. In all these there had been exposure either to sudden change of temperature, or to the prolonged action of cold; and recovery took place on restoring the activity of the functions of the skin. But by far the most interesting case occurred in the person of a citizen's wife, who had suffered from cold feet during a ride in an open carriage. Previously in excellent health, to her utter alarm, she found herself totally blind, and so she had remained, in spite of all that was done for her, for eight days, when the author was called to her. He found a handsome woman of 34, having a florid complexion, and clear blue eyes, which, however, had assumed the lack-lustre appearance of the "poor stone blind." The pupils were dilated and immoveable, but otherwise the eyes appeared normal. Her sight was *absolutely* gone, but in other respects she was quite well. The author prescribed Dover's powder, the drinking abundantly of elder-tea, and the excitement of a strong counter-irritation, by means of antimony, at the nape. After eight days she began to perceive light, and in six days more saw as well as ever.—*Casper's Wochenschrift*, No. 39.

On Rachitis. By M. TROUSSEAU.

IT is a remarkable circumstance that rachitis seems to be a comparatively modern affection, it having first appeared in England during the 17th century. So complete is the practical portion of Glisson's work on this disease, that M. Trousseau, after bestowing great pains in accumulating and arranging facts respecting it, was surprised at finding nearly every important point anticipated. The affection is never *congenital;* and although some commencement of the deformity may occur as early as the 3d or 4th month, it does not usually show itself until the 10th or 12th, augmenting most during the second year. It is rare indeed for the disease to commence after the 2d or 3d, and especially the 4th year. Parents seldom perceive the symptoms until they have considerably advanced. The child is then dull and heavy, breathes with difficulty, and suffers pain when taken up; and on examining the chest, the flattening of its sides and the projection of the sternum are obvious. A remarkable excavation exists opposite the 5th, 6th, and 7th ribs. The articulations of the ribs with the projecting sternum give rise to so many projecting points, and the same may be seen at the dorsal articulation. The clavicle is carried strongly forwards, projecting where it joins the sternum. The spine becomes bent upon itself as in old age, but not distorted laterally, and the vertebræ are enlarged as if they had been submitted to compression when soft. The anterior fontanelle remains open for two, three, or four years, though it should be closed by the 14th or 20th month; its texture remaining cartilaginous as late as six years, whereas, at latest, it should be bony by the second year. In like manner, the sutures continue ununited for a period far too long. The head enlarges in all directions; the forehead resembles that of the hydrocephalic head; the chin is short, and the jaws, especially the lower, are swollen. Almost invariably the teeth are very backward, or if present, they are ill formed, carious, and brittle. In respect to the pelvis, the iliac portion widens out, while the ischiatic narrows, contracting the cavity. The humerus and femur are especially shortened, and this occurs to some extent in all the other bones, the longest ones generally shortening most. The bones of the forearm are curved with the concavity towards the palmar aspect. The humerus is curved inwards, and the bones of the lower extremities forwards and inwards. The heads of the bones are enlarged, and their ligaments relaxed, allowing movements of the hands and feet without the intervention of the forearm or leg.

The first stage of the pathological change is termed by Guerin *ramollissement*—the whole texture of the bone becoming swollen, and the intervals of its lamellæ being filled with a substance of the consistence of currant jelly, which also fills the medullary canals of the expanded bone. The periosteum swells and becomes incrusted with osseous matter as after fracture. If these processes go on, an amorphous osseous matter becomes deposited, which gives to the bone a remarkable softness, so as to render it capable of receiving an impression as in œdema, or of being bent by the hands. However, the osseous matter becomes more and more deposited, and after a while hardens the bone and acts curatively. In from four to six years this hardening becomes so great, that the term *eburnation* may be then correctly applied, although used by Guerin at an earlier period. Very slight force, however, suffices to fracture such a bone.

Children labouring under rickets suffer much pain, and when the disease is advanced, great agony, on the least movement. The first symptom that strikes the mother is the great debility of the child, perhaps heretofore apparently strong. It suffers much when attempting to stand alone, and bends forward like an old man. If we find a child so suffering, and that it has a large head, fever, and sweats which persist for weeks or months, the disease will surely prove to be rickets. Too often both the mother and physician are induced by the cough and dyspnœa, which in so deformed a chest are present, to mistake the disease for a catarrhal affection. So, too, the large belly produced by the thrusting down of the enlarged liver by the contents of the thorax, leads to the supposition of mesenteric disease. Rickets seem quite incompatible with *tubercle*, as the two diseases are never found associated, and the same remark applies to *scrofula*. Almost all the children who die from rickets do so on account of the development of acute or chronic *pneumonia*. In other cases diarrhœa takes them off; but they never die from affection of the head.

One is struck by the fact that most cases of rickets occur between the 10th and 15th months, which is just the period of *dentition;* but the supposition that it arises from the febrile action dependent on that process will not bear examination. These children generally have not yet got any teeth. This is also the usual period of *weaning*, and rickets is of very common occurrence in children who have been prematurely weaned; and M. Guerin's experiments show the great influence exerted by improper diet in its production. In those whose diet is too animalized, it is developed sometimes with wonderful rapidity; and we have to insist on prolonged suckling or the use of milk, whereas practitioners too often order broths, meat, &c., on account of the weakness of the child; in all such children, *milk* is the proper diet for the first three years, the good diet, tonics, &c., given advantageously in scrofula being quite unadapted to this affection. Yet *cod-liver oil*, so useful in scrofula and numerous chronic diseases of debility, is of marvellous efficacy here; the common shoemaker's oil being, however, just as useful as the more expensive preparations. It first produces a cessation of pain, and, if continued, a cure rapidly follows.—*Gaz. des Hôp.*, Nos. 37, 41, 46, 66.

On Bilious Pneumonia. By M. MARTIN SOLON.

DR. SOLON relates two cases confirmatory of the existence of this form of pneumonia, formerly so generally, and now so rarely admitted; and he believes that the epidemic bilious pneumonias and pleurisies of the older writers were correctly described as such, just as sporadic cases may be at the present time. A chief circumstance that has prevented the recognition of bilious pneumonia, is the fact of the occasional coexistence of a pneumonia with inflammation of the liver or biliary canals. It is true we have here biliary symptoms, such as jaundice, &c.; but these depend upon the hepatitis, and yield to the antiphlogistic treatment which is as proper for it as for the pneumonia. Here the symptoms of the two diseases are found united, and with the burning skin we have the full hard pulse, and red, dry, cracked tongue; while the green colour of the serum of the blood, developed by nitric acid, diminishes in proportion as the phlegmasia is relieved. But the case is different when the biliary affection is a simple secretory modification, whose influence, without changing the physical signs of the pneumonia, gives to the general condition, and to the progress of the disease a peculiar character—the pneumonia now resisting continued antiphlogistics, and yielding to evacuants. In one of the cases here related, the pneumonia which had resisted two venesections, yielded after free bilious stools were procured by purgation—the amount of biliverdine of the serum of the blood (obtained by a local bleeding) being also diminished. In the second case, the elements of the bile were found not only in the serum, but also in the urine and the expectoration. The severity of the disease did not diminish after the loss of 60 oz. of blood, the tinge of the serum becoming also deeper and deeper after each venesection; and real amelioration only took place after copious bilious stools had been procured, and the serum of a small exploratory bloodletting then furnished no further traces of bile. Cases like these should lead us to admit the truth of Stoll's description of bilious epidemics; and that he had no preconceived ideas upon the subject, is shown by the fact that he knew not at first what treatment to pursue.

Nothing can be more easy than the detection of the biliverdine in the serum. If we drop 10 or 12 drops of nitric acid into two or three spoonfuls, the colouring matter will be found to dispose itself in different coloured zones, one over the other. At the bottom of the glass is seen the yellow colour which all animal matter assumes on combining with the acid: a little above this we see a rose-coloured zone, and a little higher still, other zones of different shades of bluish-green; and finally a zone of more or less deep green. The same zones thus disposed rainbow-wise, may also be seen in the urine: but as respects the expectoration, nothing but the green is developed.

M. Solon remarks that tartar emetic employed in pneumonia, sometimes cures by its contra-stimulant power without inducing vomiting; but at other times it does so by exciting bilious evacuations.—*Bulletin de Thérapeutique,* tom. xxxv, pp. 5-12.

On the Cholera of Infants. By M. TROUSSEAU.

IT is common for children at the period of weaning to suffer from diarrhœa. This from being moderate may become unrestrainable, and may persist from a week to two months; but this attracts attention and may yield to proper treatment. There is a form, however, which seems simple and without danger, when suddenly the child becomes frightfully changed and emaciated. Yesterday every sign of health was present, and to-day we behold hollow eyes surrounded by a blue circle, cold face, icy extremities, and the same absence of elasticity of the skin seen in adult cholera. The cheeks are pale or violet-coloured, according as the child had little or much colour before, and the eyes are deeply sunk and wide open, the cornea becoming dry from the immoveability of the eyelids. The skin is cold to the touch, and the mucous membranes as much so as those of a dead body. The belly is sunk in, and its skin capable of being raised up in plaits, the white mark from pressure remaining for some minutes. Pulse not to be felt. Respiration slow, difficult, and deep, but without dyspnœa. Vomiting incessant. The diarrhœa, at first lienteric, afterwards resembles the green water in which vegetables have been boiled, and is sometimes so limpid as to be mistaken for urine, which in fact is suppressed. The cries and the stiffness of the child's limbs render it probable that cramps are present.

So rapid is the progress of the disease, that you may fail to recognise a child at two o'clock whom you left without any anxiety at ten. Its duration is generally from eight to thirty hours, rarely more than two days. We must not regulate our prognosis by the mere number and abundance of the stools: for there is more than that to be taken into consideration, mere diarrhœa, however violent, never producing these effects. The nervous system is especially suffering, and the action of the heart is almost annihilated, and a stimulant treatment is imperative. For this purpose Dr. Trousseau employs the mustard bath. Having mixed some mustard with cold water, which best developes its essential oil, you place it in a strong napkin, and squeeze it into the bath (heated to 95°) until the water becomes yellow. In three minutes the child screams out; but you leave him in for ten or twelve minutes, as long as the mother's hands who hold him can bear the smarting, and then wrap him up in flannel. An almost immediate effect is produced upon the circulation, and you repeat the bath two or three times a day until the tonicity of the skin and fulness of pulse are quite restored—which is generally the case in four or five days. *Ipecacuanha* is the internal medicine to be resorted to. Unable to explain why it is so, M. Trousseau has repeatedly observed that the temporary increase of the vomiting and stools by its use is followed almost at once by reaction. To assist this reaction he gives a teaspoonful of the following mixture every half hour or hour. Aq. dest. 30 parts; syrup of Ether; syr. Aur. āā 10 parts—discontinuing it as soon as reaction is complete, or if ill effects of an opposite character appear. He never employs any but diffusible stimuli whose action soon passes off, while that of alcohol is much more persistent.—*Gazette des Hôpitaux*, No. 8.

Dr. Pickford (*Zeitschrift für Rat. Med.*, Band vii, 25,) having been induced by the recommendation of Dewees to try the efficacy of a weak infusion of *coffee* in the cholera of infants, reports most favorably of the results of his experiments. He gave a spoonful every hour, of an infusion made by adding three ounces of water to a scruple of coffee. We think his researches can scarcely have referred to the formidable cases alluded to above by Dr. Trousseau, who expressly states that the mere arresting the diarrhœa is of no avail, or even adds to the danger of the child. The resemblance of the symptoms described by Dr. Trousseau to those of Asiatic cholera, is extremely striking.

SURGERY.

On Operating early for Hare-lip.

Dr. Mason Warren has recently published a paper confirmatory of a recommendation he had formerly given, that infants should undergo this operation at as early an age as possible, he having frequently resorted to it 24 hours after birth, and with better success than in older children. This arises from the less resistance offered by the child, and the great rapidity of the healing process at that age, enabling it to suckle almost as soon as if nothing abnormal had been present.

In *double hare-lip*, complicated with fissure of the bones and a projecting tubercle, he operates on one side first, and allows that to heal; for if both sides be operated on at once, the tissues are too much stretched, and suppuration occurs. If one side has united, and a month be allowed to elapse before the second operation, the protuberant intermaxillary bone will be found to have become more or less drawn into its place. *Sutures* are very preferable to needles, however wide the separation may be; for they can be more easily introduced, cause less irritation, and can be removed in from 48 to 72 hours without disturbing the tender adhesions. They allow the part to be inspected, and any excess of inflammation to be kept down by wet compresses; so that after their removal, the line of adhesion is often free from redness, and after a short time is hardly perceptible. The suture-needles are most conveniently passed when straight, and sometimes by seizing them firmly with a forceps.—*American Journal Med. Science*, No. xxx, pp. 337-8.

Dr. Anselon states that a long experience has convinced him that the practice of immediate operation, put into force by M. Bonfils of Nancy, is the best; the child then sleeping much, wanting little nourishment, possessing only an imperfect sensibility, and offering less resistance. The longer the operation is delayed, the less perfect is the adaptation obtainable, for the two segments are never developed exactly alike. The imperfect sensibility of the child is so far from favouring, as stated, the occurrence of convulsions, that these are of far more frequent occurrence in older children. The child may easily be nourished for the first three or four days with a teaspoon, and after then it will suck with ease and safety. Much disappointment in this operation results from neglecting to divide the adhesions of the lip to the gum, without which exact coaptation cannot take place. After the operation, constant surveillance of the child by two attendants, during 72 hours, is requisite. Each of these, in turn, constantly maintains the parts in exact apposition, by gentle pressure of finger and thumb, for in this way alone can the consequences of the movements of the face be guarded against.—*L'Union Médicale*, No. 76.

M. Guersant observes that there are three periods at which this operation may be performed with different chances of success. The best chance is offered when it is performed within the first fifteen days. Later, we succeed less often, owing to the indocility of the child; its crying, eating, &c., preventing also accurate union. Later still, when the child has reached from 10 to 15 years, we may reason with him, and again operate with more success.—*Gazette des Hôpitaux*, No. 75.

In addition to the above, we may observe that M. Paul Dubois likewise, some time since, expressed a strong opinion in the Académie in favour of operating, in simple cases, upon very young infants. He uses very fine insect pins, and as those usually found in the shops are too long, and bend before the tissues, thus increasing the pain and duration of the operation, they should be shortened before passing. After 24 hours, the first threads are to be replaced by others less tightly drawn, such change being repeated daily, and much diminishing the inconvenience produced by the pins. The upper pin may be removed after the 72d hour, and the lower one from the 80th to the 96th, according to the solidity of the union, which should then be found complete. The children are suckled as usual after the operation, which M. Dubois regards as important for their welfare, and preventive of cries and struggles. He has never met with hemorrhage after the operation, and he believes the best security against this, is the bringing the pared surfaces into accurate contact, and the avoiding making incisions into any other part than the lip itself. The detaching from the gum the portion of the lip which is nearest to the upper angle of the wound, for the purpose of rendering approximation easier, is in his opinion unnecessary, as the naturally yielding character of the part allows of this being effected.

On the Employment of Aqua Saturni in Strangulated Hernia.
By Dr. Neuhold.

The object of this communication is to recommend, by additional examples of its utility, the practice instituted by Neuberg and Seitl, of administering enemata of the *aqua saturni* for the relief of strangulated hernia. The author states his firm belief that, were this practice more known and followed, operations would be of very rare occurrence. The injections have been employed by him under very different circumstances, with the same success; and he has never known any harm result from them, even when from 4 to 6 clysters, each containing 10 grs. of the acetate of lead, have been thrown up. He furnishes some particulars concerning four cases. In one an excessively large scrotal hernia resisted the taxis and all other means for 28 hours, and, the patient refusing to consent to an operation, 10 grs. of the acetate of lead were dissolved in 6 oz. of water, and thrown up lukewarm, the same being ordered to be repeated every two hours. The pulse, which had been small and contracted, soon became more developed, and, in fact, from hour to hour the general condition improved, and when the man fell asleep, the bowel spontaneously returned. In another case of inguinal hernia, the author was not called in until strangulation had continued for three days, and given rise to the worst symptoms. The patient's condition improved much after the use of the clyster, and the rupture was easily returned by the taxis.—*Oesterr. Med. Wochenschrift*, 1848, No. 25.

On Gunshot Wounds.

The discussion at the Académie was wound up by MM. Jobert and Bégin, the one representing the civil, the other the military practitioners, each being of high authority. They espoused opposite sides in the various contested points. M. Jobert objects to *immediate extraction* of foreign bodies as a needless and dangerous procedure. After the tissues are disengorged, it becomes an easy matter to remove a ball, which in their engorged state could not even be felt. He equally objects to the *dilatation of wounds*. In respect to *amputation*, he feels little disposed to remove a limb for a mere comminuted fracture; and he has succeeded in saving limbs under the following circumstances, after amputation had been proposed. 1. Penetrating wounds, with fracture of the elbow-joint. 2. Comminuted fracture of the head of the humerus without destruction of the soft parts. 3. Fractures of the wrist and hand. 4. Comminuted fractures of the thigh without disorganization of the soft parts. 5. Penetrating wounds of the knee-joint with or without fracture. 6. Comminuted fracture of the leg, when the soft parts are not too much torn, bruised, or destroyed, notwithstanding there may be several wounds and fragments of bone. He employs large bleedings and cold applications in the inflammatory stage, and warm ones in the suppurative stage, never extracting the fragments of bone, or meddling with the apertures through which they have to pass, and making incisions only when the parts are engorged from inflammatory action. When amputation is unavoidable, M. Jobert prefers immediate to consecutive amputation, understanding by immediate, as soon as the nervous shock and commotion have subsided. Thus it is deferred until the next day or the day after, but still performed before inflammatory action is set up.

M. Bégin, on the other hand, approves of the preventive *débridement* or dilatation of wounds, and he thinks it is necessary in gunshot wounds, simply because they are gunshot wounds, at least in all such as are deeply-seated, or are covered by strong aponeuroses. "The most hostile adversary to *débridement* was John Hunter, whose memory is justly venerated by English surgeons: but I must at once say I do not know a more dangerous book, or one more full of capital errors, than that of Hunter on 'Gunshot Wounds.' We can only explain the aberrations of so eminent a genius by the paucity of his personal experience in these cases. He was, in fact, only momentarily with the army at the siege of Belleisle, in 1761, and set off for Portugal in 1762, where he was peaceably employed in studying the digestion of lizards. It was not until thirty years after that he composed his work upon data so distant and so imperfect." Ample experience has convinced M. Bégin that a methodical *débridement* of the fibrous coverings made at once, prevents many of the ill consequences which result so commonly from deep-seated gunshot wounds. He considers, too, that the popular belief in the importance of *removing balls and foreign bodies* is well founded; for although these are sometimes harmless, who can say that in a given case they will prove so?—and when surgeons point to such cases, they forget the numerous examples of the ill consequences arising from the neglect to remove them. Not only would he remove all loose bodies, but even fragments of bone that are still adherent, provided that they are moveable; for these never recover their vitality, or become reunited, except by new osseous matter, amidst which they remain as foreign bodies.

M. Bégin regards the indiscriminate use of *very cold applications* as mischievous. If the integument is entire, they are often highly useful, but where there is a wound there must be suppuration, and intense cold exerts an unfavorable effect; for if it diminish the pain, it does not prevent the other consequences of inflammation. It masks the symptoms, and under such mask the tissues lose their caloric, and become indurated; the circulation is obstructed, and yet, after a variable period, we still have the suppuration with its usual consequences. Water at the ordinary temperature, and applied over the bandages according to the amount of heat generated, and the feelings of the patient, is the best application. Dressings of the wounds are too much multiplied, and an excess of care does more harm than good by disturbing the wounds. During the retreat of the French armies, the wounds which were apparently neglected throve excellently. The skilful surgeon need not uncover a wound to know how it is going on. He judges from the *general* state of his patient. If there is headache, fever, sleeplessness, and the bandage has become tense, mischief is going on; but as long as all remains both locally and generally quiet, no interference with the dressings is called for.

M. Bégin does not believe that *consecutive hemorrhage* usually arises from the divided ends of the vessels, but from a diseased action set up in the vessels which have been injured, without being opened by balls. The opening now takes place laterally, and both ends of the vessel should be tied.

The experience of the army is highly in favour of *immediate amputation;* and as regards the great number of amputations army surgeons are accused of performing, these are required in a larger proportion after gunshot wounds than after other accidents—no others producing such general, extensive, and complicated accidents. All army surgeons commence their career with the earnest desire of attempting to save limbs; but just in proportion to the experience they acquire, do they find themselves compelled to operate more frequently. The limbs which have been preserved, through the refusal of the patients to undergo amputation, ill compensate for the great loss of life due to such refusal. And even of those persons whose limbs have been thus saved, how many there are to whom they prove a true burden, may be judged by the constant applications made to the surgeons of the Invalides for the removal of them.—*Bulletin de l'Académie*, tom. xiv, pp. 64-112.

MIDWIFERY, &c.

On the Dropsical Affections of Pregnant Women. By M. DEVILLIERS.

M. DEVILLIERS considers that these affections may be most advantageously considered under the following heads: 1. Simple œdema or anasarca. 2. Œdema complicated with affections of the circulatory or respiratory organs. 3. Œdema or anasarca with albuminuria. As a preliminary he inquires *in what way do the modifications of the economy induced by pregnancy, influence the production or progress of dropsies?* In this point of view the analysis of the blood is of great importance; and the author found that 25 analyses furnished 67·7 per 1000 of albumen, the normal mean of non-pregnant women being 70; but while in the first seven months the average was 68·6, it was in the last two only 66·4. In those cases in which albuminuria was present, the proportion during the last two months was only 56·39. The researches of Andral have amply shown the coincidence between the diminution of albumen in the blood, and the formation of dropsy; and those of MM. Becquerel and Rodier have established the same in reference to the solid parts of the serum.

1. *Simple œdema or anasarca.* The author believes that the state of repletion of the vascular system is in a far less degree a cause of the effusion, than is the altered composition of the blood. He has not found age or the primiparous condition exert any notable influence in its production, but it seems to occur most easily in the lymphatic temperament. An important influence of pregnancy has been overlooked, viz. that the fact of the increased degree of uterine activity displaces or diverts the blood, and must produce *stasis* and other modifications in the lower extremities. The influence of the pressure of the developing organ is well known, and its inclination will often determine which limb shall be most œdematous. The amount and persistence of the œdema are, however, far from being always proportionate to the degree of development of the uterus. In about half the cases the œdema becomes developed especially between the seventh and ninth months, it is not unfrequent in the fourth or fifth, and is even met with in the third. Although occasionally the œdema is developed in a week or two, it usually requires several, and can never be called active. Once formed, it may undergo the most strange variations, probably dependent on mechanical causes. In a little more than one sixth of the author's cases, the engagement of the fœtal head in the cavity of the pelvis, at the end of pregnancy, was attended with partial or complete disappearance of the œdema. The *urine* in these patients is sometimes normal, sometimes sparing, deep-coloured, or turbid, *but does not contain albumen.*

Varicose veins should rather be regarded as a concomitant than a complication, depending upon the same causes as the œdema, and always preceding it. They were absent in three fifths of the cases. They often diminish on the appearance of the œdema. In simple œdema the splanchnic cavities contain no fluid, but dropsy of the amnios is sometimes present. In respect to the question whether the existence of œdema at all influences the occurrence of *puerperal affections*, the author observes that in about one fourth of his cases *metro-peritonitis* occurred; and so intimately did the disappearance of the one and the appearance of the other seem to be connected, that he was at first disposed to regard metastasis as the best explanation. Further experience showed him examples of the persistence of the œdema; and he believes that the cases of the occurrence of serous inflammation on the disappearance of the œdema, narrated by authors, were examples of dropsy accompanied with organic alterations or albuminuria. In the *treatment* of this affection, when slight, position, aperients, and mild diuretics suffice. If the œdema is great, and the woman plethoric, venesection may be resorted to. Vapour-baths and gentle friction are very useful; but compression is useless or mischievous. Iron, in certain subjects, is of great utility; but its administration must be watched, and coincident small bleedings be put into force if congestive symptoms present themselves.

2. *Dropsy with affections of the central organs of circulation and respiration.* In these cases organic causes co-operate with those already enumerated, to render the effusion more copious and more important. When the organic disease is but slight, and the dropsy proceeding from it has not preceded pregnancy, the œdema may appear only at the ordinary period, be confined to the lower extremities, and alternate in amount, as simple œdema. Generally, however, it is much more persistent, and has a great tendency to invade the splanchnic cavities, where, indeed, it may commence. When this is the case the birth is generally premature, although sometimes women labouring under dyspnœa from this cause will go their full time, and bring forth strong infants. In treating this affection, when the means advised for severe simple œdema, with punctures of the dropsical parts, or paracentesis abdominis, have failed to give relief, if danger menaces the patient, and the child is viable, we should have recourse to the induction of premature labour.

3. *Dropsy with albuminuria.* Almost all the cases which have occurred to the author were of the lymphatic associated with the nervous temperaments, very few of the sanguineous. In several cases the women had been in good or even robust health. The primiparous condition is a powerful predisponent. The author has not been able to trace the influence of damp, cold, or spirit-drinking, said to have so powerful an influence in inducing ordinary albuminuria. Most of his patients had suffered from mental disquietude. He does not agree with those who attribute any effect to the pressure of the uterus, which is only competent to the production of ordinary œdema.

Symptoms. It is to be observed that albuminuria may occur *unaccompanied by any dropsy*, especially when it takes place not long before delivery. When present, the development of the dropsy is sometimes rapid and acute, but generally gradual. It is sometimes even limited to the lower extremeties, but usually extends to the upper extremities and face, the precipitate of albumen being just as abundant in the one case as the other. The swelling may vary from mere puffiness to excessive distension, but, like the dropsy from organic causes, it is less liable to variation than simple œdema, and it very readily invades the splanchnic cavities. Whatever may have been the amount of infiltration, or the issue of the disease, the swelling usually disappears in from six to eight days after delivery. The accoucheur has rarely the opportunity of determining when the *albuminuria* first manifested itself. The period indeed seems to be variable, and by no means in relation to the amount of serous infiltration. Nor does the quantity existing in the urine bear any such relation, it being, however, generally found considerable at the approach of delivery, augmenting sensibly during labour, and even persisting awhile after confinement—gradually, however, save in case of serious complications, diminishing until the tenth or twelfth day, when it disappears. If during pregnancy or after delivery any notable febrile disturbance of the circulation occurs, the albumen becomes proportionably augmented; and the author furnishes a curious case, in which the amount fluctuated with the paroxysms of intermittent febrile action. The same increase is observed during severe labour, at the commencement of puerperal fever or other dangerous diseases, and especially at the approach of death—and that in women who but the day before had exhibited a very small quantity. Other accidents operate critically, and give rise to the sudden disappearance of the albumen;

an instance of which is given, that occurred on the development of an abundant hepatic eruption. In scarcely any of the patients do distinct febrile paroyxsms exist, and the various nervous symptoms, analogous to those of chlorosis, are often met with in pregnancy unaccompanied by albuminuria. In some cases, severe epigastric pain seemed a marked symptom.

Complications. Examples are given of the complication of this affection with effusions into the various serous cavities; but the most important accompanying disease is that first pointed out by Lever, *puerperal convulsion.* It is well known that some eclamptic women suffer little from anasarca, and others not at all; but it is an essential fact, that *in all cases albumen is found in their urine.* All women suffering from albuminuria do not, however, become the subjects of eclampsia; for of the twenty cases here related, only nine suffered from convulsions; and according to the author's researches, neither the amount nor seat of the infiltration enables us to predict their occurrence, although the appearance of albumen in the urine may always give rise to fears of this. An important conclusion to be drawn from this relation of albuminuria and eclampsia, is that if the first is the source of the second, we must consider as only occasional causes of the convulsions, the distension of the uterus, the amount of uterine pain, and the various moral and physical sources of irritation regarded by various authors as primary ones. Other forms of convulsion, as *hysteria,* are not attended with albuminuria.

Terminations. Contrary to what is usually the case, the albuminuria of pregnancy sometimes terminates in a rapid and spontaneous cure, notwithstanding its apparent gravity. It may also pass into the chronic state; but the serious complications which attend it, and the accidents produced by it, are too often attended by death.

Prognosis. M. Rayer observes that the albuminuria of pregnancy is comparable to that which is produced by a temporary hyperæmia of the kidney, and is less dangerous and more curable than other forms of the disease. Yet cannot this be too absolutely received, for women sometimes die, when the lesions after death are found to be very slight. Nor is Lever's statement to be relied upon, that the albuminuria of pregnancy gives rise to more violent and frequent convulsions, and is more persistent, than when it appears only just at labour. There is, in fact, no relation between the date of albuminuria, and the violence of its consequences. A pretty certain ground of prognosis is derivable from the diminution or increase of albumen, the persistence of this giving rise to a very unfavorable conclusion, which cannot be drawn from the mere amount of infiltration. In respect to the production of *abortion,* the cases of albuminuria may be divided into those occurring *without,* and those occurring *with complications.* Thus of 37 cases in the first category, in only five could abortion be attributed to the albuminuria, whereas in 12 cases of eclampsia, premature labour occurred in six. The author states, as the result of his own and M. Dubois' experience, that the supposition entertained by some, that there is a connexion between the albuminuria of the mother, and induration of the cellular tissue of the infant, is destitute of foundation.

Pathological anatomy. The author was struck with the numerous instances of disease of the liver occurring in these cases. In some of the cases examined, the kidney had undergone the changes observed in Bright's disease, but in others these were not observed, the organ being in a congested state, and almost always increased in size. While in ordinary albuminuria we estimate the amount and gravity of the renal changes by the amount of the albumen, and the extent of the dropsy, that is not the case in the present form. The only point in which the two diseases have a strict similitude, is the altered composition of the blood. This disease, in fact, much more nearly resembles the albuminuria occurring after scarlatina, or that dependent upon temporary renal hyperæmia, and seems limited to the period of pregnancy, which is its principal cause. This relation of cause and effect, however, is not local or mechanical, as stated by M. Rayer, but is due to a general modification of the economy produced by an altered condition of the blood.

Treatment. Our resources are limited by the obscurity of the indications, and the desire to avoid interfering with the progress of pregnancy. General bleeding may be sometimes resorted to, but strong purgatives seldom should, while diuretics and vapour-baths are useful. When the albuminuria is recognised, we should seek to modify certain of the conditions of the economy by ferruginous preparations. As to the *induction of premature labour*, with the view of *preventing* convulsions, the author is averse to it; for it is impossible to be certain that these will occur, and the necessary manœuvres may themselves give rise to complications that would prove dangerous to mother or child. Threatened suffocation from the extension of the swelling would alone, in his opinion, justify it.—*Archives Générales*, t. xvi, 145-62, 318-32; t. xvii, 48-69, 287-317.

M. Miquele, after relating several cases, lays down the following rules in reference to the treatment of the albuminuria of pregnancy:—1. That the anasarca with albuminuria of pregnant women is not due to a mechanical obstacle, but to an affection of the kidney, which is shown by the early period at which it may occur; 2, diuretics and other stimuli are, to say the least, useless; 3, much trust is not to be placed in bleeding, which is to be used only as an auxiliary when signs of congestion exist; 4, the most powerful means we have of overcoming or preventing this disease, is by an abstemious or vegetable regimen; while a too animalized diet, or any which much disturbs the digestive functions, and irritates the urinary organs, produces or increases it; 5, when convulsions occur, opium, given in enemata, in spite of the existing congestion, is very useful; 6, large flying blisters, applied to the groins, are so likewise.—*Revue Médico-Chirurgicale*, iii, 191-200.

Anomalies of the Menstrual Functions in relation to Fecundation. By M. PAUL DUBOIS.

M. PAUL DUBOIS has recently illustrated some of these at his Clinique by several cases. Two young women commenced menstruating at 11 and 12, and were confined at 13 and 14½. Such cases of premature nubility, coinciding with a precocious condition of the rest of the body, are by no means rare; and they may be contrasted with certain examples of tardy menstruation, which give rise to very embarrassing questions. In one of these the woman was not regular until 21; and in a similar case, in which M. Dubois, sen., was consulted, he, having found on examination the genital organs normal, sanctioned marriage; but the person never became pregnant. Still, there are cases on record in which women have become mothers without menstruating; and M. Dubois relates one in which menstruation first occurred after the eighth confinement. Again, aged women have become pregnant after ceasing to menstruate. A woman æt. 21, now pregnant, was regular at 12, and has never been so since. In civil practice a cessation of the menses is considered a principal sign of pregnancy; but in legal medicine it is of little account, for it is well known how subject this flux is to arrest by many moral and physical circumstances. In many newly-married women the menses become suppressed for awhile, and then return in augmented quantity, giving rise to the belief in the occurrence of a miscarriage. But for this there has been no foundation, the suppression arising from change of habits on the part of the woman. The persistence of menstruation after fecundation is an anomaly of less frequent occurrence than is popularly believed; for it is wrong to confound with this a slight accidental flux of blood, which usually occurs at quite irregular intervals.—*Rev. Méd.-Chir.*, iv, p. 47.

On Trismus Nascentium. By DR. SIMMS.

DR. SIMMS, in continuing his account of this disease, first qualifies some of the statements heretofore made by him (Amer. Journ. Med. Sc., April, 1846). Thus, the statement that *deficient ossification of the cranial bones* is an essential condition of its production implied a misnomer, the deficient ossification being no pathological condition, but the physiological one so conducive to safe and easy parturition. He formerly thought also that *dorsal decubitus* on a hard pillow always preceded the disease; but he has since found it may arise while the child is lying in a featherbed, upon what mothers call its side. He has also found that death may result without spinal hemorrhage being induced.

But having made these corrections, he adds that all subsequent experience has only confirmed him more and more in the belief of the truth of his fundamental position, that the trismus of infants is due to a mechanical pressure exerted by the occipital bone, displaced inwards, upon the medulla oblongata and its nerves. We are bound to say we think he has made a strong case out for the consideration, not only of his medical brethren in America, where the disease is fearfully frequent, but also for those of Europe; inasmuch as he attributes to the same cause many symptoms often met with here, as we shall presently see.

The additional cases now adduced, may be divided into four categories:

In the *first* of these the various symptoms of trismus, such as spasmodic closure of the jaw, inability to suck, stridulous breathing, &c., resulted from easily detected displacement of the occipital bone, and were relieved when this was remedied by placing the child in a suitable position—and that sometimes almost immediately. We may refer to one of the cases, which the author states to have been one of the most remarkable he ever saw, on account of its chronic duration, and the age of the child—infants a few hours or days old being usually the subjects of the affection. The disorder occurred in the person of a little negro child (the children of the negroes being preeminently liable to it) between 4 and 5 months old, and had existed in a greater or less degree from its birth. It rolled its head about continually, just like a child in hydrocephalus; and the *right* hand and leg were constantly performing regular gyratory movements, while the *left* were paralysed. The child, by lying on its back, and constantly rolling its head, had worn the hair entirely off the occiput. On examining the head, the *right* edge of the occipital bone was found pushed under the corresponding part of the parietal, while the *left* was riding over its parietal. Turning the child completely on its side relieved the symptoms in a few hours; but their reproduction and removal were found to be induced according as the child was replaced on its back or its side. Even eleven days after relief had been obtained, the symptoms were capable of renewal by compressing the occiput with the thumbs.

2. In the next class of cases the displacement is far from obvious; and a careless examination would declare its non-existence; and such a degree as this may occur when the child is laid *diagonally* instead of being placed square on its back. This position will be generally found to be the one which mothers call laying a child on its side, and is one which allows of a partial pressing inwards of the occiput. The author observes, that we must lay down as the general rule, that *during labour the occiput is displaced inwards, having its lateral edges under the parietal bones.* After birth, when the child is placed in a proper position, this displacement is gradually rectified, from 12 to 48 hours, and generally about 36, being required for this purpose; but in this rectification the edges of the bones are *not brought level and opposite* to each other, so as to make the commissure tense between them. In hundreds of infants' heads examined by the author, he has seldom found such a disposition. The occiput after rectification is found riding *over the edges of the parietal;* and then no position or force is competent to push it back beneath them. But to decide what is really the position of the bone, requires other than a superficial examination; and this is the plan the author follows. He presses the forefinger firmly against the occiput, about half an inch from the lambdoid suture, and half way between the posterior fontanelle and mastoid process, and carries it slowly *across* the suture, making considerable pressure the while. When the finger reaches the suture, if the occipital bone is *under* the parietal, it yields, and the finger has to be raised a little before it can pass over the parietal edge; but if the occipital is *in its normal position, outside* the parietal, there is no obstacle to the progress of the finger. If we reverse the course of the finger, we find the occiput when *inside* opposes no obstacle, which, however, it does when *outside.* This method has served to detect displacement, when superficial examination has declared none to have existed.

In reference also to these obscure cases, the author observes that the ***symptoms*** of trismus are by no means so well marked as some observers seem always to expect. There may be no ***locking*** of the jaw, or various other signs observed in the traumatic form of the disease; but there is one symptom which, from its invariable presence, may be regarded as ***pathognomonic***, viz., ***inability to suck*** though no mechanical obstacle exist. With this symptom some other, such as stridulous breathing, diarrhœa, or other indication of general disorder, is always present; but this one is never absent.

3. In the next category of cases, are placed those which the author thinks may be termed ***trismoid***, inasmuch as they want some of the characters which appertain to true trismus, and yet are produced by the same cause, and relieved by the same means. They may almost be said to be examples of prolonged duration of the initiatory stage of the affection. There is nothing definite in the description given, most of the cases cited having suffered from some inability to suck, fretfulness, colics, impeded respiration, sleeplessness, &c.—all remedies failing to relieve these disorders, until the pressure from the occiput had been removed. We think this class of cases well deserving the attention of practitioners of this country. Every one must be familiar with them in practice, must have discovered the inefficiency of carminatives and anodynes, and have observed the marked relief obtainable by the child during ***change of posture*** in the nurse's arms, and especially in such postures as take pressure off from the occipital region. Another objection to the dorsal decubitus so generally maintained for infants, is the ***stasis*** produced in the pulmonary circulation—a fertile source of mischief in the French Foundling Hospitals, where the infants are left for long periods on their backs. While adverting to the hygiene of young infants, we may notice the mischievous practice which prevails, of covering their faces entirely with the bed-clothes, or even under soft pillows, obstructing the supply of oxygen so essential to their healthy respiration.

4. Cases occasionally occur in which mere position will not suffice to restore the displacement, and then a surgical operation (a case of the successful performance of which is given) is required for the elevation of the bone. In these cases, either the bones are too much ossified and consequently impacted against each other to allow of any motion, or the occiput is misshapen, and thus presents a physical obstacle to spontaneous rectification.—***Amer. Journ. Med. Sc.***, vol. xvi, pp. 59-78.

Extraordinary Size of Twins. By Dr. BERTOLET.

MRS. A. H., in her sixth labour, was delivered of a living female child, weighing 9½ lbs.; and shortly after, another was found to be presenting, which, owing to the occurrence of hemorrhage, was extracted by means of the long forceps. It was a male and still-born, and weighed 11¼ lbs.; so that the conjoined weight of the two was 20¾ lbs. The mother and first child did well.—*Phil. Med. Ex.*, 1848, No. 44.

MATERIA MEDICA AND THERAPEUTICS.

On the Question whether Calomel exerts any Specific Influence on the Biliary Secretion.

By M. MICHEA.

M. MICHEA, after detailing the various opinions which have been advanced as to the nature of the green stools which so often ensue on the administration of calomel, states the results of his own examination of fecal matters under varying circumstances. He observes, that in the dejections of healthy persons, the greater proportion of the elements of the bile exist only in a state of combination, and require alcohol and potass for their detection. Free bile, i. e. bile soluble in water, is, according to Berzelius, found only in the proportion of ⅞ per cent. of fecal matters. For the detection of free bile M. Michea prefers nitric acid to Pettenkofer's test.

In six persons in good health the acid furnished no traces of bile; and of three others suffering from gastro-intestinal affections, in one only, who had bilious vomiting and green purging, was a notable proportion indicated. Of eight cases in which calomel was administered, in some in large, in others in small doses for successive days, in four only were green stools produced. This shows that the action of the calomel on the bowels is very uncertain, and tends to confirm the truth of Mialhe's doctrines, that it is dependent upon the conversion of the chloride into deutochloride; most of the persons in whom it produced no action, having been women, whose humours contain less of the chlorides than do those of men. Not only are the stools changed in colour, but they are so in consistency, possessing neither the solidity of the natural stool, nor the aqueousness of those of dysentery or typhoid fever, but assuming an intermediate viscous character. In two of these cases the acid plainly evidenced the existence of bile; and not only of biliverdine, but of the albumen of that fluid. In the other two cases albumen was also precipitated, giving, however, a colour more analogous to the bilifulvine of Mulder. In stools produced by various other purgatives, in five individuals no traces of bile were produced, nor did they assume the consistency of those induced by calomel. The general result of the investigation is, then, to confirm the opinion of those who maintain the agency of calomel on the biliary secretion.—*L'Union Médicale*, 125-6.

On the Employment of the Seeds of Phellandrium Aquaticum in Phthisis and Chronic Bronchitis. By M. SANDRAS.

IN our Number for July we quoted M. Michea's testimony in favour of this substance; and since then M. Sandras has published the results of a careful investigation into its merits, conducted during eight years at the Beaujon. He speaks of it in the highest terms of praise as a palliative of the most distressing symptoms of phthisis; and believes that occasionally it even exerts a curative agency, and, at all events, indefinitely postpones the progress of cases which furnish all the symptoms of incipient tubercle. He is, however, fully aware how deceptive these symptoms often are, and speaks with due caution on this point. The important agency of the seeds, however, in relieving suffering in undoubted and advanced cases of the disease seems certain; the days of the sufferer not only being considerably prolonged, but his path to the grave most materially smoothened. The good effects generally manifest themselves in from a week to a fortnight, by a diminution of fever and diarrhœa, a return of appetite and sleep, less dyspnœa, and an easier cough, so that the patient often supposes himself nearly well. The strength is supported in this way for a considerably longer period than it otherwise would be; and when at last it finally gives way, the course of the disease then becomes very rapid. *Chronic bronchitis* is obviously and speedily modified advantageously by this medicine. It is especially indicated in that form, which comes on in aged persons in cold damp weather, and persists until this changes; and in young lymphatic subjects, deficient in reactive power, it cuts short the tedious cough left by colds. M. Sandras has found it of no avail in emphysema and nervous asthma, except inasmuch as these were connected with chronic bronchitis.—*L'Union Médicale*, No. 134.

On the Treatment of Lupus by Cod-liver Oil. By M. EMERY.

DURING eighteen years' service at St. Louis, M. Emery has had abundant opportunities of studying this intractable disease, inasmuch as cases are sent to that hospital from all parts of France; so that, among 120 or 130 patients he has in his wards, there are seldom less than from 15 to 20 cases of lupus. After describing the various forms which the disease assumes, he goes on to state that he has given a full trial to all the various caustic and other applications which have been recommended for its removal; combining with these a good regimen, and a variety of internal remedies; and he has always remained in doubt whether the few cases so treated, which he has seen recover, would not have done so if left to the unaided resources of Nature. Still, upon the whole, he believes that caustics may form a useful addition to the internal means employed; but to be useful, they should be varied.

M. Emery, having been disappointed in his trials of *cod-liver oil,* given in the usual doses, resolved to try the effect of increasing these to a great extent. Having so administered it to 74 patients, he now states that the results are highly satisfactory, having been able to accomplish a far greater proportion of cures by its means than by any other. The doses are indeed formidable, when we consider the repulsive nature of the substance; but, in the prospect of removing so hideous and intractable a disease, the patients freely take them. He begins with 25 drachms *per diem,* and augments the quantity gradually for 15 days, until 125 are taken, and, in the case of the persistence of the disease, until the quantity amounts to 175 or even 250 drachms. If, in the course of taking it, the stomach revolts somewhat, a glass or two of Seltzer-water is given; while if there is great derangement of the alimentary canal or febrile action, he suspends its employment, recommencing with the 25 drachms. MM. Malgaigne and Devergie have witnessed the results of the treatment, and speak highly of its efficacy.—*Rev. Méd. Chir.,* tom. iv, pp. 65-73.

On Vinum Sem. Colchici Opiate in Gonorrhœa. By Dr. FROINUS.

DR. EISENMANN, in a communication to the 'Wochenschrift' in 1847, demonstrated the great utility of the *Vin. sem. colch.* (V. s. c. ʒiij; Tr. opii, ʒ½), in doses of from 25 to 30 drops three or four times a day, in the treatment of both male and female gonorrhœa. Without denying that it may be sometimes desirable to precede its use by purgatives or oleaginous fluids, he had himself found it applicable in all stages of the disease, effecting a cure, upon an average, in about seven days. In the present paper 10 additional cases are related of its successful employment in various stages of the affection; and reference made to some 50 others, in which it proved as satisfactory in the results. It is not only useful in infectious gonorrhœa, but in discharges from the mucous membranes from other causes.—*Casper's Wochen.,* No. 36.

Belladonna in Eneuresis. By M. TROUSSEAU.

M. TROUSSEAU takes the occasion of an obstinate case of eneuresis, occurring in a child 5 years of age, yielding to belladonna, to make known the modes in which M. Bretonneau has long employed it in this affection with great success. He prefers the powder of the root, and, to secure its proper preparation and preservation, he grows it in his own garden at Tours; but M. Trousseau has seldom found this in good condition in the chemists' shops, while the powder of the leaf may easily be so obtained. This is made into pills, each containing 1 *centigramme* (abont ⅐ grain), and one of these is given every night. If the incontinence diminishes, the dose is not increased; but otherwise it is doubled or tripled. After taking the medicine for a week, it is suspended for a few days, then resumed for a week, and suspended for a week. Resumed again for a week, it is next suspended for two; and after another week's employment, is suspended for a month, and so on for a twelvemonth. Unless this persistence be observed, the cure of a disease so liable to relapse will not prove permanent.—*L'Union Médicale,* No. 122.

Poisoning by Digitalis. By Dr. FORGET.

M. FORGET of Strasburg, who is not one of those who confine themselves to the publication of their successful cases, furnishes an account of the fatal results which attended the administration of the tincture of digitalis in a case of confirmed and advanced phthisis. The case occurred in the person of a woman, æt. 36, for whose relief the entire materia medica seems to have been ransacked, and who, before the digitalis was commenced, had become exceedingly reduced. Beginning with 15 drops *per diem,* in divided doses, on the 2d of June, the dose was daily increased until the 8th, when it amounted to 100 drops *per diem,* no ill effect having manifested itself—save that the pulse on this last morning was a little irregular, and smaller, and slower than it had been. Towards the latter part of this day, however, the patient, after some vomiting, became attacked with convulsions, and expired. (The propriety of administering the medicine at all in so exhausted a subject may be questioned, and at all events, possessed as it is of so remarkable an accumulative operation, the dose should not have been so rapidly increased.)

In contrast with this case, two others may be briefly quoted, in which recovery occurred after taking large quantities of digitalis. In the first of these (occurring in a woman, æt. 28, in the hospital for heart disease), 10 drachms of the tincture were swallowed by mistake on the 25th of April. Half an hour after, violent cephalalgia, with vertigo and confusion of ideas, occurred, and this was soon followed by abundant vomiting, which was promoted by tartar emetic, and continued for several hours. The skin then became cold and damp, the pulse quicker, and the urine abundant. Next day (20 hours after) the patient remained in a partial stupor, her respiration being very slow, and her pulse numbering 50, but regular, and the abdomen being very tender. After this she may be said to have slowly but regularly amended, the somnolence gradually disappearing, the pulse first sinking to 48 and 44, and then rising to 65, 68, and 80; the respiration being sometimes embarrassed, and great muscular debility long abiding. The urine long continued abundant. By the 14th of May the symptoms of poisoning may be said to have passed away, except some occasional headache and confusion.

In the second case, a woman, æt. 68, recovering from *œdema pulmonum*, took, on the 24th June, one half of an infusion of digitalis, made with 4 drachms of the leaves. Vertigo, convulsions, loss of vision, and vomiting, were the speedy consequences, and to these succeeded coldness of the surface, sleeplessness, great diminution of the heart's impulse, a filiform, slow, and intermittent pulse, great prostration, and a disappearance of a habitual dyspnœa. The treatment consisted chiefly in the vigorous employment of external stimuli, and the use of stimulant enemata. By the 30th all alarming symptoms had disappeared.—*Bulletin de Thérapeutique*, tom. xxxv, pp. 328, 418.

MR. NEILL, OF LIVERPOOL, AND HIS REVIEWER.

It was scarcely to be expected that Mr. Neill should quietly submit to the severe criticism, to which we took upon ourselves to subject his work on Cataract in our last Number. Authors, if unjustly treated, have more than one course open to them in seeking redress, and are free to make their election. Suffice it to say, Mr. Neill was written to and requested to specify the charges in the Review of which he had reason to complain, with an undertaking that the same should be looked into, and that if injustice had been done to him (Mr. Neill), we should withdraw any unsustainable accusations by which he had been aggrieved.

We have received in reply so strong a remonstrance, both against the general tone of the review, and against certain specific charges contained in it, as calculated seriously to damage Mr. Neill's professional character, that we feel it requisite so far to depart from our usual course as to take this public notice of it, and in justice to withdraw or explain such imputations at the earliest opportunity.

In the first place, we would assure both Mr. Neill and our readers that nothing was farther from our intention than to disparage Mr. Neill's abilities as a practitioner; but it has been felt not only that the criticism was excessive, but that we have ventured into matters beyond the legitimate province of reviewers to meddle with.

One of the specific grounds of complaint urged by Mr. Neill is our charge of "quackery." This was not a *general* charge, but was founded on a particular case. Mr. Neill calls upon us to withdraw it, on the ground that the basis or effective ingredient was declared, and that the formula is no secret in Liverpool, of which he has furnished us with proof. True, the *muriate of atropina*, which Mr. Neill says "is the best for the purpose" (viz. of dilating the pupil), was stated by him in his book, but not the formula, however secondary. Had it been so, the charge would not have been made.

With reference to our imputation of "hypocrisy," that also was founded upon a particular case,—namely, Mr. Neill's assumption of credit to himself for having withheld the *name* of the *printer*, the *bookseller*, and *place of publication*, of a quackish pamphlet,—none of which we believed he had any power to give. As Mr. Neill did not say (expressly) that he was in possession of the pamphlet, and made no farther citation from it, than that given by Desmarres, we felt ourselves justified in assuming that it was "extremely improbable that Mr. Neill ever saw Schlesinger's pamphlet," and the reviewer certainly then went on to say more. Improbable, however, as Mr. Neill's possession of Schlesinger's pamphlet appeared to us, we have adequate proof placed before us that he *really did possess it*, as the note in his book (p. 111) implied. We, therefore, unreservedly withdraw this charge, and express our sincere regret at having been led to make it.

Mr. Neill further complains of our treatment of his statistics. He, and others we learn, have misunderstood our remarks upon the coincidence of numbers in different years; supposing that they imply *one* and the *same* coincidence, between the numbers set down for several years, instead of a coincidence repeated in five successive years between the number of cataract-*cases* and that of cataract-*operations*. We declared the repeated coincidence, in the qualified sense now explained, to be incredible. Mr. Neill has unconsciously confirmed our assertion, by showing that he has *understated*, even the cataract-operations performed by him. We did not allege, or even insinuate that Mr. Neill had *overstated* the number of his operations. It is considered, however, that there is an unfortunate ambiguity in the language used by the reviewer. If, consequently, by reason of such ambiguous expressions in the critique, an impression, either of exaggeration or of falsification in the returns, has been produced unfavorable to Mr. Neill's character, we are sorry for it, and willingly rectify an inference which was not intended.

Next, as regards the exaggeration alluded to (p. 436), as having been made by the mover of a resolution at a public meeting, whose proceedings were appended to the Annual Report of the Liverpool Eye Infirmary, it is but just to Mr. Neill and to the Committee of the Institution to observe, that the *true* number of patients for the year in question is given both in Mr. Neill's work on Cataract (p. 164, note) and in the official part of the Annual Report itself. We never intended to charge Mr. Neill with being a party to any *ex parte* exaggeration; and are sorry if the allusion to it in our pages has been understood as implicating him.*

Lastly. The use of the term "base" not being justifiable by the usages of professional criticism,—as applied to Mr. Neill's supposition, that Drs. Morton and Wells might to some extent have profited by his suggestions in completing the discovery of ether-inhalation in America,—it is regretted that the epithet should have proceeded from the pen of the Reviewer, and that it escaped our own attention.

---

* Taking either the Report (1846) or Mr. Neill's own book (1848), nothing can be clearer in meaning, than that the numbers (whether loosely stated or not), namely, 17,656 (Neill), or 19,220 (speaker quoted by the Reviewer), or 19,277 (Report), refer distinctly, not to the number of *cases*, but to the *visits* of those who, as patients, received the benefit of the charity. The number of "patients" in contradistinction to "visits," is unmistakeably set down by Mr. Neill in his work.

BOOKS RECEIVED FOR REVIEW.

Elements of Anatomy. By Jones Quain, M.D. Fifth Edition. Edited by Richard Quain, F.R.S., and William Sharpey, M.D., F.R.S. Part III, 1848. 8vo, pp. 606. With numerous Wood-engravings.

The Microscopic Anatomy of the Human Body in Health and Disease. Illustrated with numerous Drawings in Colour. By Arthur Hill Hassall, F.L.S., &c. Part XIV, London, 1848. With Eight Plates.

Reflections on Organisation; or Suggestions for the Construction of an Organic Atomic Theory. By Henry Freke, A.B. M.B. T.C.D., M.R.I.A. Dublin, 1848. 8vo, pp. 80.

Hints towards the Formation of a more Comprehensive Theory of Life. By Samuel Taylor Coleridge. Edited by Seth B. Watson, M.D. London, 1848. Post 8vo, pp. 94.

The Natural History of Man; comprising Inquiries into the Modifying Influence of Physical and Moral Agencies on the different Tribes of the Human Family. By James Cowles Prichard, M.D., F R.S., M.R.I.A., &c. &c. Third Edition, enlarged. London, 1848. 8vo, pp. 676.

Physiological, Anatomical, and Pathological Researches. By John Reid, M.D., F.R.C.S.E. Edinburgh, 1848. 8vo, pp. 658.

Pathologia Indica, or the Anatomy of Indian Diseases, based upon Morbid Specimens, from all parts of the Indian Empire, in the Museum of the Calcutta Medical College. By Allan Webb, B.M.S. Second Edition. Calcutta, 1848. 8vo, pp. 540.

Pathology of the Human Eye. By John Dalrymple, F.R.C.S. London, 1848. Small folio. Four Plates.

An Inquiry into the Degree of Certainty in Medicine; and into the Nature and Extent of its Power over Disease. By Elisha Bartlett, M.D. Philadelphia, 1848. 8vo, pp. 84.

Observations upon Bulam, Vomito-Negro, or Yellow Fever, with a Review of a "Report upon the Diseases of the African Coast, by Sir William Burnett and Dr. Bryson," proving its highly contagious powers. By Sir William Pym, K.C.H. London, 1848. Post 8vo, pp. 311.

A Practical Treatise on the Domestic Management and most important Diseases of Advanced Life. By George E. Day, M.D. London, 1848. 8vo, pp. 342.

Observations on the Pathology of Croup; with Remarks on its Treatment by Topical Medications. By Horace Green, A.M., M.D. New York, 1848. 12mo, pp. 116.

On the Influenza, or Epidemic Catarrhal Fever of 1847-8. By Thomas B. Peacock, M.D., Physician to the Royal Free Hospital. London, 1848. 8vo, pp. 184.

Clinical Lectures on the Practice of Medicine. By Robert J. Graves, M.D., M.R.I.A., &c. &c. Second Edition. Edited by J. Moore Neligan, M.D., M.R.I.A. In Two Volumes. Dublin, 1848. 8vo, pp. 1156.

Clinical Lectures, delivered in the Theatre of Mercer's Hospital, during the Session of 1847-8. By James F. Duncan, M.D. T.C.D. Dublin, 1849. 8vo, pp. 122.

The Diagnosis and Treatment of Eruptive Diseases of the Scalp. By J. Moore Neligan, M.D., M.R.I.A., &c. &c. Dublin, 1848. 12mo, pp. 56.

Observations on Malignant Cholera, intended to illustrate the Natural Course of the Disease, the Natural Processes by which a Spontaneous Recovery is effected, and the Mode of Treatment best adapted to second the Curative Efforts of Nature. By Andrew Buchanan, M.D. London, 1848. 8vo, pp. 42.

Practical Observations on a successful Mode of treating Cholera. By Charles Patterson, M.D. Dublin, 1848. 8vo, pp. 8.

Observations on the Asiatic Cholera, during a Residence in St. Petersburg in 1848, and on its Prevention and Cure. With an Account of the Sanitary Regulations proposed to be adopted against the Spreading of the Disease in this Country. By Adair Crawford, M.D. London, 1848. 8vo, pp. 24.

On the Treatment of Asiatic Cholera. By Archibald Billing, M.D., A.M., F.R.S. London, 1848. 8vo, pp. 15.

Reports on Asiatic Cholera, in Regiments of the Madras Army from 1828 to 1844, with Introductory Remarks on its Modes of Diffusion and Prevention, and Summary of the General Method of Treatment in India. By Samuel Rogers, F.R.C.S. London, 1848. 8vo, pp. 268.

Plain Directions for the Prevention and Treatment of Cholera. By Thomas Allen, M.R.C.S. Oxford, 1848. 8vo, pp. 40.

The Cholera at Malta in 1837; from the Italian of Guiseppe Stilon, M.D., &c. &c., Physician to the Cholera Hospital in Valetta. By Seth B. Watson, D.M. To which is prefixed, a Letter from Sir John Stoddart, LL.D., Chief Justice of Malta. London, 1848. Fcap. 8vo, pp. 178.

Some New Views respecting Asiatic Cholera. By Arthur Leared, A.B., M.B. London, 1848. 8vo, pp. 50.

Table for the Uniform Medical Registration of Cases of Cholera, issued by the Western Medical and Surgical Society of London.

Diet and Cholera; showing the vital importance of wholesome Diet, and that its impurities and deficiencies are the chief Cause of Cholera. By William B. Boddy. Lond. 1848. 8vo, pp. 40.

A Letter to Lord Morpeth, and the Members of the Board of Health, on the Question—Is Cholera Contagious or Not? By William Reid, M.D., F.R.C.P. Ed., &c. London, 1848. 8vo, pp. 16.

Report on Epidemic Cholera. By C. B. Coventry, M.D. New York, 1848. 8vo, pp. 14.

A Few Thoughts on Cholera, in reference to its Origin, the Nature of the Exciting Cause, and the Principle of Treatment. By Michael Thomas Sadler, M.R.C.S. London, 1848. 8vo, pp. 52.

State of the Lincoln Lunatic Asylum. Lincoln, 1848. 8vo, pp. 54.

A Critical Treatise on the General Paralysis of the Insane. By James M. Winn, M.D. London, 1848. 8vo, pp. 50.

Observations on the Management of Madhouses. Part III. By Caleb Crowther, M.D. London, 1849. 12mo, pp. 126.

Surgical Anatomy. By Joseph Maclise, M.R.C.S. Fasciculus I. Four Plates, with Commentary. Folio. London, 1848.

Observations on Hospital Gangrene; with reference chiefly to the Disease, as it appeared in the British Army during the late War in the Peninsula, with Prefatory Remarks. By John Boggie, M.D., Surgeon to Her Majesty's Forces. Edinburgh, 1848. 8vo, pp. 152.

On the Treatment of Ulcers of the Leg without Confinement; with an Inquiry into the best Mode of effecting the permanent Cure of Varicose Veins. By Henry T. Chapman, F.R.C.S. London, 1848. Post 8vo, pp. 156.

A Treatise on the Cure of Ulcers by Fumigation; in which a rational Treatment is deduced from the Physiology of Ulceration. By George A. Walker. London, 1847. 8vo, pp. 112. With six Plates.

A Short Account of a Case of Disease of the Appendix Cæci, cured by Operation. By Henry Hancock, F.R.C.S. London, 1848. 8vo, p. 12.

On Bandaging, and other Operations of Minor

Surgery. By F. W. Sargent, M.D. Philadelphia, 1848. Post 8vo, pp. 379.

Clinical Midwifery; comprising the Histories of Five Hundred and Forty-five Cases of Difficult, Preternatural, and Complicated Labour. With Commentaries. By Robert Lee, M.D., F.R.S., &c. &c. Second Edition. London, 1848. Fcap. 8vo, pp. 228.

Lectures on the Diseases of Infancy and Childhood. By Charles West, M.D., Senior Physician to the Royal Infirmary for Children, and Lecturer on Midwifery at St. Bartholomew's Hospital. London, 1848. 8vo, pp. 488.

The Medical Practitioner's and Student's Library.—A Practical Treatise on the Diseases of Children. By J. Forsyth Meigs, M.D., &c. Philadelphia, 1848. 12mo, pp. 576.

Practical Pharmacy: the Arrangements, Apparatus, and Manipulations of the Pharmaceutical Shop and Laboratory. By Francis Mohr, PH.D., and Theophilus Redwood. With 400 Wood-engravings. London, 1849. 8vo, pp. 390.

Medical Jurisprudence. By Alfred S. Taylor, F.R.S. Third Edition. London, 1848. Fcap. 8vo, pp. 850.

Elements of Chemistry, including the Applications of the Science in the Arts. By Thomas Graham, F.R.S. Second Edition. Parts I, II, III. London, 1848. 8vo, pp. 544.

Chemical Reports and Memoirs, on Atomic Volume; Isomorphism; Endosmosis; the Simultaneous Contrast of Colours; the Latent Heat of Steam at different Pressures; the Artificial Formation of Alkaloids; and Volcanic Phenomena. Edited by Thomas Graham, V.P.R.S. Published by the Cavendish Society. London, 1848. 8vo, pp. 370.

Medical Chemistry, for the Use of Students and the Profession; being a Manual of the Science, with its Applications to Toxicology, Physiology, Therapeutics, Hygiene, &c. By D. P. Gardner, M.D. Philadelphia, 1848. Post 8vo, pp. 396.

Chemistry, as exemplifying the Wisdom and Beneficence of God. By George Fownes, F.R.S. Second Edition. London, 1849. Fcap. 8vo, pp. 160.

An Introduction to Practical Chemistry, including Analysis. By John E. Bowman. With numerous Wood-engravings. London, 1848. Fcap. 8vo, pp. 280.

Chemical Technology; or Chemistry applied to the Arts and Manufactures. By Dr. F. Knapp, Professor at the University of Giessen. Edited, with numerous Notes and Additions, by Dr. Edmund Ronalds and Dr. Thomas Richardson. Vol. II, illustrated with 214 Wood-engravings and a Steel-plate. London, 1848. 8vo. pp. 492.

An Introduction to Botany. By John Lindley, PH.D., F.R.S. With Six Copper-plates and numerous Wood-engravings. Fourth Edition. London, 1848. Two vols. 8vo, pp. 834.

An Analytical Compendium of the Various Branches of Medical Science, for the Use and Examination of Students. By John Neill, M.D., and Francis Gurney Smith, M.D. Philadelphia, 1848. Post 8vo, pp. 908.

A Dictionary of Medical Science; containing a concise Explanation of the various Subjects and Terms, with the French and other Synonymes, &c. &c. By Robley Dunglison, M.D. Seventh Edition, carefully revised and greatly enlarged. Philadelphia, 1848. 8vo, pp. 912.

Surgical Experience of Chloroform. By James Miller, F.R.S.E., F.R.C.S.E., &c. &c. Edinburgh, 1848. 8vo, pp. 60.

Anæsthetic Midwifery: Report on its early History and Progress. By J. Y. Simpson, M.D. Edinburgh, 1848. 8vo, pp. 54.

On the Employment of Chloroform in Dental Surgery, its Mode of Exhibition, &c. By Francis B. Imlach, Dentist, L.R.C.S. Ed., &c. Edinburgh, 1848. 8vo, pp. 10.

On Medical Education: an Introductory Lecture, delivered at the Liverpool Infirmary School of Medicine, at the opening of the Medical Session 1847-8. By Joseph Dickinson, M.A., M.D., F.L.S., &c. Liverpool, 1848. 12mo, pp. 28.

Inaugural Address, delivered before the New York Academy of Medicine, Feb. 2, 1848. By John W. Francis, M.D. New York, 1848. 8vo, pp. 24.

Anniversary Discourse before the New York Academy of Medicine. By John W. Francis, M.D.

Summary of the Transactions of the College of Physicians of Philadelphia, from December, 1847, to March, 1848.

Medico-Chirurgical Transactions. Vol. XXXI. London, 1848. 8vo, pp. 522. With seven Plates.

Memorie della Società Medico-Chirurgica di Bologna, seguito agli opuscoli da essa publicati. Vol. V. Fascic. I. Bologna, 1848.

Guy's Hospital Reports. Second Series. Vol. VI. Part I. October, 1848. 8vo, pp. 200. With Five Plates.

Remarks on the Extension of Education at the University of Oxford, in a Letter to the Rev. W. Jacobson, D.D. By Henry Wentworth Acland, M.D., F.R.S. Oxford, 1848. 8vo, pp. 40.

A Practical Treatise on the Use of the Microscope, including the different Methods of Preparing and examining Animal, Vegetable, and Mineral Structures. By John Quekett. Illustrated with Nine Plates and Two Hundred and Forty-one Wood-engravings. London, 1848. 8vo, pp. 464.

The Poetry of Science, or Studies of the Physical Phenomena of Nature. By Robert Hunt. London, 1848. 8vo, pp. 464.

Tracts for the Improvement of our Popular Literature. No. III, containing Physiology, its Moral, Political, and Hygienic Teachings. An Essay in Blank Verse. With Minor Pieces. By Robert Dick, M.D. London, 1848. 18mo, pp. 80.

Underwood's Medical Appointment-Book, Diary, and Almanac for 1849, for the especial use of Physicians, Surgeons, General Practitioners, Dentists, &c. &c. London, 1849. 12mo.

Wonders displayed by the Human Body in its endurance of Injury. From the Portfolio of Delta. With Etchings by the Author. London, 1848. 8vo, pp. 40.

Lectures on the Actual Condition of the Metropolitan Grave-Yards. By George Alfred Walker, Surgeon. London, 1847. 8vo, pp. 120.

An Essay on the Teeth. By Augustus Cook. London, 1848. 12mo, pp. 76.

THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL REVIEW.

APRIL, 1849.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

1. *The Elements of Materia Medica and Therapeutics.* By Jonathan Pereira, m.d., f.r.s., &c. Third Edition. Vol. I.—*London*, 1849. 8vo, pp. 898.
2. *Traité de Thérapeutique et de Matière Médicale.* Par A. Trousseau, et H. Pidoux. Troisième Ed. Tome I et II.—*Paris*, 1847. 8vo, pp. 895, 898.
3. *Traité de l'Art de Formuler, ou Notions de Pharmacologie appliquée à la Médecine.* Par le Docteur Mialhe.—*Paris*, 1845. 12mo, pp. 220.
4. *Lectures on the Chemistry of Pathology and Therapeutics, showing the application of the Science of Chemistry to the Discovery, Treatment, and Cure of Disease.* By Alfred B. Garrod, m.d.—(Lancet, Vol. I and II, 1848.)
5. *Lectures on the Influence of Researches in Organic Chemistry on Therapeutics, especially in relation to the Depuration of the Blood, delivered at the Royal College of Physicians.* By Dr. Golding Bird, a.m., f.r.s., &c. (Medical Gazette, 1848.)

For a great number of years the rich mine of Chemistry was worked solely for the purpose of ministering to the wants of the artist and the manufacturer; and from the circumstance of these wants being comparatively few, and, as it were, easily supplied from the produce of the very surface, it might be said that the mine was almost unexplored, and that but little was known of its real wealth and importance. Of late, however, new and increasing demands have been made upon its resources; and the workmen, urged on this account to extend their operations, have gone so much deeper and further into it, that they have by their labours laid bare such prolific veins as will serve to enrich nearly every branch of human knowledge. Already have Agriculture and Physiology, two sciences which were for a long time crippled and kept poor by the blighting influences of prejudice, false doctrine, and a slavish dependence on the authority of the past, made their selection from this storehouse of wealth, whereby they have in a very short time become enriched beyond all precedent. And from the list of works which stands at the head of this article, it will be seen that Pathology, Therapeutics, and Practical Medicine are about to make a similar selection, and we doubt not with a similar result. At present, however, it cannot be said that the powers of Chemistry are applied in such a way as to bear with the greatest advantage upon the last-named science, for we have as yet but a very unsatisfactory account of the chemistry of the healthy and morbid structures of the human body; and our ideas are extremely vague with regard to the influence of remedies on the chemical composition of the animal tissues, fluids, and secretions. Nay more, we have but an imperfect knowledge of the nature of the changes to which remedial agents are subject during their passage through the animal economy. And excepting the few observations which have been made by Wöhler, Stehberger, Mialhe, Orfila, Müller, and some others, upon the existence of certain animal, vegetable, and mineral substances in the secretions, we have not yet obtained any precise information as to the means whereby therapeutic and toxic agents are expelled from the human body. How, then, until our knowledge of these matters has become more extensive, can we expect to be able to make a good, a sound, or a successful application of chemistry, either to the discovery or to the cure of disease? Nevertheless, it is to be hoped that we are now in the true path for the recognition of great truths; and sure are we that the authors to whom we have referred, as well as those whose labours we are about to review, are entitled to our greatest praise, for having, like good pioneers, cleared away many of the difficulties which were opposed to our progress. And though it must be admitted that more than one of them have appeared upon the road mounted somewhat after the fashion of my Uncle Toby, yet where is the harm of their so doing; for we may say with Sterne, "who has not a *hobby-horse,* and so long as a man rides peaceably and quietly along the king's highway, and does not compel either you or me to get up behind him, pray, sir, what have you or I to do with it?" We shall, therefore, touch as gently as possible upon this infirmity of human nature.

With respect to the merits of the first work which stands upon our list, it is almost superfluous for us to express an opinion; for to say that it is a good and a useful book, would be but to repeat the cry of the whole medical profession; and to characterise it as a vast and comprehensive collection of valuable information, doing the highest credit to the industry, discrimination, and accuracy of its learned author, but not claiming for itself any great originality, is after all only telling our readers what every one of them probably knows as well as ourselves.

It is enough, then, for us to remark, that Dr. Pereira's two volumes on Materia Medica have passed through three editions in the course of ten years; and that they have been made the text-books on this subject, both upon the Old and New Continents. These are, we think, sufficient evidences of their claims to public notice, and of the manner in which they have been estimated by the great body of the medical profession through a large part of the civilized world.

On comparing the present volume with that of the last edition, we perceive that about thirty pages have been added to it, and that the chief alterations have been made in the first 250 pages of the work;—the author having cut down the details of many of the subjects, as for example, those which relate to hygienic remedies, such as food, diet, &c.; while other chapters have been considerably enlarged, as, for instance, those on electricity, exercise, and that (as yet) unprofitable subject, the classification of medicines. In fact, 131 pages—just one half of Dr. Pereira's account of the general properties, &c. of medicines—have been devoted to the classification of therapeutic agents; a subject which, as the author himself admits, is in a very unsatisfactory state, and which he has endeavoured to amend by the introduction of a new physiological arrangement, which has evidently cost him great labour, but which he can only himself regard as a step towards something better. In this classification Dr. Pereira first arranges medicines into two great groups; namely, into those which act topically (*Topica*), and those which act generally (*Generalia*). He then divides these groups into classes, the former into three, the latter into six, thus:

1. Medicines employed for their external or topical effects.—*Topica.*
    - α. Acting mechanically—Class 1. T. Mechanica.
    - β. Acting chemically—Class 2. T. Chemica.
    - γ. Acting dynamically—Class 3. T. Dynamica.
2. Medicines employed for their remote or general effects—*Generalia.*
    - α. Acting on the blood—Class 4. Hæmatica.
    - β. Acting on the respiratory organs—Class 5. Pneumatica.
    - γ. Acting on the nervous system—Class 6. Neurotica.
    - δ. Acting on the digestive organs—Class 7. Cœliaca.
    - ε. Acting on the excernent system—Class 8. Eccritica.
    - ζ. Acting on the sexual organs—Class 9. Genetica.

These classes are further divided into orders, and occasionally into sub-classes, orders, and sub-orders; the division being founded not always on the physiological effects of the remedies, but frequently upon their physical and chemical effects. To these subdivisions the author has appended some most unutterably hard names, which are not likely, we think, to come into general use.

Here, however, we must stop; for it is not our intention to take a general review of Dr. Pereira's work, but merely to refer to those portions of it which relate to the chemistry of therapeutics. By this means we shall be enabled to introduce to our readers an analysis of the other works which are before us, and thus to present them with some account of the present position of chemistry, in so far as it is related to the discovery and cure of disease.

And in order to effect this object, we shall direct their attention to the following questions:

1. What is the condition of our knowledge with regard to the chemical composition of the solids and fluids of the healthy human body? Are we enabled to deduce from this knowledge any data which will serve as a starting-point for our other inquiries?

2. How far are we acquainted with the nature of the changes which occur in the chemical composition of the solids and fluids during the existence of diseased action?

3. Do we know whether alterations take place in the chemical composition of the solids and fluids during the operation of medicinal substances? and if so, what those alterations are?

4. Are we prepared to say what changes are produced in the chemical composition of remedies during their passage through the animal body?

5. Are we in a position to make any practical application of our chemical knowledge to the discovery or cure of disease?

In reference to the First question, it may be at once said that the data which relate to the chemical composition of the solids and fluids of the healthy human body are most unsatisfactory; for if we examine the results of the countless analyses made of the tissues, fluids, and products of the animal economy, and endeavour to construct therefrom what may be termed standard formulæ for comparison, we shall perceive not only that there are many difficulties surrounding the inquiry, but also that the discrepancies are too numerous, and the information too incomplete for the accomplishment of this most desirable object. Some of these discrepancies have no doubt arisen from the imperfection of our analytical processes; and others are unquestionably due to the influence of age, sex, temperament, diet, locality, &c., upon the composition of the substances upon which the chemist has operated. Truly, as Dr. Simon has remarked,—

> "It will require an immense number of analyses in order to ascertain and determine these modifications, to express them in definite terms, to connect them duly with functional disturbances in the organism, or with other symptomatic phenomena, and finally, as far as possible, to endeavour to discover their origin."

But in order to perceive the truth of these observations, it will be necessary for us to take a survey of the work which has been already accomplished in this department of science. Let us, therefore, in the first place, direct our attention to the facts which have been made out with regard to the chemistry of the healthy Human Blood.

At first sight hardly anything would appear to be more satisfactorily determined than the chemical composition of this fluid; but when we proceed to ascertain the true state of our knowledge respecting it, we find not only that we are unacquainted with the changes which occur in the composition of healthy blood, as it traverses the different organs of the body, and that we are in comparative ignorance with respect to the relative constitution of venous, arterial, renal, pulmonic, and portal blood,* but that we have not yet even gone far enough in our inquiries to be enabled to construct a standard formula for our starting-point. Even with regard to venous blood, which has been examined over and over again, and the normal composition of which, in a medical point of view, is of vast importance, how unsatisfactory is our knowledge; for if we compare the results of about seventy analyses, made by Simon, Lecanu, Dennis, Nasse, Becquerel and Rodier, Andral and Gavarret, Fricke, and others, we may perceive that there are many remarkable discrepancies, all of which must be cleared away, before we can ever expect to make a satisfactory application of chemistry to the study of disease. Looking at the

* Beclard has been recently occupied in making a chemical examination of the blood, taken from different parts of the system; and he states, that the arterial blood has the same composition in every part of the body; but that venous blood presents great diversity in this respect. He found, for example, that, while the blood from the external jugular contained 79·4 of albumen, and 141·72 of globules and fibrin, that from the splenic vein contained 124·4 of albumen, and 128 of globules and fibrin; and that from the vena portæ, 70·0 of albumen, and 227·1 of globules and fibrin. So that it would appear from these results, that, contrary to the opinion of Ecker and Kölliker, the spleen destroys the globules, while the reverse action occurs in the vena portæ.

general results of the above-named analyses, we find that the specific gravity of the blood may vary from 1049 to 1060; and in 1000 parts of healthy venous blood, the proportion of water has been variously estimated at from 771 to 822, the solid constituents from 178 to 229, the globules from 106 to 155, the albumen from 59 to 77, the fibrin from 2 to 3, and the salts and extractives from 6·5 to 14 parts. Where, therefore, we would ask, are the correct data for the guidance of the physiologist and the physician? Whose numbers, among the whole, are we to regard as the most correct? or would it be better to take the mean of all the analyses, and thus expose ourselves to what some consider to be the very certainty of error? Into one of these positions, however, we must place ourselves; for it is absolutely impossible to proceed in our pathological inquiries, without having some standard for comparison; and being in this dilemma, we have thought it right to amalgamate the differences, and to construct a table out of the whole of the analyses which have been presented to us. The following is the result thereof:

	Venous Blood of Man.	Venous Blood of Woman.
Water	790·	798·7
Solid residue	210·	201·3
Blood-corpuscles	130·93	120·8
Albumen	69·59	68·61
Fibrin	2·46	2·36
Salts and extractive	8·96	8·9

The second table shows that the amount of the solid constituents in the blood of woman is a little below that of man; a fact which has been noticed by almost every investigator; indeed Becquerel and Rodier have made an especial allusion to it, saying, "that the influence of sex is so great, that in order to arrive at correct conclusion with respect to the deviation of morbid blood from the healthy standard, it is necessary to compare diseased blood with blood taken from healthy individuals of the same sex." Again, it has been found that other circumstances may exert a marked influence on the composition of this fluid. It has been remarked, for example, that the blood is always richer in solid elements, and particularly in blood-corpuscles, in persons of robust constitutions, and in those of sanguine temperaments. It has also been noticed that these constituents are less abundant during early life, that they are at their maximum from maturity to the period of middle age, and that they again diminish in quantity as the individual advances in years.

But we will now turn to the consideration of another most important animal liquid, viz. Urine, a secretion which has occupied the attention of almost every labourer in the field of physiological chemistry; for the knowledge of its chemical composition has, at all times, been regarded as one of the greatest desiderata of science. Physiologists and pathologists are aware, to use the expression of Dr. Golding Bird, that the kidneys pump from the blood all excess of water, that they remove the metamorphosed products of effete tissues or mal-assimilated food, and that they get rid of the many noxious matters, which are either produced in the system by disease, or admitted thereto by accident or design; that these organs are, in fact, the great depurators of the system, the exponents of the numerous chemical changes which are constantly taking place both in the healthy and diseased animal fabrics. It is, therefore, of the utmost importance that chemists should be agreed with respect to the normal composition of the renal secretion. A review, however, of the great body of facts which are already before us, will show that the composition of healthy urine varies under the commonest circumstances of life, and that this desirable object cannot be satisfactorily effected. It will be seen, for instance, if we direct our attention to the specific gravity of normal urine, that its density may vary from 1004 to 1036 without the individual passing beyond the boundaries of health. And as regards the proportions of its various solid constituents, they may range from 9·6 to 82·7 per 1000; the urea from 7· to 33·, the uric acid from ·4 to 1·, the extractive matters from 8· to 17·, and the fixed salts from 3·7 to 16. Much of this difference, however, is due to the constantly varying proportions of water in which the solid matters are dissolved; and hence the discrepancies are more apparent than real; for if we make examinations of all the urine which is passed by an individual during several periods of twenty-four hours each, it will be found that the amounts of solid matters are pretty uniform. These amounts, however, do vary somewhat in different individuals, and hence arises the difficulty in forming a normal standard of healthy urine. Nevertheless, Dr. Golding Bird has, from a great number of observations, stated that from 600 to 700 grains of solid materials are daily excreted by ~~the~~ kidneys of a healthy person. These may be mixed, as we have already said, with varying proportions of water, and may hence give rise to the apparent discrepancies to which we have just alluded. But in order to avoid these sources of error, and at the same time to give the physiologist and pathologist an opportunity of knowing what amount of work has been done by the kidneys during any given period of time, different formulæ have been constructed, all of which are founded upon the fact that there is a pretty constant relation between the specific gravity of this secretion, and the amount of solid matters contained in it. Thus, in Dr. Henry's formula, it is proposed to multiply the difference between the specific gravity of the urine and that of distilled water by 2·58, and to regard the product as the number which represents the grains of solid matter contained in 1000 grains of this secretion. Dr. Becquerel multiplies the difference by 1·65, and Dr. Christison, Dr. Day, and Dr. Golding Bird have adopted 2·33 as the multiplicator. Proceeding on this principle, Dr. Golding Bird has constructed a very useful table, which exhibits the relation that exists between the specific gravity of urine, and the amount of solids contained in a fluid ounce of it. As Dr. Golding Bird states:

> "A glance at this table presents us with a mode of recollecting the quantity of solids existing in urine of different specific gravities, when the table is not at hand for reference—a piece of short memory of no small service in practice. Thus, if the specific gravity of any specimen of urine be expressed in four figures, the last two will indicate the quantity of solids in a fluid ounce of the urine; within an error of little more than a grain, when the density does not exceed 1·030; above that number the error is a little greater. To illustrate this, let us suppose we are called to a patient, the integrity of the depurating functions of whose kidneys we are anxious to learn. The quantity of the urine excreted in twenty-four hours amounts, we will suppose, to 3 pints, or sixty ounces, and the density of the mixed specimens passed in the time alluded to is 1·020; now we merely have to multiply the number of ounces of urine by the last two figures of the specific gravity, to learn the quantity of excreted; or 60 × 20 = 1200 grains of solids. If the table were at hand the calculation would be more rigid, for we should then multiply 60

by 20·79, instead of 20; the product 1247 grains shows that by the former mode an error of 47 grains has been committed; an amount not sufficient to interfere materially with drawing our inductions at the bedside, and of course capable of immediate correction, by referring to the table at our leisure." (Fifth Lecture, p. 145.)

The observations of Lecanu lead to the conclusion, that the total amount of solid matters excreted by the kidneys is greatest in men, less in women, still less in children, and least of all in old persons. This is partly indicated by the following table:

	Men.	Women.	Children.	Old persons.
Urea	432	294	207	125
Fixed salts	260	222	135	124

And with respect to the proportions of the various constituents of this solid matter, it may be said, taking the mean of twenty-five analyses performed by Berzelius, Simon, Lehmann, Becquerel, Marchand, and Day, that it consists, in the 100 parts, of—

Urea	44·50
Uric acid	1·50
Extractive matters	24·20
Alkaline chlorides	10·25
Alkaline sulphates	12·35
Alkaline phosphates	5·40
Earthy phosphates	1·84
	100·04

This table, however, must be admitted with a little reservation; for the researches of Lecanu show, that, though the quantity of urea and uric acid excreted by the same person, during equal periods, is pretty constant, still the proportions of these substances, as well as of the extractives and salts, vary in *different* individuals, without reference to their age or sex. In fact, Lecanu has found, that the amount of earthy phosphates secreted in twenty-four hours by different persons, may vary from 30·3 grs. to less than half a grain, and that the amounts of alkaline chlorides may alter from 116 grs. to ·25 of a grain.

Much of this difference may arise from alterations in diet, exercise, &c.; for Drs. Lehmann, Simon, and Percy have remarked, that both of these circumstances exert a remarkable influence over the proportions of the various solids contained in the urine. The experiments of Lehmann, for instance, show that a purely animal diet causes an increase in the entire amount of solid matter, urea, and earthy phosphates, while it occasions a decrease in the quantity of extractives. A strictly vegetable diet, according to this author, has an opposite effect; for it diminishes the proportion of the urea, and increases that of the extractive. And a non-nitrogenous diet of oil, starch, and sugar, gives rise to a rapid destruction of the general health; the urea, uric acid, and fixed salts being still excreted in very nearly the usual proportions. Lehmann has further shown, from experiments made upon himself, that active exercise causes an increase in the amount of urea and alkaline sulphates; and that it diminishes the proportions of uric acid and extractives. These facts are clearly shown in the following table, which has been deduced from Lehmann's researches.

	Mixed diet.	Animal diet.	Vegetable diet.	Non-nitrogenous diet.	Active exercise.
Absolute weight of the urine in 24 hours	1057·8	1202·5	909·	1045·	982·6
Specific gravity	1022·	1027·1	1027·5	—	—
Solid matters passed in 24 hours	67·82	87·44	59·23	41·68	82·59
Solid residue in 1000 parts	65·82	75·48	66·41	40·45	83·29
Per centage composition of the solid residue:					
Urea	64·23	61·29	39·08	36·93	54·86
Uric acid	1·71	1·67	1·73	1·72	0·77
Extractive matters......	16·64	5·81	29·48	28·43	10·23
Alkaline chlorides	5·59	5·39	5·44	4·65	—
Alkaline sulphates	11·14	11·38	11·09	7·46	18·21
Alkaline phosphates ...	5·81	5·85	5·64	6·57	5·56
Earthy phosphates......	1·74	4·05	1·73	2·27	1·33

On glancing at the views put forth by different chemists as to the composition of the urine, it will be seen that there is a great difference of opinion with regard to its acidifying principle; for some think that it is due to free phosphoric acid; others, as Liebig, that it arises from a superphosphate, in which uric acid is dissolved. Many suppose that it is dependent on free lactic acid, and a few that it is occasioned by sulphuric or hydrochloric acid. These different opinions will, we believe, be sufficient to show, that there are at present insuperable difficulties in the way of making a satisfactory formula of the composition of healthy urine, and that our knowledge may be summed up in a very few words, namely, that—

"The urine is most abundant in urea, uric acid, and the most important salts, in men in the prime of life. It is less rich in these constituents in women; while the minimum occurs in old men and children. The nature of the food exerts an influence upon the composition of the urine; the amount of urea is increased by an excess of nitrogenous food, and diminished after living on food deficient in nitrogen. Upon a diminution in the quantity of food, the urine becomes deficient in nitrogen, as has been shown by my own experiments and those of Lehmann; but the separation of nitrogenous compounds, as, for instance, urea through the urine, occurs when no food is taken. The urine contains more urea and sulphates after active bodily exercise, in consequence, doubtless, of increased vascular excitement. The quantity of urine discharged in twenty-four hours, amounts on an average to about forty-five ounces. It is more abundant in the prime of life than in old age or childhood, and in the male than in the female sex." (Simon, Animal Chemistry, by Dr. Day, vol. ii, p. 169.)

Our space will not permit us to refer to all the difficulties which are opposed to the construction of standard formulæ of composition for the other animal secretions. Indeed we can only allude to the fact, that chemists are not yet agreed as to the normal constitution of any of them. Even with regard to the composition of the Bile, a liquid which has been examined again and again, it may be seen that we have no certain knowledge; for Mülder considers it to be composed of but one essential organic element, namely bilin, a substance which, he says, is very prone to undergo decomposition, and which contains the elements of taurine, ammonia, and a primitive principle compounded of $C^{50}\ H^{36}\ O^{6}$; while M. Strecker, the friend and pupil of Liebig, has endeavoured to confute this opinion, by saying that bile is composed essentially of two salts, namely, the cholate and sulpho-cholate of soda, the latter being sometimes replaced, as in the bile of the pig, by another salt, the hyocholate of soda. Mulder's view of the composition of bile has, however, been adopted by Dr. Golding Bird, who states that—

> "The biliary secretion is most correctly to be regarded as consisting of a compound of an organic electro-negative body with soda, or bilate of soda. In addition to this, a small quantity of cholesterine and fatty acids, with mucous and a colouring matter, or *biliphaen*, are present. The colouring matter is very small in quantity,—just sufficient to give a yellow tint to the fresh secretion,—although, in disease, it is often formed in very large quantity, as we shall subsequently see. It may be regarded as an important means of eliminating carbon from the body, when the normal secreting functions of the liver are partially or wholly suspended. The composition of this pigment is, from the analysis of Professor Scherer, carbon 68·182, hydrogen 7·437, nitrogen 7·074, oxygen 17·261............
>
> "Bile-pigment is unquestionably a result of metamorphosis of blood, probably of old blood-corpuscles. It bears no small analogy to blood-pigment; and an Italian chemist, Polli, has lately suggested that they are producible from each other by processes of oxydation or reduction. He has drawn attention to the fact of blood and bile assuming an exactly opposite series of colours, by exposure to the air. The play of colours in blood he noticed in the well-known varying tints of an ecchymosis, and the changes of colour in bile-pigment he watched by simply exposing it to the air. According to his observation, the following changes of colour were observed:
>
> " Blood-pigment—black, violet, blue, green, and yellow.
> Bile-pigment—yellow, green, blue, violet, black."
>
> (Lecture III, p. 1022.)

Again, with reference to the composition of the Gastric Juice, it is impossible to fix a standard formula, for chemists are not even agreed as to the nature of the acidifying principle of it: some affirm that it is free hydrochloric acid, others that it is free phosphoric acid, or perhaps a superphosphate of soda. Schmidt states that it is a peculiar complex organic acid, which he terms hydrochloropepsic acid; while Berzelius, Bernard, Barreswill, Liebig, and Lehmann are disposed to think that this principle is free lactic acid.—Similar disagreements exist with regard to the composition of Saliva. In fact, the researches of Berzelius, Simon, Wright, L'Héritier, Hünefels, Mitscherlich, and Vogel merely go to show that this secretion contains about 1 per cent. of solid matter, of which chlorides, phosphates, and an animal substance are the chief constituents; some chemists, however, affirm that a sulphocyanide is also present, and Mialhe states that the digestive power of this liquid is due to an animal diastase, which Bernard says is derived from the mucus of the mouth.

In this manner we might proceed exhibiting the numerous discrepancies, and often positive contradictions, which occur in the analytical results of our inquiries into the composition of the various solids and liquids of the animal body. It is fortunate, however, that these discrepancies do not prevent us from arriving at certain general conclusions with respect to the chemistry of the healthy animal products, from which we are enabled to make a very great and a very useful application of chemistry to the study and cure of disease. Andral, for example, has shown that, not to regard minuter differences, the chemical reactions of the secretions are exceedingly constant. He states that the perspiration is always acid, and the sebaceous matter alkaline; that the fluid from every mucous surface is acid, but may be neutralized or made alkaline by admixture with some other secretion, as the salivary, pancreatic, hepatic, or the products of inflammatory action, in which case the serum of the blood is effused; the former occurs in the duodenum, and the latter may take place in any part of the alimentary canal, or upon any mucous surface, and it is always recognised by the presence of pus or altered mucus-globules. The secretions of the larger glands are for the most part alkaline; that, however, from the kidneys is acid; but if at any time the urine becomes alkaline, it results from the admixture of diseased or putrid products.

These considerations lead us to our Second question, namely, *whether or not we are acquainted with the changes which occur in the chemical composition of the solids and fluids of the animal body during the existence of diseased action?* And to commence with the Blood, it may be stated that many notable alterations are produced in the composition of this fluid by disease: we shall find, for example, that variations may occur in the proportions of the several normal constituents of it; secondly, that its composition may be altered by the presence of those substances which are destined for excretion; and, thirdly, that in all probability great changes are effected, in both its chemical and physical properties, by the introduction of matters derived from miasmic and similar morbific agents.

Among the normal constituents, *fibrin* is one of the most important, and the proportions of this substance have been found to vary in several diseased conditions of the system; thus, its amount is always increased during the existence of an inflammatory affection. This fact is particularly well illustrated in inflammations of the serous and cellular structures, and in no instances is it better seen than in cases of pneumonia, where, according to the results of Simon, it may amount to 6 parts in the 1000 of blood, though, according to the more extended inquiries of Andral and Gavarret, Becquerel and Rodier, it may rise to about 7·4 per 1000. In inflammation of the mucous membrane, however, it appears that the increase is not quite so large; for Andral and Gavarret have found that in bronchitis it only reached to about 6·16 per 1000, while Becquerel and Rodier say that in this disease it amounts to about 4 parts in the 1000.

It has been noticed, moreover, that there is always a direct relation between the proportion of fibrin contained in the blood, and the intensity of the inflammatory action. This fact is evidenced by the appearance of a buffy coat, a phenomenon upon which the physician has always been accustomed to rely with a great deal of confidence. In endeavouring to account for this increase in the proportion of fibrin during an inflammatory attack, Simon has referred to his well-known theory, namely, that the plasma of the blood serves for the nutrition of the body, while respiration and the production of fibrin is effected by the blood-corpuscles. In support of this opinion, which was originally advanced by Hewson, he alludes to the circumstance that the amounts of fibrin and globules are always in an inverse ratio to each other; and he says that in inflammation the blood-discs are in a state of increased activity, and that they are, therefore, subject to a more rapid metamorphosis. Mulder is of opinion, that during an inflammatory attack, a considerably larger quantity of protein, perhaps fibrin, is oxydized and retained in the circulation, and that when these oxides accumulate in any particular part of the system, local inflammation is the result. In the latter case, morbid products, such as false membranes, &c., are evolved, which are found on analysis to be composed almost entirely of oxydized protein. The buffy coat, according to this author, is formed whenever these oxides of protein predominate in the system.

Again, it has been remarked that the amountof fibrin is increased in the blood of individuals who are suffering from tubercular disease; and if we take the mean of 22 analyses made by Andral and Gavarret, and of 9 which were made by Becquerel and Rodier, it will appear that this increase amounts to about 4·4 per 1000, though, according to Simon, the blood of phthisical patients contains as much as 5 parts of it in the 1000.

The amount of fibrin is also increased during attacks of rheumatism, erysipelas, dysentery, and scurvy; the last fact was originally noticed by Mr. Busk, and though it was opposed to the observations of Andral and Gavarret, yet it has since been confirmed by the investigations of Parmentier and Deyeux, Becquerel and Rodier, Chatin and Bouvier, Marchall, Fricke, and even by the more recent inquiries of Andral himself.

The diseases in which the amount of fibrin has been found to be deficient, are those in which there is a low and typhoid condition of the system, where, in fact, the blood appears to have been poisoned by the introduction of some morbific agent, as, for example, in typhus, cholera, plague, intermittents, and in many of the exanthemata. Its quantity is likewise diminished when there is a defective state of the nutritive powers, when the blood is said to be poor, and when there is a tendency to hemorrhagic effusions, as in purpura, &c. Its proportions are also lowered when there is a drain upon the system, as during pregnancy and attacks of albuminuria.

With regard to the alterations which take place in the proportions of the *blood-corpuscles* during disease, Andral and Gavarret state that their amount is increased during a plethoric state of the system. This, however, has been contradicted by Becquerel and Rodier, who say that the relative proportions of globules are not altered during this condition of the body, but that there is an increase in the total amount of blood. Lecanu affirms that the quantity of blood-discs is augmented during an attack of cholera, and the same thing has been said to occur in dysentery. If this be true, it is doubtless owing to the separation of the aqueous portion of the liquor sanguinis. On the other hand, all investigators are agreed that the amount of blood-corpuscles is diminished in inflammatory affections, in phthisis, diabetes, intermittent and puerperal fevers, lead poisoning, chlorosis, anæmia, Bright's disease, in most carcinomatous affections, and in those conditions of the system where the nutritive functions are badly performed.

Mr. Nicholson and Dr. Glover have, moreover, stated that the amount of corpuscles is below the average in the blood of patients who are suffering from attacks of scurvy.

A third constituent of the blood, *albumen*, does not appear to undergo a very marked alteration in its relative proportions during the existence of disease; this may account for the fact that the specific gravity of the serum is generally at about the same point, namely, 1025.

Simon, however, has stated that the quantity of albumen is increased in most inflammatory affections; but his statement has not been verified by the more recent observations of Becquerel and Rodier, Andral and Gavarret. Other investigators affirm that the proportion of albumen is diminished in intermittents, dysentery, puerperal fever, scurvy, and indeed in all low typhoid conditions of the system, where there is a tendency to dropsical effusions, and where albumen is largely excreted by the kidneys. Judging from the observations of Becquerel and Rodier, its amount may be reduced in Bright's disease to 60 parts in the 1000, while, according to those of Andral and Gavarret, it may even sink as low as 50.

As to the alterations which occur in the *fatty constituents of the blood*, it may be said that they are often increased to so great an extent as to give a milky, or even a purulent, appearance to the serum; but though this is not always dependent on disease, yet many observers have reported that the fatty matters are increased in quantity in diabetes, in albuminuria, in delirium tremens, in spirit-drinkers, in those affected with disease of the liver, in cholera, and in some forms of inflammation.

And, lastly, with reference to the *saline constituents* of the blood, Scherer says that, they are in excess in typhus, and Jones, that they are above the normal standard in scurvy. In fact, Andral and Gavarret, Magendie, Aldridge, and others, have remarked that the *alkaline salts* are considerably increased in this disease; in one case of Dr. Aldridge's, they even amounted to double the natural quantity. Dr. Garrod, however, is of opinion, that in scurvy there is a deficiency in the *salts of potash* in the blood. This, indeed, is the view which he has taken of the pathology of this disease, and he has endeavoured to support it with a great deal of ingenuity, by stating that potash is always deficient in scorbutic diets, and that the disease is apparently cured by the use of such substances as contain a larger proportion of this mineral; but it is needless to observe that his hypothesis cannot be established upon such narrow grounds, and that it is necessary, before we can even regard it in the light of a possibility, to show, by a series of analyses, that the blood of scorbutic patients is actually deficient in this alkali. (See vol. ii, p. 467 et seq.)

With respect to the second part of our proposition, it has been found that the blood often contains an excess of those effete elements which are the products of decomposed tissues, &c. Under ordinary circumstances, these products are excreted by various depurating organs, as the liver, kidneys, lungs, skin, &c.; but when these organs are diseased, and cease to perform their duties in a proper manner, the blood becomes charged with their respective secretions. In this way *urea* may accumulate in the blood, as happens in Bright's disease, in acute inflammatory affections of the kidneys, and when the secretion of urine is almost or altogether suppressed. Dr. Garrod states that he has found as much as 1·142 of it in the blood of a cholera patient, and Pasquale says that he obtained 0 684 parts of urea from 1000 of blood in a case of albuminuria.

The same affections may also give rise to the accumulation of *uric acid* in the blood. It is also stated that this substance exists in great excess in the blood during an attack of gout.

Again, diseases of the liver, attended with a suppression of its secretion, will give rise to the appearance of *bile*, or of its colouring principle, in the blood, as we see in jaundice, &c. And, lastly, there are many reasons for believing that noxious compounds accumulate in the blood of those who are attacked with fever, cholera, plague, the exanthemata, &c. It is probable, moreover, that these noxious compounds are produced in the blood by reason of the catalytic action of some miasm, poison, or decomposing agent, which has been introduced from without. This, in fact, is the great hypothesis of Liebig, and we shall have occasion to discuss it somewhat fully hereafter.

The researches of many chemists have, moreover, proved that sugar exists in the blood of individuals who are suffering from diabetes.

Let us now turn to the facts which have been made out with regard to the chemical pathology of the Urine, a secretion which, as we have already stated, is constantly participating in the changes going on in the animal economy, and therefore capable of furnishing the physician with some of the best evidences of disease. These evidences may be divided into two kinds; as, first, those which exhibit the alterations which occur in the normal constituents of the urine; and, secondly, those which indicate the changes which are produced in it by the introduction of new or foreign substances, as, for example, the products of diseased action, or the constituents of other secretions.

We have before remarked that it is not an easy thing to judge of the work which is being done by the kidneys at any given time, in consequence of the variations which are ever occurring in the relative proportions of the water and solid constituents excreted by these organs. In order therefore to form a correct opinion upon this important matter, it is necessary to collect all the urine which has been passed during a period of twenty-four hours, to measure it, and to ascertain, by means of experiment, the amount of solids contained therein. Becquerel states that the increase or diminution in the amount of the urine cannot be referred to diseased action, except less than twenty-seven, or more than fifty-two ounces of it are excreted in one day. And with respect to the amounts of solid matters which indicate disease, Dr. Golding Bird says that—

> "Although certain peculiarities connected with muscular exercise, regimen, and diet, as well as certain idiosyncrasies of the patient, may influence the amount, yet if we regard 650 as the average expression of the number of grains of effete matter excreted in twenty-four hours by the kidneys, we shall not commit any very serious error. In calculations of this kind much latitude must be allowed, and it ought, at least, to be assumed that the kidneys may excrete fifty grains, more or less, than the assumed average, without exceeding or falling short of their proper duty." (Lecture V, p. 145.)

Simon states that the diseases in which the quantity of water separated by the kidneys is absolutely or relatively increased, are diabetes in its different forms, and certain hysterical or nervous disorders, in which a perfectly limpid and thin urine is discharged in very large quantity; thus Becquerel relates a case of a young chlorotic girl who ordinarily passed about thirty-seven ounces of water daily, but in whom the amount rose to ninety ounces upon the accession of a severe hysterical attack.

And, to quote again from Simon, "the amount of water separated by the kidneys is diminished in inflammatory affections," in which Becquerel has seen it fall as low as twelve ounces in twenty-four hours.

In these cases the urine is of a very dark colour, of a high specific gravity, and it possesses a strong acid reaction. As the quantity of water increases, the solid constituents relatively, but not always absolutely, diminish, as may be found by comparing them with the amount secreted in twenty-four hours in a state of health.

Again, the flow of urine is always remarkably diminished, and often totally suppressed, during an attack of cholera.

As to the *urea*, it has been found that its amount is generally increased during the existence of fever, or any inflammatory affection of the system. This circumstance has been especially referred to by Nysten, and although it is in a measure opposed to the results obtained by Becquerel, yet it is manifest that the conclusions which have been arrived at by Nysten are perfectly correct; for, as Simon states, if we remember that, in acute inflammatory diseases, only very small quantities of nitrogenous foods are taken, and that the quantity of urea must necessarily decrease under such a diet, we may regard it as increased, if it falls a little below the physiological average. Now Simon has found that in these diseases the urea is either above or equal to the quantity secreted in health. Prout and other investigators have remarked that the proportion of urea is increased when there is any derangement of the digestive organs.

On the other hand, the amount of urea is diminished in those diseases which tend to impoverish the blood, as, for example, in anæmia, chlorosis, diabetes, albuminuria, scrofula, ascites, and in many diseased conditions of the liver. Rose, in fact, affirms that in several cases of acute and chronic hepatitis he has found the urea to be entirely absent from the urine, and the same observation has been made by Henry and by Coindet; indeed Coindet states that the urea is replaced on these occasions by a substance which resembles bilin. The experiments of Simon and of Becquerel, however, do not confirm this statement.

Again, the amount of urea is said to be deficient in hysteria; and, according to Simon, it is considerably below the natural standard in typhus and in cholera. This last observation is confirmed by the analyses made by Heller and by Herrmann, and more recently by the inquiries of Dr. Letheby.

A third constituent of the urine, *uric acid*, is, like urea, commonly present in abnormal quantities in inflammatory affections, and during the paroxysms of fevers. It is also abundant in the urine, in phthisis, in rheumatism, and in all those diseases where the function of the skin is interfered with, as, for example, in the exanthemata, in diabetes, and after exposure to cold.

The increase in the amount of this acid, during inflammatory and febrile states of the system, is remarkably great. In pneumonia, Simon has found it increased to about 3 per cent., and Becquerel to about 6. The fact of the urea being in excess in phthisis, is a circumstance of great importance to the pathologist; and we may state that it has been especially referred to by Simon, Becquerel, and Schönlein.

An abundant lateritious deposit in the urine, at the crisis of an intermittent, was regarded by the old physicians as a great characteristic of this disease. At present, however, such a condition of the urine is not commonly witnessed; indeed Schönlein and Becquerel say, that the appearance of a sediment in the urine at the termination of a paroxysm is by no means a constant occurrence. Andral has made a similar observation; and he states, moreover, that such deposits only occur when the fever is complicated with functional derangement of the digestive organs. These apparent contradictions are referred to by Dr. Golding Bird, who says:

> "I have no doubt that all who have perused the Hippocratic writings, and even those of a more modern date, have been struck with the apparent obvious relation between the termination of some phases of disease, and a *crisis* by urine; and have, perhaps, like myself, wondered at their not having observed these things for themselves. I think, however, that this admits of a ready explanation, for on referring to the history of fevers, earlier than the last century and a half, no one can fail to notice the fact of the remarkable tendency to periodicity they exhibit; indeed we might almost doubt whether a genuine continued fever was then known. The greater prevalence of malaria, arising from large extents of unreclaimed forest-land and marsh, will explain this, and I hope to produce evidence, ere we part to day, that a genuine crisis is really indicated by the urine in malarious fevers." (Lecture V, p. 143.)

Many physicians have also remarked that there is usually a lateritious deposit at the period when patients begin to recover from typhus. This, as Simon states, has led to several important questions, as to whether urinary sediments in typhus occur at any fixed epochs of the disease? whether there is any connexion between their appearance and a favorable result? and whether their appearance is prognostic of such a result? Some physicians, as Willis and Schönlein, are disposed to answer these questions in the affirmative; while, according to Becquerel, the appearance of a deposit in the urine affords no evidence whatever of the progress of the disease; for, from a table which he has drawn up, it appears that some persons have died, notwithstanding that the urine deposited sediments at all periods of the disease; and Andral has made a similar observation.

It has been found, moreover, that uric acid is excreted in great abundance in many forms of dyspepsia, especially the inflammatory; and that it appears in large quantity in the urine of those who are recovering from attacks of gout, and in those who are affected with disease of the liver.

According to Becquerel, its proportions are also increased when there is great functional disturbance of the system, such as arises from oppressive dyspnœa in pulmonary emphysema, or cardiac disease, acute pain, convulsions, delirium, &c.

The amount of uric acid is, however, generally diminished in all affections which tend to depress the vital powers, and to exhaust the pabulum of the blood, as, for example, in anæmia, chlorosis, scrofula, hysteria, in advanced stages of Bright's disease, and in the low condition which succeeds to an attack of fever.

With regard to the alterations which occur in the proportions of the *fixed salts* in the urine, we have but little accurate information, though, according to Simon, we may, generally speaking, assume that the quantity of salts decreases in most pathological states of the body; the cases in which the salts increase during disease being very rare. Becquerel states that, in those diseases in which the amount of urea is only slightly diminished, the proportions of salts are not materially affected; but that in those cases in which the urea suffers considerable reduction, the same takes place with regard to the salts. Analyses of inflammatory urine, however, are opposed to this statement; since in these cases the urea sometimes exceeds the normal amount, while the salts are decreased in an extraordinary manner.

Scherer says that they are diminished in typhus; Simon, that they are wanting in scrofula and in diabetes; and Vogel and Letheby, that their proportions are reduced in cholera.

Another important point, which is still open to the inquiries of future investigators, is an accurate examination of the variations which occur in *the relative proportions of the several saline constituents* of the urine during disease; for at the present time our information upon this subject is remarkably imperfect. A few examinations, however, have been made by Simon, Becquerel, and one or two others; from which it appears that the *chlorides* are always deficient in typhus, in cholera, and in most inflammatory diseases. Vogel states that the *sulphates* are in excess during an attack of cholera, and that the *calcareous and magnesian salts* are very much reduced. But these observations have not been confirmed by the more recent analyses of Dr. Letheby, who found, in one case, that the sulphates were very much below par, while the proportions of the calcareous and magnesian salts were not much altered. Lastly, it is said that the *phosphates* are increased in amount in scrofula, mollities ossium, in irritable states of the nervous system, and in acute inflammatory diseases of the brain and spinal cord. Dr. Bence Jones, who has devoted considerable attention to this subject, says that the alkaline phosphates are excessively increased in all acute diseases of the nervous centres, and in some functional disturbances thereof; but he remarked that they were again reduced to their normal standard when the diseases became chronic, and when the functional derangements had subsided. His inquiries have, moreover, established the fact, that the proportions of these phosphatic salts are not increased in fevers, in acute inflammations of the fibrous, muscular, cellular, or cartilaginous tissues, nor are they altered in paralysis, mania, melancholia, or in any other chronic derangement of the nervous system.

It may be here mentioned that the appearance of a phosphatic deposit in the urine, is not by any means indicative of a superabundance of these salts. On the contrary, such a deposit may occur even when the phosphates are deficient in the urine, for the precipitation of them is due to the decomposition of urea, whereby ammonia is generated; and this by neutralizing the free acid of the urine, and combining with the earthy phosphates, occasions the deposit in question. This phenomenon has been witnessed in many functional derangements of the nervous system, and also in catarrhal affections of the urinary passages, and in the later stages of typhus fever.

Dr. Bence Jones has found, from the results of his inquiries, that the proportion of phosphates is sometimes diminished in delirium tremens, as when the patient does not partake of any food. And, lastly, Simon has stated that the phosphates are deficient in the urine in chronic albuminuria.

A consideration of the nature and proportions of the *extractive matters* of diseased urine, leads to the conclusion that these matters may often contain compounds which are foreign to the constitution of normal urine; but as our knowledge with regard to the composition of these substances is very imperfect, and as we are totally ignorant of the nature of the changes to which their various organic components are subject during different conditions of the system, we cannot at the present time attempt to point out the relations which exist between the chemical composition of these extractives and the various diseased states of the body. In fact, our information will only permit us to say, that the proportion of the extractive matters in urine is often considerably altered during the occurrence of disease. It is, for example, increased in scurvy, in typhus, in cholera, and in the exanthemata. It does appear, moreover, from the peculiarity of the reactions which are generally manifested in the urine on these occasions, that its nature is also changed, and that there is a foreign, and, as it were, a readily decomposable substance contained therein. The ancient fathers of medicine made a similar observation; and it led them to the belief that, during the existence of disease, a morbid substance was generated in the blood, and excreted by the kidneys. Upon this belief depends much that has been written about crisis by the urine, and the appearance of this secretion in different phases of disease. In fact, "it was to these critical features of the urine that the attention of our forefathers was especially directed, more particularly in those diseases which certainly better bear out the theory of *zymosis* than any other: I mean," says Dr. Bird, "the whole class of fevers." And not despising this empirical knowledge of the ancients, but rather attaching a degree of importance to it, Dr. Golding Bird has brought it to the test of modern science, and he has succeeded in proving that their statements with regard to a crisis by the urine are founded in fact. In evidence of this he refers to some cases of intermittent fever which were treated by him in the wards of Guy's Hospital, and he prefaces these cases by saying—

> "If I can prove that, concomitantly with an enormous increase in the excretions of the kidneys, sudden improvement occurs in a patient—which change for the better does not take place until this great change is observed—I think it will be conceded that I shall produce sufficient evidence to show that the observations of our predecessors were correct, and that something like a critical excretion from the kidneys does take place, at least in the diseases which have been sufficiently carefully studied in this point of view." (Lecture III.)

In the first case of ague which is referred to by Dr. Golding Bird, the patient was kept in the hospital from the 23d of May, 1845, to the 16th of June following, when he was considered to be convalescent, and made an out-patient of the hospital.

The following is a tabular view of the examination of the urine of this patient, from the Reports of Mr. Howard Johnson:

Date.	Quantity passed in 24 hours.	Specific gravity.	Action on litmus.	Weight of solids present.
May 23	12 fluid oz.	1·028	Acid, pink deposits	352 grs.
„ 26	40 „	1·020	„ „	828 „
„ 28	35 „	1·020	Acid, no deposit	725 „
„ 30	48 „	1·020	„ „	1054 „
„ 31	45 „	1·016	„ „	743 „
June 2	35 „	1·014	Alkaline	514 „
„ 4	30 „	1·028	Acid, pink deposits	879 „
„ 6	27 „	1·034	„ „	1036 „
„ 7	35 „	1·013	Acid, no deposit	436 „
„ 9	40 „	1·028	„ „	1172 „
„ 11	45 „	1·016	„ „	742 „
„ 13	40 „	1·022	„ „	916 „
„ 14	43 „	1·022	„ „	984 „
„ 16	37 „	1·027	„ „	1044 „

This case was treated with sulphate of berberine. There was a decided improvement in the patient on the 30th; he had a severe paroxysm on the 3d; he was much better again on the 6th; and there was no return of the ague after the 9th. In commenting upon this case, Dr. Golding Bird says:

> "It must be admitted that there exist, to say the least, some curious coincidences between the free action of the kidneys, *quoad* the excretion of solids, and the improvement of the patient. The unusually large quantity of solid constituents removed by the kidneys of this patient is remarkable, and certainly very unfrequent." (p. 147.)

In the second case of ague to which Dr. Golding Bird refers, the patient, a girl, aged 13, was kept in the hospital from the 23d of May, 1845, to the 7th of June. On the 24th of May there was a severe paroxysm, and the amount of solids excreted by the kidneys was 280 grs. On the 27th there was also a paroxysm, and the solids amounted to 375 grs. On the 28th the patient was better, and the solids amounted to 538 grs. On the 30th they amounted to 625 grs.; and from this time the patient rapidly got well; so that here also, "*pari passu* with the patient's improvement, a gradual increase occurred in the solids excreted by the kidneys."

It is highly probable that a careful examination of the solid matters which were voided in these cases would have shown that the increase chiefly occurred in the extractive, for it does not appear that the other constituents of the urine are ever subject to such an enormous alteration in their proportions; and doubtless this extractive consisted for the most part of a vitiated pabulum, the product of diseased action.

Again, it has been remarked by most practical physicians, that the urine in typhus, and in many of the exanthemata, is very much disposed to become putrid and alkaline. Becquerel, Willis, Pelletan, Simon, Schönlein, and others, have referred to this fact, saying, that the rapidity and facility of this spontaneous decomposition usually corresponds with the severity of the disease. From the observations which were made by Pelletan, in Bouillaud's 'Clinique,' he remarked that, during the first days of typhus, the urine was of a dirty yellow colour, and transparent; during the whole of the first stage, it was always more or less acid, and the darkest kind, which has an odour like gingerbread, was usually most acid. At a later period it changed, resembled turbid whey or putrid broth, and it was usually neutral; it was also sometimes found of a dark colour, with an odour like cowdung. At a still later period it was turbid, putrid, smelt rather ammoniacal, and assumed at the same time a corresponding reaction. If the disease took a favorable turn from this period, the urine went through the same changes in a reversed order.

And Simon states that the observations which he has made in Schönlein's clinical ward, correspond entirely with those already communicated. Schönlein himself states, that in the mild form of typhus which prevailed in Berlin in 1843, he noticed these changes in several cases, and, in fact, when from being alkaline, the urine again became acid, and at the same time clear and abundant, there was scarcely any risk in giving a favorable prognosis. (Simon, vol. ii, p. 245.)

All these facts tend, in our opinion, to show that in most diseases of a low, typhoid, putrid, and zymotic character, there is an effete animal substance, doubtless the product of disease, thrown off by the kidney in the form of altered extractive matter.

A somewhat similar condition of the urine has been observed by Erlenmeyer, Simon, Becquerel, Henrich, Sutherland, and Rigby, in cases of insanity. May not this also be due to the presence of an altered form of extractive, which, rapidly undergoing chemical decomposition, acts upon the urea, and gives rise to the formation of ammonia, which presents itself in the urine as carbonate, lithate, muriate, or magnesian phosphate?

Moreover, it is not at all improbable that the sulphur-extract of Mr. Ronalds may undergo a change in its chemical composition during certain diseased states of the system, and so give rise to the production of cystin or cystic oxide.

> "I will venture to guess," says Dr. Golding Bird, "that it may be an amorphous form of this body. I am quite sure that cystin is by no means so rare as has been supposed; a few crystals of it are by no means uncommon in the urine, especially in people of low powers or strumous diathesis. A talented provincial physician, Dr. Shearman of Rotherham, has more particularly drawn attention to this fact. The credit of showing it in the urine of chlorotic girls is entirely due to him. Not long ago, Dr. Shearman kindly placed into my hands the urine passed by a strumous child labouring under pneumonia, which was actually loaded with cystin." (Lecture IV.)

From these, and our preceding remarks on the extractive matters of the urine, it will, we think, be evident that neither the chemistry, the physiology, nor the pathology of these most important substances is at all understood, and that a more careful study of the chemical constitution of these matters offers a wide field for the labours of future investigators.

Albumen, another important morbid constituent of the urine, was, like cystin, for a long time confounded with the extractives of this liquid. In the proper and healthy condition of the animal body, this substance never appears in the renal secretion; and notwithstanding that it occasionally occurs therein in certain erratic conditions of the system, which can hardly be termed diseased,—as, for example, during pregnancy, after exposure to cold, and after a too free indulgence in alcoholic liquors,—yet these exceptions are not of sufficient moment to prevent us from saying, that the frequent appearance of albumen in the urine is a symptom of the greatest importance, for it indicates, not that a vicarious or complementary action of the kidneys has been set up, but that a very serious alteration has taken place in the structure of these organs. Such a condition of the urine occurs in the congestive stage of fevers, during many inflammatory affections, especially of the skin, and in those diseases which are attended with dropsy of the abdomen and lower limbs. Schönlein says that albumen is also present in the urine in the early stage of diabetes, but that as the disease advances the albumen gradually disappears, giving place to sugar, which constitutes the true abnormal product in this disease. An albuminous urine is occasionally excreted in typhus; and Bouchardat, Nysten, and many others, affirm that albumen occurs in the urine of women who are suffering from puerperal fever. But the great occasions on which albumen presents itself in this secretion are, firstly, during that granular condition of the kidneys known as Bright's disease; and, secondly, during the period of desquamation which succeeds to an attack of scarlet fever. In both of these instances the physician anxiously watches for every alteration which may occur in its amount, being certain that the proportion of albumen present in the urine, is strictly indicative of the amount of mischief done to the organs which excrete it. There appears, however, to be some doubt with regard to the period at which albumen makes its appearance in the urine of patients who are suffering from scarlet fever; for Becquerel states that the presence of a small quantity of it is not by any means uncommon during the *febrile stage* of the disease, though most other observers are of opinion that this substance exists in the urine only during *the period of desquamation.* Dr. Begbie has recently devoted some attention to this subject, and he has recorded the results of his examinations in the urine in twenty-one cases of scarlet fever which were not followed by dropsy. In all these cases albumen was recognised by the usual tests, and with two exceptions it appeared in the urine within a few days after the commencement of desquamation. In the excepted cases, albumen was present from the setting-in of the fever; but he states that the proportions of it were very small, and that its presence was accounted for by the existence of blood in the urine in one case, and of pus in the other. Dr. Begbie moreover remarked, that in the excepted cases the amounts of albumen were considerably increased during the periods of desquamation; so that the issue of these cases cannot be said to be opposed to his general results. In this respect the observations of Dr. Begbie correspond with those made by Solon, who found that albumen existed in the urine in twenty-two out of twenty-three cases of scarlatina which he examined; and they also accord with the *general* conclusions of Becquerel and of Simon.

With regard to the *duration* of the presence of this substance in the urine, Dr. Begbie found that the shortest period was forty-eight hours; and excluding the two cases to which we have referred, the longest period of its continuance was ten days. In speaking of the *amount* of albumen which was thus present in the urine of his patients, Dr. Begbie says: "I never found the urine of simple scarlatina highly coagulable; in several cases, especially in those in which the albumen appeared, for the first time, at a late period of the disease, it was only faintly coagulable." He thinks, moreover, that the albumen appears earlier, and continues longer, in the severer cases; so that even here, as we have before said, the quantity of albumen and the duration of its existence afford good signs of the intensity of disease. But there are cases of scarlet fever in which albumen is said to be entirely absent from the urine; in fact, Philipp states that in the scarlatina which was so prevalent in Berlin during the years 1839 and 40, and in which anasarca could not be warded off, he observed at least sixty cases where the urine was tested both with heat and nitric acid, without affording any indication of the presence of albumen. Similar instances have been recorded by Simon and by Becquerel; they do not, however, invalidate the great rule to which we have referred.

Dr. Bence Jones states that he once met with a peculiar jelly-like substance in the urine of a patient who was suffering from mollities ossium, and that it existed therein to the amount of 67 parts in the 1000 of the secretion; the nature of this substance, however, was not precisely determined.

Other substances of an albuminous character have also been detected in the morbid renal secretion; thus *blood* is a frequent constituent of it at the commencement of Bright's disease, and during the febrile period of scarlatina. This substance may also be present in the urine in acute inflammation of the kidney, and when there is a breach in any part of the urinary passages.

Again, *mucus* and *epithelium* commonly exist in the urine in albuminuria, and during the period of desquamation in scarlet fever; in the latter case their presence is indicative of an exfoliation of the mucous membrane of the bladder and kidneys; and, according to Simon, these substances should always be sought after with as much care as albumen. Mucus-globules, epithelium, and *pus* are frequently present in the urine, when there is disease of the urinary passages; and at these times the urine commonly contains albumen likewise.

Zimmermann states that he has met with *fibrin* in the urine in some inflammatory cases, as of pneumonia, pleurisy, and endocarditis.

Lastly, a peculiar protein substance (*kiestein*) has been so frequently detected in the urine of pregnant women by Nauche, Golding Bird, M'Pherters and Perry, Letheby, Lehmann, Sir Robert Kane, Audouard, and many others, that its occurrence therein may be regarded as a very important sign of pregnancy.

Fat has likewise been discovered in the renal secretion in many cutaneous affections, in phthisis, in albuminuria, in tabes mesenterica, and, according to Simon, in those diseases in which there is a rapid loss of substance and of force.

In certain diseased conditions of the body, a *saccharine substance*, allied to grape-sugar, presents itself in the fluids excreted by the kidneys, and it is always indicative of a very serious condition of the system,—one, in fact, in which the products of digestion are either badly prepared, or imperfectly appropriated, and in which the animal fabric is slowly broken up and robbed of its most important materials. We shall hereafter discuss the views which have been taken of the pathology of diabetes.

The occurrence of oxalic acid, or rather of *oxalate of lime*, in the urine, is not by any means an unusual event, for it may arise from the use of a diet in which this acid abounds; nevertheless its presence in the urine is always to be regarded with some consideration, for the oxalates are among the commoner products of disease; they have been found, for example, by Pickford in cases of anæmia, pemphigus, cardialgia, typhus, empyema, pleuropneumonia, rachitis, and, in fact, in most cases in which there was a poor condition of the blood. Simon detected oxalate of lime in the urine in a case of indurated pancreas, in which there was great acidity of the stomach; and Lehmann has discovered it in cases of tubercular disease, arthritis, endocarditis, and in some other acute inflammatory affections. Dr. Walshe states that the oxalates frequently appear in the urine of patients who are recovering from the last-named disorders; and he always regards their presence as a most favorable omen.

Dr. Bence Jones, adopting the theory of Liebig, has affirmed that these morbid products are occasioned by a deficiency of oxygen in the system, when, in fact, the uric acid of the effete tissues cannot be fully oxydized and converted into urea. Schmidt, however, who is opposed to this opinion, thinks that the oxalates are secreted by the mucous membrane of the kidneys and bladder. In proof of this, he refers to the fact that a morbid increase of the vesical mucus is always attended by an increase of oxalate of lime in the urine. He moreover states that he has always found this salt to be present in the mucous membrane of the gall-bladder, and in that of the uterus during pregnancy. Another point which has been advanced by Schmidt, is, that the oxalates are kept in solution in the blood and mucus by means of albumen; for he says that oxalate of lime may be always found dissolved in the albumen of the bile, and that it exists in a soluble form in the albuminous fluid that is contained within the vesicles of ordinary yeast.

Lastly, in most diseases of the liver, *biliary* matter makes its appearance in the urine; it being manifested by the presence of the colouring matter of the bile, and often, as Simon states, by cholesterine and resinous compounds. Schönlein says that bile is to be found in the urine in the fever which accompanies erysipelas; and Vogel states that it is generally present in the urine of cholera patients; indeed it appears, from the observations of most pathologists, that bile is always present in the urine during those deranged conditions of the system in which the function of the liver is in any way interfered with. In this manner the kidneys perform a sort of vicarious duty, which is frequently of the utmost importance to the suffering patient.

Dr. Golding Bird has commented on this fact, and he truly states that—

> "It is a duty which these organs can perform readily, because I presume it is less in violation of their normal and definite functions than is the case with any other gland. Thus the liver, excreting normally but thirty-seven grains of nitrogen, could hardly be expected to secern any considerable proportion of this matter from the blood,—not so the kidneys, for these organs, as we have learned, always excrete, besides the nitrogenized bodies, urea, uric acid, creatine, and creatinine, a pigment (the uroxanthin), nearly as rich in carbon as the bile itself, to which it bears no small analogy, and a peculiar extractive allied to cystin, although not yet isolated, but containing much sulphur, and thus, in another important point, approaching the hepatic secreta. But, dismissing theory, look to bedside observation; observe any case in which the hepatic functions are deficient, and we see the urine assuming a compensatory, although, of course, not quite a complementary function, from the kidneys depurating the blood of carbon in the form of an increased quantity of its peculiar pigment,—a body containing fifty-nine per cent. of carbon; and, as a proof, the addition of a few drops of hydrochloric acid to the warm fluid, developes a magnificent crimson or purple hue, instead of the pale lilac of healthy urine thus treated. Let, however, the liver remain inactive, no matter whether from disordered function or lesion of structure, still the industrious kidneys labour on, and the chamber-pot is now observed by the patient to present a delicate high-water mark of an exquisite lake colour. Soon this matter increases, and deposits of varying shades of crimson and purple occur. What is this purple deposit? What its function and origin? It consists of the ordinary urate of ammonia, mixed with the body once suspected to be murexide, or purpurate of ammonia, but with which it has not the most remote analogy, save in colour.
>
> This *purpurine*, as I ventured to name it when I first suggested its then probable, and now ascertained function, is, as I hinted to you last week, but a slightly metamorphic form of an element of the bile, and contains no less than sixty-three per cent. of carbon. Let, however, the disease assume another phase, let the excretion of bile by the liver become arrested, the varying shades of yellow of the surface attesting its presence in the blood; *then*, not by assuming any new function, but in accordance with a law announced by Wöhler, of removing all soluble noxious matters, the kidneys secern and excrete the matter in health proper to the liver, and the contents of the bladder become nearly as bilious as urinous. The

picture I have sketched is a familiar one; and of every-day occurrence as it is, can we not deduce from it a useful lesson in learning, and (what will be better) acting upon the important fact, *that the kidneys can depurate the blood, not only of matters generally regarded as proper to their function, but of substances which it is the normal duty of other emunctories to separate from the animal organism.*" (Lecture VI.)

We dare not venture upon an account of the changes which are produced in the composition of the other secretions and solids of the animal organism by disease, but we must refer our readers who are anxious to obtain more complete information upon these points, to the admirable work of Dr. Franz Simon, lately published by the Sydenham Society. It is proper, however, before we leave this part of our subject, to allude to the general results obtained by Andral from an extensive examination of the various morbid secretions of the human body; for it is a common opinion that the fluids, and especially the secretions, undergo a change with regard to their acid or alkaline characters during diseased states of the system. This, however, Andral positively denies; his words are, "that the amount of reaction in each case may vary according as the fluid is diluted or concentrated; but an acid fluid never becomes alkaline, nor an alkaline fluid acid even in disease." This observation is, therefore, opposed to the statement of Mialhe; namely, that the serum of the blood is acid in diabetes, a statement which has moreover been refuted by the experiments of Bouchardat. Another conclusion of Andral's is to the effect that, generally speaking, all those pathological fluids which are characterised by the presence of albumen and globules, are formed by the exudation of serum, and partake of its alkaline property; such, for example, are the fluids of blisters, of herpes, eczema, pemphigus, and those which result from inflammation of mucous surfaces.

And now, on leaving this part of our subject, it becomes our duty to make a few comments upon the labours of Dr. Garrod; for his Lectures, amounting to thirty-six in number, are devoted almost entirely to a consideration of the chemistry of the healthy and diseased structures and products of the animal body, there being only two of them, namely, the thirty-fifth and thirty-sixth, which can be said to relate to the therapeutics of his subject. We have, to use a common but not less apt expression, waded through these lectures, and have sought in vain for a spot of new or fertile ground whereon to rest ourselves; indeed, excepting that, at every step we took, we have been reminded of some old and familiar scene, our journey through them would have been as void of interest as it has been of profit. But, to drop metaphor, we may say that these lectures contain scarcely anything which is not to be found in Dr. Day's translation of 'Simon,' and Dr. Day's Reports in Ranking's 'Abstract;' the materials supplied by the latter having been interwoven with the former, to make up the staple of the course. The only points on which we find any original information or even speculation, are as to the existence of urea and uric acid in the blood, and a new view of the pathology of scurvy. For his researches on the former topic, we readily accord him full credit (see p. 440); with his speculations on the latter, we have felt compelled on a former occasion to express our entire disagreement. It is far from our intention to throw discredit on the labours of a judicious and industrious compiler. There are men who deserve our warmest praise for the manner in which they make use of the labours of others; for in so doing they collect, as it were, the "*disjecta membra*" of science, and mould them into a good and useful form; among such men is the author whose work stands at the top of our list. But we cannot award this praise to Dr. Garrod, for he has neither gleaned new facts, nor made a better arrangement of old ones; he has done little else than epitomise a work which has been before the profession for more than three years. We are therefore utterly at a loss to conjecture under what plea Dr. Garrod has come forward as the writer (not the reader) of thirty-six Lectures upon the Chemistry of Pathology and Therapeutics. If, however, he lays claim to the merest shadow of a purpose, it must be sought for in the last two lectures, where he has endeavoured to point out the applications of chemistry to therapeutics; but here, though our author has, for the first time trusted to himself, and has avoided all mention of the important labours of Mialhe, Dumas, Bonnet, Bouchardat, Blake, Bernard, and many others in this department of science, yet we cannot perceive that he has advanced us a single step in the cause which he advocates.

With reference to our Third question, it may be said that we are not very intimately acquainted with the nature of the chemical changes to which the solids and liquids of the animal body are subject during the operation of medicinal substances: but that very considerable chemical changes do take place, cannot be questioned; for we find that numerous substances have the power of entering into combination with the organic textures, &c., and thus of giving rise to the formation of new compounds; this, for example, is the case with the caustics, astringents, antiseptics, emollients, and probably many of the animal and vegetable acids. According to Mialhe, the first-named substances exert their action by combining with the tissues of the body; and by so doing, they either produce an insoluble, and a more or less plastic compound, when he terms them *coagulating caustics;* or they give rise to the formation of a soft, soluble, and *gelatiniform* substance, when he names them *fluidifying caustics.* As instances of the former, he mentions the mineral acids, the chlorides of antimony, gold, zinc, mercury (deuto), the nitrates of silver and mercury, the sulphate and acetate of copper, creasote, concentrated acetic acid, &c.; and as examples of the latter, he refers to the caustic solutions of potash, soda, and ammonia, arsenious acid, arsenic acid, hydrated phosphoric acid, oxalic acid, &c. All these substances, however, do not act with the same intensity, and they are, therefore, not applicable to the same purposes; but we must refer our readers to the work of Mialhe (p. ccxxxv) for more complete information upon this subject.

Again, the action of astringent substances is somewhat similar to that of the coagulating caustics, but it is of a much milder character; for though astringent matters have the power of combining with the tissues of the part to which they are applied, and thus of giving tone and firmness to them, yet they commonly do this without destroying their vitality. They may also act by coagulating the fluid contents of the tissues, and by uniting with the liquid matters which are thrown out upon secreting surfaces. In this way they are frequently serviceable in checking the circulation in any part, and in putting a stop to inordinate discharges. Most of the antiseptics exert a similar action; while emollient substances, such as water, and the mucilaginous, oily, and albuminous compounds, act by distending the tissues of the part to which they are applied, thereby rendering them soft and more flexible.

All substances, however, which are capable of being absorbed into the animal economy, must exert an influence upon the chemical composition of every part of it; for, being admitted into the circulation, they must affect the composition of the blood; being thus brought into relation with the various solid structures of the body, they produce a chemical change in these portions of the animal fabric; and, lastly, being eliminated from the system through the portals of one or more of the glandular organs, they must likewise alter the chemical composition of the secretions. It is unfortunate, however, that the attention of chemists and pathologists has not yet been directed to a careful examination of the various chemical alterations which are thus effected in the system; and excepting that a few experiments have been made with the view of determining the action of medicinal substances on the organic fluids, &c., *after* their removal from the animal body, the whole subject remains in nearly the same condition in which it existed in the days of Hewson, for at that time it was known that many saline substances had the power of retarding the coagulation of the blood.

> "I have tried," says Hewson, "most of the neutral salts, and have made a table of their effects on the blood; but it is not necessary to trouble the reader with it, as we do not see what use it can be in medicine; for we must not conclude that their effects within the body would be the same as out of it. Indeed these experiments, as well as some others, were not made so much with the view of any immediate application to medicine, as to determine the properties of the blood chemically."

And, in recapitulating the principal facts and conclusions to which his experiments had led him, Hewson stated that "neutral salts, on being mixed with the blood out of the body, make it more florid; they likewise, if used in great quantity, prevent its coagulation when exposed to the air, and some of them allow the lymph to be precipitated, or to jelly on being diluted with water." Now this is almost the sum total of our knowledge upon this matter, even at the present time; for though Dr. Davy, Mr. Prater, Mr. Ancell, and others, have been occupied in investigating the action of medicinal substances on the blood, yet they have not thrown much light upon the chemistry of the subject. Denis, however, has lately opened the way to a new order of inquiries, for he has shown that the action of neutral salts on the blood is probably dependent on the solvent influence which they exert over the fibrin of this liquid. He has, in fact, demonstrated that fibrin may be dried and powdered, without losing its power of coagulating when it is dissolved in a solution of a neutral salt, and diluted with water. It is, moreover, probable, from the investigations of this chemist, that fibrin may be kept in a state of solution in the blood by the agency of the saline constituents, and that the albumen of the serum is but a reconversion of fibrin effected by the solvent action of the same substances; for on digesting fibrin with a solution of nitre, he succeeded in producing a liquor which was coagulated by heat, and which closely resembled the serum of the blood. The same thing, he says, was brought about when he exposed fibrin to the action of air, warmth, and moisture. In taking up the inquiry from this point, it would be in teresting to follow out the before-mentioned hypothesis of Mulder, and to endeavour to ascertain if any changes are produced, either in the proportion or composition of his oxides of protein, by the introduction of saline matters into the circulation.

Again, the acids and alkalies doubtless exert a considerable action on the chemical composition of the blood; for when they are added to this fluid after its removal from the body, it becomes dark and gelatiniform. This fact was also observed more than seventy years ago by Hewson, who said that the alkalies were found to corrugate the vesicles, and that, in proportion as they were diluted, their effects became similar to water alone; but he remarked that it was not easy to find the point of strength at which the vesicles would remain unaltered in the solution.

"The effects of acids," said Hewson, "are very different. I have tried the vitriolic, nitrous, muriatic, distilled vinegar, and the acid of phosphorus; these, when much diluted, have the same effect as water in making the vesicles spherical, and in proportion as they are less diluted, they dissolve the vesicles, without making them spherical, as water does. I never could find any point of dilution when the acids, like the neutral salts, produce no change in the figure of the vesicles." (Hewson's Works, by Gulliver, p. 231.)

Dr. Pereira, in commenting on the influence of acids on the blood, and on distant parts, says:

"It is important to bear in mind the fact just mentioned; namely, that the acids enter the blood in combination with bases, so that they react on the stomach and alimentary canal as acids, but in the blood as salts. The impossibility of dissolving ossific deposits in distant organs by the internal administration of the acids is therefore readily accounted for. It is obvious, moreover, that no analogy can exist between the chemical influence of free acids added to the blood after it has been withdrawn from the body, and that of acids combined with bases (that is, of salts) which enter the blood from the alimentary canal." (p. 171.)

And he goes on to state that,—

"From the preceding remarks, it may be inferred that the precise changes effected in the blood by the internal administration of the acids are very obscure; nay, the very action of these bodies on the circulating fluid is rather assumed than demonstrated. The statements of authors as to the changes in the physical and chemical properties of the blood, produced by the administration of acids, are therefore, for the most part, hypothetical, as are also the pathological and therapeutical deductions therefrom. Several of them, indeed, are founded on erroneous premises. Thus, Dr. Stevens states that, 'when acids are used internally, some of them enter the circulation, darken the colour of the current, and reduce the force of the vascular organs; or when we add a weak acid solution to healthy blood out of the body, we make it quite as black as it is either in scurvy, cholera, or the last stage of the climate fever.' Now it is obvious that Dr. Stevens's statements, respecting the effect which acids produce on the blood, when they are administered internally, are not only entirely hypothetical, but are founded on the erroneous notion that the acids enter the circulation in the free state, and that their action on the circulating blood is similar to that which they exercise on blood that has been drawn from the body."* (p. 172.)

---

* We have been bold enough to make a slight alteration in the construction of the last sentence of the preceding quotation, by which we believe that the author's meaning will be more completely expressed. The original runs thus: "Now it is obvious that Dr. Stevens's statements, respecting the effect on the blood of acids administered internally, are entirely hypothetical, and are founded on the erroneous notion that the acids enter the circulation in the free state, and that their action on the circulating blood is similar to that which they exercise on blood drawn from the body."

In the same manner Dr. Pereira combats an opinion which has been advanced by Schultz, namely, that the acids and salts (alkaline and earthy) act on the blood as disorganizing agents; the former, namely, the salts, operating on the plasma; while the latter disorganize the blood-corpuscles, abstract their colouring matter from them, and render them unfit for the purposes of respiration. It is in this manner, according to Schultz, that the acids are beneficial as cooling agents in fever.

In speaking of the action of acids and alkalies on the animal economy, Mialhe says that there is much greater danger in administering acids than there is in giving alkalies; for the natural secretions of the system are generally alkaline, and it is not so easy to disturb this condition by the use of alkalies, as it is by that of acids. He states, moreover, that if the natural alkaline fluids become neutralized, or, what is worse still, if they become acid, then functional disorders will result; for the vital fluids are no longer enabled to produce the necessary interstitial changes, and though the economy may struggle for some time against this condition, yet it must, ere long, yield to it, and debility, wasting, together with such serious disturbances as pyrosis, gravel, gout, scurvy, or diabetes, may result.

Again, there is evidently a considerable change produced in the composition of the blood during the action of certain poisons upon the animal economy; for in all cases in which death results from the operation of such substances, the blood is found to be black and uncoagulated; we are, however, almost entirely ignorant of the nature of the changes which are thus effected in it, notwithstanding that Mialhe endeavours to show that such poisonous matters act upon the blood either as coagulants, liquefacients, or ferments. (pp. cccii, ccciii.)

Experiments have also shown that water, urea, bile, and many other substances which are likely to gain access to the circulation, have the power of dissolving the blood-corpuscles. Some compounds, however, and especially the saline, exert an action of an opposite character; and Dumas states that, during his investigations, he observed that the salts of the organic acids, such as tartaric and citric, had not so great a power in preserving the integrity of the blood-discs as those of the mineral acids. He noticed, moreover, that the potash salts were weaker in this respect than the salts of soda.

It will doubtless be remarked that all these illustrations have been derived from experimental inquiries into the action of medicinal substances on the blood, *after* it has been removed from the animal body; and although it is very possible that the results of these experiments may afford some indication of the nature of the changes which such agents produce in the chemical composition of the blood during its circulation in the living vessels, yet, it may be asked, are we justified in placing an implicit reliance thereon? We answer, no; for, as Dr. Day has stated, there is, in the first place, a great deal of difficulty in distinguishing between the results of a purely physical action (endosmose) and of a purely chemical one; and, secondly, the blood which has been removed from the body, in order to be submitted to experiment, is in a very different condition from that which it possessed when it was circulating in the living vessels; whilst, thirdly, we cannot secure to the agent the same state, either of dilution or of chemical combination, when we thus apply it to the blood, as that which it acquires when it is absorbed into the animal body. So that, although we may have obtained a considerable amount of information regarding the influence of various medicinal substances upon the blood after its removal from the living body, yet, for the reasons which we have just assigned, it is doubtful if we are warranted in applying this knowledge to the treatment of disease. And if we attempt to form conclusions from the results of our experiments upon the living blood, we are likely to go still further into error, for these results are but few and imperfect. It is probable, however, that the specific gravity of the blood is always diminished directly after the administration of large quantities of water, or weak saline solutions; and that its density is increased by the operation of those substances which tend, by their great specific gravity, to cause an exosmose of serum into the alimentary canal. So, again, the density of the blood is doubtless increased by the action of diaphoretic and diuretic substances, both of which act by removing water from the circulation.

Again, the introduction of saline matters, of acids, and of alkalies into the circulation must alter the composition of the blood, in so far as its salts are concerned. Experiment has also taught us that ferruginous preparations have the power of increasing the amount of blood-corpuscles; while the alkalies and the metallic preparations, especially those of lead, exert an opposite action.

It does appear, moreover, from the investigations of Schultz, that the neutral salts tend to reduce the amount of fibrin in the blood; for in a case of acute pneumonia, where, at the first venesection, the blood contained 5 per cent. of fibrin, he found that, on putting the patient under the influence of nitre and sulphate of soda, the amount of fibrin was, in the course of twenty-four hours, reduced to 3·4 per cent.; and on continuing the medicines for twenty-four hours longer, the blood contained only 1·9 per cent. of this organic substance. So also, with regard to the alkalies, the acids, most of the metals, and the preparations of iodine and bromine, they are found to diminish the amount both of corpuscles and of fibrin in the blood, and thus to produce what Simon has denominated spanæmia, or poverty of this liquid.

It is probable, moreover, that the administration of alcoholic and ethereal agents may tend to increase the amount of extractive and effete products in the circulation, inasmuch as such agents are now known to appropriate the oxygen of the blood, and so to prevent the elimination of the animal matters in question. According to Mialhe, the action of tartar emetic is of this kind; indeed, the last-named author, in generalizing upon the action of medicinal and toxic substances on the blood, says, that their operations may be reduced to one of four kinds, thus:

1. *Some operate by retarding the circulation of the blood,* as—

A. Those which coagulate its serum. To this division belong nitric acid, creasote, alcohol, the poisonous principles of fungi, &c.; and

B. Those which are decomposed by the salts of the blood, and precipitated in an insoluble form; such, for example, are the preparations of lime, strontia, baryta, &c., which, on being introduced into the circulation, are converted into the insoluble carbonates, sulphates, and phosphates of these earths.

2. *Some substances act by liquefying the blood,* and in this way they

facilitate its passage through the capillary vessels. Among such substances are acetate of ammonia, the nitrates of potash and ammonia, iodide and bromide of potassium, &c. (Poisseuille).

3. *Some act by modifying the chemical reactions which take place in the blood.* The majority of such agents operate by appropriating the oxygen contained in the blood; and the want of hæmatosis (*d'hématose*), or of oxydation, causes asphyxia and death. To this class belong the essential oils, sulphuretted hydrogen, &c. Others produce immediate death by suddenly arresting hæmatosis, the phenomenon being but ill understood. Mialhe, however, attributes it to the operation of a catalytic force, and he thinks that the action of prussic acid may probably be of this kind; for, from the interesting researches of M. Millon, it appears, that the presence of a very small quantity of prussic acid will instantly arrest the energetic oxydizing influence which iodic acid exerts upon organic matters.

4. *Some substances act by producing abnormal chemical reactions.* To this class belong the virus of rabid animals, the poison of the rattlesnake, &c. There is no doubt (says Mialhe) but that these substances act on the economy after the manner of certain ferments, and that they comport themselves with the organic elements of the blood somewhat as synaptase does with those of amygdaline, and diastase with those of amidon. In proof of this, he states that those medicinal agents which have the power of annulling the specific action of such poisons, are the very agents which can put a stop to the phenomena of fermentation; these are heat, strong acids, caustic alkalies, &c.

But a review of our knowledge with regard to the action of medicinal substances on the blood, will show that we are not yet in a condition to venture upon such extensive generalizations as these.

And so again, with reference to the influence of remedial agents upon the chemical composition of the solid textures of the animal frame, it may be said that our information is very imperfect.

> "We know that, by the internal use of madder, the bones and some other parts become coloured, and that the long-continued employment of nitrate of silver occasionally gives rise to a deposit of silver under the skin; but, with two or three exceptions of this kind, no chemical changes in the living tissues have been demonstrated." (Pereira, p. 87.)

Müller, however, thinks that the operation of many medicinal substances is due to the chemical influence which they exert upon the composition of the tissues, &c., of the living body. He believes that they produce a chemical change in the substance of unhealthy parts, the effect of which is, perhaps, not an immediate removal of the diseased tissue, but a removal of that state of combination which was opposed to healthy action; or the chemical change produced is such as to render the part no longer sensible to a morbid stimulus; or the medicine may act by producing a change in the chemical composition of the nutritive fluids.

It has also been discovered, through the investigations of Devergie, Danger and Flandin, Orfila, Millon, Mialhe, and others, that many of the metallic poisons have the power of combining with the organic tissues of the living body; for such substances have been detected in the muscles, glands, lungs, brain, and bones of animals which have been subjected to their influence. According to Mialhe, the poisons are frequently precipitated in the blood by means of its saline elements, and in this way, by giving rise to the production of insoluble compounds, they are arrested in the first set of capillaries through which they endeavour to pass. Liebig, however, believes that the true inorganic poisons enter into combination with the membranes, tissues, and muscular fibres of the living body, so as to produce a series of stable compounds; and this author, in speaking of the action of the vegetable alkalies, says, that, as their composition is nearer to that of nervous matter than any other compound, they may act by promoting or checking the formation of nervous matter. The same author remarks, that certain vegetable alkalies, such as caffeine (theine) and theobromine, may operate in promoting the secretion of bile, and he states, that soda acts in a like manner; for, when soda is deficient in the organism, the metamorphosis of albuminous compounds can yield only fat and urea. These observations were advanced by Liebig, chiefly with the view of showing that most substances exert a chemical action over the composition of the solids and fluids of the body, and that we may expect, in progress of time, to explain the actions of all remedies on chemical principles.

There cannot, moreover, be any question with regard to the fact, that dietetical agents exert a very important influence over the composition of the solid structures of the body. Indeed, the investigations of chemists have led to the discovery of the great functions which are performed by the leading constituents of the food; and there is hope that the prosecution of such inquiries will lead to a knowledge of the exact influence which these substances exert over the composition of the various animal textures; for already the researches which have been prosecuted in the schools of Giessen, Utrecht, Paris, and Edinburgh have served to establish the fact, that the continued use of many non-nitrogenous articles of diet will give rise to the production of an increased amount of fatty matter in the system; and future investigations will, doubtless, determine the manner in which an excess in the supply of the protein elements of our food is appropriated. Judging from the experiments of Liebig, Le Bel, Thompson, Boussingault, Playfair, and Knapp, it appears that, in order to supply the wear and tear of the system, and so to sustain the health of an individual, it is necessary to make use of foods in which there is a certain relation kept up between the blood-forming and heat-forming principles. This relation must vary, according to Knapp, with the age of the animal and its mode of life. In human milk, which may be said to indicate the proper relation of these substances in the food of the growing child, it is as 1 of the former to 6 of the latter. In English wheat flour, which may be taken as a fair representative of the food required by an animal at rest, it is as 1 to 8; while, for an individual taking exercise, the relation should be, according to the actual results obtained by Liebig, as 1 to 4·7. Now, an examination of the table which has been drawn up by Dr. Knapp, will show that the relation of 1 to 4·7 is almost exactly that which exists naturally in the various kinds of grain, wheat, corn, &c., including the husk. And, says Dr. Knapp—

> "Those barbarous nations which live entirely on flesh, receive a large excess of blood-forming matter, which may be counterbalanced either by the addition of calorifiant matter, or by increased bodily exercise. On the contrary, the poorer classes amongst us are obliged to live on the cheapest food which they can obtain, such as potatoes, &c., which are one half poorer in blood-forming constituents than the various kinds of grain. In the first case, Nature has only to get rid of an ex-

> cess; but in the latter she has to supply a deficiency. This must be done by milk, bread, cheese, &c. It must be evident to every one, that this manner of living is unnatural in the extreme. A person living entirely on potatoes, may be said to be on the edge of a fearful precipice, without a single inch of ground before him, where the only safety lies in retreat, if that be possible. Its disadvantages are shown in three different ways:—first, it leads to imperfect bodily strength, and unsoundness of health; secondly, to increased mortality, and shortness of life; thirdly, to want of energy, and to a kind of stupidity and want of interest in everything but what concerns the merest animal interests. A people in this state are always rife for rebellion and ready to join in every insurrection."

Professor Mülder entertains a similar opinion with regard to the degenerating action of the potato; and Dr. Thompson, in commenting upon the influence of certain diets upon the health of the animal body, says:

> "It is well known to feeders of cattle, that an animal fed on large quantities of potatoes is liable to complaints, such as affections of the skin, and also to loss of weight. These, consequently, it may be readily inferred, are derived from the want of the proper balance between the elements of the food."

Again, in scurvy, there can be no doubt that great alterations are produced in the composition of the textures, in consequence of a deficiency of some important element in the food.

In observing the effects of remedial agents on the composition of the renal secretion, we find that the administration of a large quantity of water causes an increase in the amount of urine; and on collecting this secretion, and calculating for the amount of solids contained therein, we find, as was first stated by Becquerel, that the kidneys have excreted a larger proportion of solid matter than usual, so that the water may be said to have washed away effete matters from the animal economy.

It appears, moreover, from the observations made by Dr. Golding Bird,—

> "*That we possess remedies which, when administered, remarkably increase the metamorphosis of tissue, and enable us to produce at will the very depurative effects which I have pointed out to you as resulting normally in the course of certain zymotic diseases.* In taking a practical view of the so-called diuretic agents, it will now become necessary to divide these into two classes: the one including those which simply increase the bulk of the urine; the other, those which act as *renal alternatives*, and aid the depuration of the blood.
>
> "To the former class belong all those agents which, out of the body, exert no chemical effects on animal matter, as all the vegetable diuretics—squill, copaiba, broom, juniper, guaiac, digitalis, &c. All these, in the absence of any opposing cause connected with mechanical obstructions to the free course of the circulation, will, it is well known, increase the discharge of fluid by the kidneys, and become often valuable agents in enabling us to successfully treat dropsical accumulations. Hitherto no distinction has been drawn between these agents and those which exert a chemical influence on organic matter; and hence two sets of agents, exerting most different physiological effects, were confounded. If the urine secreted under the influence of the diuretics I have enumerated be examined, the quantity of solids present will never be found to much exceed the normal quantity; nay, sometimes they will even be in smaller quantity than in health, in consequence of their, in some instances, acting as irritants to the kidneys, and by producing congestion, interfering with active secretion. (Lecture VI.)

In proof of this statement, Dr. Bird has quoted the experiments of Professor Krahmer, which show that many diuretics, as, for example, juniper, Venice turpentine, squill, digitalis, guaiac, and colchicum, do not increase the total amount of solids which are secerned by the kidneys in twenty-four hours; and from these results Dr. Golding Bird has enunciated the following proposition:

> "*Remedies, then, which exert no chemical action on organic matter out of the body, appear to be incapable of augmenting the quantity of solids in the urine, and hence are only of use in increasing the elimination of water,—they may, and do act as renal hydragogues, but not as renal depurants.*" (Lecture VI.)

The second class of diuretic agents to which Dr. Golding Bird refers, are those which increase the metamorphosis of the tissues, and thereby act as depurating agents. This class includes the alkalies, the alkaline carbonates, and those salts of the alkalies which are capable of being decomposed in the body and converted into the carbonate; such, for example, as the acetates, citrates, tartrates, &c. Dr. Golding Bird moreover believes that the action of potash in the system is like that which occurs when the alkali is brought into contact with protein compounds, as albumen, fibrin, &c., out of the body, and that it causes a solution of these substances. This opinion, says Dr. Bird, "I have repeatedly confirmed by absolute experiments," and he adduces one case in illustration of the fact; from which it appears that the administration of three drachms of acetate of potash, the exhibition of it being divided over a period of twenty-four hours, caused an increase in the amount of solid matters, from 416 grains, the normal quantity, to 782 grains.

> "The results of these analyses show that, after deducting the excess in the amount of soluble salts arising from the conversion of acetate of potash into carbonate, the solids of the urine excreted under the influence of the chemical diuretic exceed those recovered without its aid by 190 grains; and we further learn that, although a large proportion of matter was metamorphosed into both uric acid and urea, when the remedy was given, still that the greatest increase was in that mixture of organic products set down as extractive, and consisting chiefly of creatine, creatinine, uroxanthin, and matter rich in sulphur."

Dr. Bird is, moreover, of opinion that the solutive action of the alkalies is exerted with the greatest energy on those tissues which are diseased, or are low in the scale of organization; for he says:

> "Admitting that the elements of our frames resist chemical influences in the ratio of their vitality, it would follow that such constituents of our fabrics as present the greatest departure from health are less highly vitalized, and thus yield the easiest to the chemical force exerted by the alkaline diuretics. On this account, it is fair to presume that, when we cause an alkaline carbonate to circulate through the blood, it exerts an influence on the nascent elements of those matters less highly influenced by life, allied to that which they exert on dead matter, aids their resolution into substances allied to those produced out of the body, and actually causes the matter to assume so soluble a form as to allow of its ready excretion; this remarkable effect of the alkaline diuretics, although now for the first time demonstrated by actual experiment, and the results of their chemical influence detected in the stream by which they are washed from the body, was not overlooked by the observing physicians of other days."

And he concludes his remarks by saying:

> "I would earnestly beg those who are now doing me the honour of listening to my remarks, to give a careful and steady trial to the *depurating or chemical diuretics*, especially the salts of potass with vegetable acids, when they are called upon to treat a chronic affection in which the exciting cause, or existing disease, depends upon the presence of some product of less vitality, or imperfect organization. I fully believe

that in many instances such matters will be often found to yield, whether they present themselves as albuminous deposits in glands, furuncular disease of cellular tissue, or incrustations on the skin, as in some of the squamous and tubercular cutaneous diseases. That they will succeed in increasing the waste of matter, is, from my observation, beyond all doubt; that the lowest vitalized matters will yield to the solvent the readiest is most probable, and that an important and powerful addition to our supply of therapeutic weapons is certain." (Lecture VI.)

We have already said that the administration of acid substances robs the system of alkalies by combining with these matters in the blood; and it is proper to remark that the salts thus formed, as well as all others which gain access to the circulation, are chiefly excreted by the kidneys, and that the presence of these substances must therefore alter the saline composition of the renal secretion. Again, the administration of alkalies, and the alkaline salts of the vegetable acids, not only occasion an increase in the amount of extractive matters in the urine, as Dr. Golding Bird has shown, but, according to Wöhler, Millon, Laveran, and Mialhe, they sometimes have the effect of rendering the urine alkaline. Schmidt, however, has denied the truth of this statement; but, says Dr. Pereira, "that the urine becomes alkaline from the employment of the alkalies and their carbonates, is a fact to which I and most practical physicians can bear testimony;" and he here refers to some authorities in order to establish the correctness of his observation.

"Mascagni (quoted by Wöhler) found the urine alkaline from the use of one drachm of carbonate of potash daily. Mr. Brande (Phil. Trans. for 1810) observed the alkalinity of the urine after the employment of the carbonates of soda and potash. He says, that two drachms of carbonate of soda rendered the urine alkaline in six minutes, and produced the full effect in a quarter of an hour, occasioning a precipitation of the phosphates of lime and magnesia, and restoring the blue colour to reddened litmus paper. Magnesia had the same effect of occasioning a precipitation of the phosphates; but, on account of its insolubility, required a greater time to produce the effect. Lime-water required five hours to produce a sensible precipitation, which, even then, was not nearly so distinct as that from the alkalies. Dr. Bostock (Medico-Chirur. Trans., vol. v, p. 80) found that the urine became alkaline, and effervesced with acids, after the use of two ounces and a half of carbonate of soda daily." (p. 177.)

And we may add, that in the experiments of Laveran and Millon, the urine was rendered alkaline by the use of Rochelle salt (the potassio-tartrate of soda) in 175 cases out of 268.

We have before alluded to the influence which dietetical substances exert over the composition of the urine, and have stated that, while a vegetable diet increases the amount of extractive matter, an animal diet diminishes the proportion of these substances, and increases that of the urea. In fact, the researches of Lehmann, Simon, Percy, Boussingault, and Dr. Golding Bird show that there is a certain relation between the nitrogen contained in the food, and that excreted as urea or uric acid by the kidneys. This is particularly well illustrated by the experiments of Lehmann; and also by those of Boussingault, who found that when ducks were fed on different articles of diet, they excreted different amounts of uric acid; for example, when these birds were kept without food, or fed on substances which did not contain nitrogen, such as clay and gum, they excreted about 4 grains of uric acid in twenty-four hours; but on partaking of 635 grains of fibrin, the proportion of uric acid amounted to 138 grains;

and on being supplied with 1491 grains of gelatine, the uric acid rose to 157 grains; while a diet of 427 grains of casein produced 162 grains of uric acid, and one of 956 grains of flesh gave rise to the formation and excretion of 291 grains of this acid. It would appear, from the laborious investigations of Dr. Peters, that the use of water which contains much lime in solution, and a corn and bacon diet, may be the principal causes of the great excess of earthy phosphates and oxalates which are observed among the inhabitants of certain localities.

Here, for the present, we must drop the subject; as the questions remaining to be considered require an ampler discussion than our space will at present afford. We shall resume it in our next Number.

Art. II.

Physiological, Anatomical, and Pathological Researches. By John Reid, M.D., F.R.C.S.E., Chandos Professor of Anatomy and Medicine in the University of St. Andrews, &c. &c.—*Edinburgh*, 1848. 8vo, pp. 659.

This volume consists of a collection of Papers, which, with the exception of two, have been published in various periodical journals during the last thirteen years; and it might therefore appear to some of our readers that, in noticing them at any length, we should be unnecessarily occupying their attention, and encroaching upon space which should be devoted to matters of greater novelty. We are convinced, however, that few, save those who have made Physiology a special object of pursuit, are at all aware how largely the world is indebted to Dr. Reid for the series of important researches, whose results he has from time to time made public. The author of a systematic treatise is pretty sure to get sufficient credit for his labours, if they are really deserving of it; but the pains-taking observer, or the sagacious experimenter, who aims at the discovery of new truths, or the settlement of intricate and controverted questions, may long pursue his researches without an adequate appreciation of them by more than the select few, and may not receive his due tribute of public gratitude until it comes too late to afford him that gratification which would have been an encouragement and stimulus to him during the active portion of his career.

We consider that Dr. Reid has done an act of justice to himself, in thus bringing together the results of his endeavours to place various departments of medical inquiry upon a firmer foundation, so as to enable the public to appreciate the extent of his labours, and the success which has attended them. And it is from the conviction that he has hitherto suffered in public estimation from the absence of *system* in his researches, and that he has shared the common lot of those who think it more honorable to seek for truth than for popular applause, that we shall now do what in us lies to promote among our readers, and through them we trust in the profession at large, a more correct estimate of the value of his researches, and of his distinguishing qualities as a physiologist. With this aim we shall pass his principal memoirs *seriatim* under review, bringing into notice the leading points which we consider him to have established, and comparing his results, where desirable, with those obtained by other experimenters. In this latter part of our plan, we derive much assistance from Dr. Reid himself, who has appended to each of his most important papers a summary of the researches of those who have been labouring in the same field since his own were published; this summary, executed with the honesty and pains-taking care which characterise all Dr. Reid's productions, is not the least valuable part of the volume, since it brings to the present time the subject of each of the essays to which it is appended.

In reprinting his papers, Dr. Reid has faithfully kept to his original text, except in so far as mere verbal alterations are concerned; "taking special care not to alter the meaning of any passage, either by introducing new words or by withdrawing the old, so as to change the character of the statements or opinions they originally contained." In this he has exercised a wise discretion. Memoirs such as his will be hereafter referred to as matters of history; and the value of this collection would be altogether destroyed, if it could not be relied on as presenting the author's original meaning, in fact, his *ipsissima verba* in reference to every important topic. We have in our minds an instance of departure from this plan, which has given rise to the most painful suspicions of want of integrity in a distinguished physiologist now no more; the text of a collection of memoirs, which professed to be a reprint of the originals, having been altered in successive editions, so as to be brought into conformity with the advance of knowledge, and to be made to appear as the author's original expression of opinions which he had been subsequently led by the corrections of others to adopt. Upon such topics, however, we have no wish to dwell; we refer to the subject only to show the danger of any such course, and gladly proceed to the more agreeable task of making our readers acquainted with the chief results of Dr. Reid's inquiries.

The First Memoir is entitled "On the Relation between Muscular Contractility and the Nervous System;" a subject on which the interest is still kept alive by the recent discussion, to which we elsewhere refer (p. 447), between Dr. Marshall Hall and Dr. R. B. Todd. We shall not, therefore, apologise for stopping to notice the contents of this paper, although it has been before the public for nearly eight years; more especially as few, who have not expressly studied the question at issue, are aware of its exact bearings. The nature and degree of the dependence of muscular contraction upon nervous agency, was a subject of earnest debate, nearly a century ago between Haller and his contemporary Dr. Whytt, of Edinburgh. The former, adopting and rendering more clear and precise the doctrine of Glisson, maintained that muscular tissue possesses the property of contractility as its inherent property, or *vis insita;* this property being perfectly independent of nervous agency, or *vis nervosa,* though generally called into action through its means. The latter, on the other hand, attributed muscular contraction to the exclusive agency of the nervous system, denying to the muscular structure any independent endowment, and regarding it somewhat in the light of a piece of mechanism for the advantageous application of nervous power. As each party could cite a number of indisputable facts in support of its position, the controversy did not end in the establishment of either doctrine; and it has continued, under various modifications, down to the present time. The following extract from Dr. Reid's memoir will show the character of the chief of these modifications:

"Many of the Hallerians have extended the term contractility, and in our opinion justly, to certain contractile movements which may be induced in the arteries, in various secretory ducts, in the cellular tissue, and in the skin. They have also freely admitted certain effects of the nervous system upon the muscular contractility,—such as the influence of mental emotions, &c., upon the contraction of the heart; a conclusion which Haller would have willingly avoided. On the other hand, very many neurologists have given up the original opinion, that muscles derive their contractility from the central organs of the nervous system—the brain and spinal marrow,—and maintain that the nerves distributed in the different muscles have themselves the faculty of furnishing the conditions upon which contractility depends, believing that in the muscles of involuntary motion this is effected by the ganglionic or sympathetic system of nerves, and in the muscles of voluntary motion by the cerebro-spinal nerves. Some, again, exclude the cerebro-spinal nerves from all participation in this faculty, and confer it entirely upon the ganglionic system." (p. 3.)

The tendency of all those general views towards which modern physiological research has pointed, is decidedly in favour of the Hallerian doctrine, as modified by its present supporters; for instead of the old hypothesis that the entire organism owes its vital powers to the nervous system, the conviction is now forced upon us that each integral part has its own vital endowments, which are dependent upon its due nutrition; and that the nervous system is only one of such parts, possessing more extensive and intimate relations to the rest than they have to each other, though in no way superseding their several powers, but rather (as it were) taking advantage of them, to carry into effect its own peculiar functions. To maintain that the muscular apparatus is merely an appendage to the nervous system, serving to distribute and apply its power, would be, as it seems to us, to run counter to the whole tendency of modern physiological science; yet we find this doctrine, or something equivalent to it, upheld by many eminent physiologists, especially in Germany. Dr. Marshall Hall is, so far as we are aware, the only physiologist of note in this country who upholds the doctrine of the dependence of muscular contractility upon the nervous centres; and his views on this subject are dissented from by many of those who espouse his general system.

Dr. Reid's Memoir does not embrace a general view of the whole discussion; but contains the result of an experimental inquiry into the validity of an objection which has been of late frequently urged against the Hallerian doctrine,—namely, that muscles cut off from connexion with the nervous centres lose their contractility after a certain time, contractions being no longer excitable in them by any stimuli, ordinary or extraordinary; and that this loss of contractility proves that the property is derived from the nervous centres, or dependent on the connexion of the muscles with them. At first sight this objection seems to have considerable weight; but its cogency altogether disappears on a closer examination. For it is then found that the muscular contractility does not disappear *immediately* on section of the nerves; but that it persists, in fact, for a long period, departing gradually as the nutrition of the muscle is impaired (like that of every other tissue in the body) as a consequence of its functional inactivity. Not satisfied, however, with thus indicating the invalidity of the objection, Dr. Reid has turned the tables upon the objectors; for he has proved experimentally,—first, that if muscles, separated from their connexions with nerves, be kept in a state of occasional use (by the application of artificial stimuli), their nutrition is kept up, and their contractility retained, for any length of time,—and, secondly, that if the contractility of a muscle thus circumstanced be exhausted by repeated stimulation, it will be recovered after an interval of rest, and this even at an interval of some days after section of the nerves. We are not aware that any one has attempted to set aside these facts, or has succeeded in giving any other satisfactory interpretation of them than that which they are shown by Dr. Reid to bear.—In the supplementary remarks appended to this memoir, the author cites several other facts confirmatory of the doctrine sustained in it; especially the uniform results of numerous and diversified experiments, in regard to the persistence of contractility in muscles after the nerves supplying them had lost their excitability. Altogether we quite agree with him that the Hallerian doctrine seems now placed beyond the reach of assault; as we are not aware of a single fact which is not readily explicable in accordance with it, whilst there are many for which it is difficult, if not impossible, to account on any other hypothesis.

The Second Memoir, "On the order of succession in which the vital actions are arrested in Asphyxia," is an important contribution to the true knowledge of that condition; which knowledge, as the author justly remarks, "is not only useful in elucidating the exact nature of the function of respiration, and in pointing out rules for our guidance in the direction of certain remedial agents, but it may also be brought to bear in an important manner upon the investigations into the general laws of physiology." The memoir consists in part of an examination of the various opinions which have been advanced on the subject, by those who have devoted themselves to its investigation; and in part of an account of the experiments performed by Dr. Reid himself, with a view of testing the merits of these opinions. The principal questions in the pathology of asphyxia that are open to discussion are,—1, The nature of the impediment to the circulation through the lungs;—and 2, The cause of the arrestment of the sensorial functions. Dr. Reid regards it as demonstrated by the experiments of Dr. D. Williams and Dr. J. P. Kay, that the circulation is first brought to a stand by some impediment to its free passage through the lungs; but he does not consider that these experiments enable us to determine the nature of this impediment, whether it arises from the cessation of the respiratory movements of the chest, or from the arrestment of the usual chemical changes between the blood and the atmospheric air. Dr. Alison has obtained results from experiments on the respiration of rabbits in nitrogen, which were in harmony with the latter doctrine; but Dr. Reid's ingeniously-devised experiments were better adapted to bring it to the test. Into the details of these we must not enter; but shall simply confine ourselves to a statement of the general fact, that the circulation in the lungs, as indicated by a hæmadynamometer applied to the femoral artery, was rapidly diminished in force and amount during the inspiration of nitrogen, and immediately increased on the substitution of atmospheric air, the mechanical movements of the chest in both cases remaining the same. Another very unexpected result was obtained,—namely, that at the commencement of the experiment, while the flow was still vigorous through the lungs, the pressure within the systemic arteries (as indicated by the hæmadynamometer) was greater than usual, whilst that on the systemic veins was less: when, however, the asphyxia became more complete, the pressure within the systemic arteries fell to much below its proper level; but, on the other hand, when atmospheric air was first readmitted, the mercury rose at first to a higher point than usual, and only sank to its ordinary level as the blood became more perfectly arterialized. We do not see any other feasible explanation of this curious phenomenon, than the one offered by Dr. Reid, namely, that it is due to an obstruction to the passage of imperfectly arterialized blood through the systemic capillaries; and we are further in accordance with him in regarding this obstruction, as well as the stagnation of the pulmonary circulation, as due to the suspension of those chemical changes to which the blood ought to be subjected in its passage through the capillaries. Since the publication of Dr. Reid's paper (1841), the subject has been reinvestigated by Mr. Erichsen; the results of whose experiments entirely coincide with those of Dr. Reid, so far as they are repetitions of them, due allowance being made for a difference in the hæmadynamometers employed. But Mr. Erichsen does not assent to this part of Dr. Reid's interpretation of the results; preferring to substitute for it the hypothesis that venous blood acts as an excitant upon the contractility of the ultimate ramifications of the pulmonary veins, and thus causes an obstruction to its passage along these vessels. This view is noticed by Dr. Reid in his supplement to the original paper; and the following argument, which seems to us of great cogency, is advanced against it.

> "The property of contractility possessed by the arteries is that kind termed by some physiologists *simple contractility*, to distinguish it from the contractility of the muscles of voluntary motion, and manifests itself by slow contractions, followed by equally slow relaxations. As Mr. Erichsen himself states, one experimenter found that a period of from one to three minutes elapses before the contractility of an artery responds to excitants. When contraction has been induced in the coats of an artery by an excitant, it is well known that relaxation does *not follow suddenly*, even on withdrawal of the excitant; but it occurs slowly and gradually. If, then, the ultimate ramifications of the pulmonary veins are stimulated to contraction by the venous blood, relaxation ought *not to follow instantly* upon the withdrawal of this excitant; and the entrance of pure atmospheric air into the lungs ought not to be instantly succeeded by the free passage of blood from the right to the left side of the heart. The following experiment, mentioned by Bichat, and which I have frequently repeated, appears to me to be an *experimentum crucis* upon the point under discussion. If a tube, with a stopcock upon it, be tied into the trachea of an animal, and the stopcock be turned to exclude fresh air from the lungs until the circulation of dark blood along the arteries has become much enfeebled, as ascertained by exposing a large artery and making an opening into it, *instantly* on the admission of fresh air into the lungs, the blood springs from the cut artery, of a bright red colour, and with greatly increased force. I have observed the same thing repeatedly, when the hæmadynamometer was fixed in the femoral artery; no sooner was the stopcock opened, and fresh air permitted to enter the lungs, than the mercury suddenly sprang up several inches in the ascending portion of the tube of the instrument. Notwithstanding, therefore, all that Mr. Erichsen has so ingeniously advanced against the part of the theory of asphyxia that refers the impediment to the passage of the blood through the lungs to the cessation of the chemical changes which occur there in natural respiration, my belief in its truth has not been shaken." (p. 46.)

The other question discussed by Dr. Reid, is the immediate cause of the suspension of the sensorial functions; which was attributed by Bichat to the circulation of venous blood in the arteries of the brain; whilst Dr. Kay regards it as chiefly due to the diminution in the supply of blood transmitted to that organ, in consequence of the impediment to the circulation in the lungs. In order to arrive at a true solution, a variety of experiments were made by Dr. Reid, of which he details the results, subsequently comparing them with those of Dr. Kay; and from the whole inquiry, he comes to the conclusion, that the doctrine of Bichat was essentially correct as to this particular; the suspension of the functions of the brain taking place *before* there is any considerable diminution in the pressure of blood in the arteries, and being chiefly, if not entirely referable, therefore, to the circulation of venous blood through the organ. He very justly remarks, that we are not hence to conclude that venous blood exerts any positively noxious influence upon the nervous texture, its effects being solely attributable to the want of proper excitation of the properties of that texture by their appropriate stimulus, oxygen; for, when the circulation of arterial blood through the brain is renewed, its functions rapidly remanifest themselves, provided that this be done within a given time. Important confirmation is thus afforded to the general doctrine, that the combination of the elements of nervous matter with oxygen is a necessary condition of its *functional activity;* just as, in the galvanic trough, the amount of electricity developed is in precise accordance with the quantity of zinc that has undergone solution. The *nutrition* of nervous matter, or the replacement of that which has thus undergone disintegration by a fresh growth, appears from many considerations not to be dependent upon highly arterialized blood in nearly an equal degree.—Dr. Reid is perfectly justified in citing, as an example of the diminution of cerebral activity, produced by the circulation of venous blood through the brain, the gradual torpor which frequently creeps over the sensorial functions in severe cases of bronchitis, when an ill-arterialized blood is circulating in the vessels of the brain, and the pulse is still pretty strong at the wrist. We much want an explanation, however, of the remarkable freedom from any such disturbance which presents itself in many cases of the severer forms of cholera; the intellect being perfectly unclouded, though not in a state of activity, and the patient's consciousness not failing, when the pulse is scarcely to be felt at the wrist, and the blood in circulation seems completely venalized.

The Third Memoir treats "Of the Effects of Venesection in renewing and increasing the Heart's Action, under certain circumstances." It appears, from several observations made by Dr. Reid, whilst witnessing other experiments, as well as from inquiries specially directed to this point, that when the right side of the heart has become loaded with blood, in consequence of obstruction to the pulmonary circulation, and its contractions are consequently enfeebled or suspended, the detraction of a small quantity of blood from the jugular vein has in some cases a decided influence in renewing its movements,—a fact that may be turned to valuable account in practice. It may be thought that the valvular apparatus of the heart and large veins, by preventing any reflux of blood, would prevent the flow from an orifice in the jugular vein from having any tendency to unload the right cavities of the heart; but it has been found experimentally, that neither the tricuspid valve nor the valves of the jugular act efficiently when there is much distension, and that they permit a certain amount of reflux,

—as, indeed, is evident in the "venous pulse," so often visible in the necks of patients labouring under obstruction to the pulmonary circulation, and consequent distension of the right cavities of the heart and systemic veins. When the jugular vein is opened, therefore, in cases of asphyxia, we ought to encourage bleeding from the *lower* orifice, rather than from the upper; since, by unloading the right side of the heart, we favour the renewal of its contractions; whilst it is very questionable whether we really ever "unload the cerebral circulation," which is the object usually supposed to be attained by this measure, and to favour which the bleeding from the *upper* orifice is encouraged by preference.

Dr. Reid's 'Experimental Investigation into the Functions of the Eighth Pair of Nerves," constituting the Fourth and Fifth of the Memoirs contained in this volume, is the most important and elaborate of all his contributions to physiological science. We could not fully explain its peculiar merits, without entering into more detail than our limits permit, as to the state of our knowledge, or rather of our ignorance, on the subject, previously to the time when he first took it up. It must be sufficient to state that on scarcely any one point were experimenters agreed; and that the most contradictory hypotheses were entertained by theoretical physiologists with regard to the functions of each division of the nerve. It would be too much to affirm that Dr. Reid's experiments have elicited the whole truth with regard to every portion of this extensive and complicated inquiry; but it may be confidently stated that he has settled a very large number of questions so satisfactorily, that very little correction or extension of his views can be anticipated; and that the methods of inquiry which he has indicated, are those which afford the surest promise of success in the determination of the points yet remaining to be made clear. Our space will only allow us to indicate briefly the principal questions settled by Dr. Reid's investigations, and to notice some of the results of more recent inquiries.

The functions of the *glosso-pharyngeal* nerve were considered to be purely sensory by some physiologists, purely motor by others, and by others, again, both sensory and motor. Dr. Reid has clearly proved that it is essentially an *afferent* nerve; one of its chief functions being to serve as the *excitor* to the act of deglutition, by the impressions which it conveys from the fauces; whilst it is also the chief *sensory* nerve of the fauces and of the back of the tongue. With regard to the *motor* powers of this nerve, Dr. Reid concluded that they were not *direct* but *reflex;* no contractions being observable in any muscle except the stylo-pharyngeus, when the distal end of the trunk was irritated after its severance from the medulla oblongata; and the movements of this muscle being only observed in those cases in which there was a deficiency of evidence that the irritation was confined to the filaments originally composing the nerve. He now states, however, that in subsequent experiments he observed movements of the stylo-pharyngeus, when it appeared that no other filaments than those of the glosso-pharyngeal were embraced within the forceps. His first conclusion was supported by the experimental results of Valentin and Longet; but other inquirers have observed muscular movements, not merely in the stylo-pharyngeus, but in other muscles of the fauces. As to these last, however, there is such a discrepancy of statement, that we incline with Dr. Reid to think that other filaments than those of the nerve in question must have been involved in the irritation; the weight of evidence, however, is now decidedly in favour of its direct motor powers, as far as regards the stylo-pharyngeus muscle.—The *sensory* and *excitor* properties of the glosso-pharyngeal have been recognised by numerous other experimenters; and the chief point that now remains open for discussion, is the participation of this nerve in the sense of the taste. The opinion of Panizza, that it is the sole and special nerve of taste, has been sustained by Valentin and Bruns; but it is more or less distinctly negatived by the experiments of Guyot and Cazalis, of Magendie, of Longet, and of Volkman and Bidder, all of whom agree with Dr. Reid in the belief that this sense is to a certain degree persistent, especially in the front of the mouth, after section of this nerve, and that the lingual branch of the fifth pair must consequently be looked upon as participating in it. This might be inferred from anatomical evidence; since it is impossible to prove that the glosso-pharyngeal is universally distributed over the tongue, especially the sides and upper surface of its anterior third, which appear to be exclusively supplied by the fifth pair. As additional evidence in favour of this view, Dr. Reid refers to numerous cases of paralysis now on record, in which common sensation and the sense of taste were annihilated in those parts of the tongue supplied by the fifth pair. Two curious cases put on record by Mr. Noble (Medical Gazette, Nov. and Dec. 1835), in one of which common sensation was lost in the parts supplied by the third branch of the fifth pair whilst the sense of taste remained in those parts, while in the other there was loss of taste with maintenance of feeling, might seem at first sight to invalidate this view; but, as Dr. Reid has justly remarked, they merely indicate that the gustative filaments are not the same with those which minister to ordinary tactile sensation, so that we may have the functions of the one set annihilated, without those of the other being at all affected. We are decidedly of opinion that there is a ganglionic centre for the sense of taste, quite distinct from that of common sensation; and if the lesion be in the central organs, it is as easy to understand how one set of filaments may be paralysed without the other, as it is to account for anæsthesia without loss of motor power. Even in nerves of common sensation, there seems good reason to think that the filaments which convey impressions of simple contact must be different from those which minister to the sense of temperature. It is where the disease lies in the *trunk* of the nerve, and not in its central terminations, that we should expect to find the complete destruction of its functional power; and Dr. Reid remarks that he is not aware of any recorded case in which the trunk of the third division of the fifth pair has been found diseased after death, in which there has not been during life a loss of the sense of taste in the middle and anterior parts of the tongue. For the present, therefore, we are quite disposed to agree with him in the conclusion, that the glosso-pharyngeal and the lingual branch of the fifth pair are both nerves of common sensation, and both participate in the sense of taste; and we think that we see an obvious reason why the supply of the tongue should be thus divided between two sensory nerves. It is by the sensations and impressions made on the *anterior* part of the tongue, that the movements of *mastication* are chiefly guided; and these movements are effected by the agency of the motor portion of the third branch of the fifth pair, whose close central relation with the sensory nerve of the front of the tongue is thus satisfactorily accounted for. On the other hand, the sensations and impressions made on the *back* of the tongue and on the surface of the fauces, are partly the guides, and partly the direct excitors, of the movements by which the food is carried back to the fauces and then swallowed; and these movements are chiefly performed by the agency of motor nerves closely connected at their central origin with the glosso-pharyngeal.

We come next to the par vagum; Dr. Reid's inquiry into which is, perhaps, the most laborious and scrutinizing investigation of the functions of any single nerve ever carried through by any single physiologist.—With regard to the endowments of this nerve at its origin, Dr. Reid had concluded that its own proper roots are motor as well as sensory, although part of the motor filaments bound up in its trunk are derived from the internal branch of the spinal accessory. The motor powers of the par vagum *proper* have been subsequently affirmed by numerous trustworthy experimenters; and the only physiologists of eminence who now uphold the purely sensory character of the roots of the par vagum, are Valentin and Longet, whose negative testimony is by no means sufficient to set aside the mass of positive evidence in favour of their mixed character. The motor filaments derived from the spinal accessory appear especially to pass into the *pharyngeal* branches of the vagus, which are chiefly motor and muscular, the supply of afferent fibres to the pharyngeal surface being afforded by the glosso-pharyngeal nerve. In his conclusion that the pharyngeal branches of the vagus afford the chief motor supply to the pharyngeal muscles, Dr. Reid has been fully borne out by the experiments of Longet and others.—It is to the experiments of Dr. Reid that we owe our present knowledge of the curiously related functions of the superior and recurrent *laryngeal* nerves. He stated it as the definite result of his experiments, that the former is almost entirely an afferent nerve, having no direct motor influence over any of the laryngeal muscles, save the crico-thyroid; whilst the latter is exclusively motor. This conclusion is supported by the experiments of Volkmann, Traube, Van Kempen, and especially of Longet, to which last physiologist the credit of having first determined the relative functions of the laryngeal nerves is attributed in several works lately published on the Continent, notwithstanding that Dr. Reid's memoir, containing most explicit conclusions on this very point, derived from careful experimental inquiry, had been published between three and four years previously. These conclusions are in precise accordance with the anatomical evidence supplied by the dissections of Mr. Hilton; and they harmonise also with the results of morbid affections of the recurrent nerves.

The researches of Dr. Reid upon the *œsophageal* branches of the par vagum afforded clear proof that the movements of the œsophagus are, to a certain extent, of a reflex nature, depending upon the excitor impressions and motor impulses conveyed through its afferent and efferent fibres respectively; and in settling this point, he afforded the first decided indication of the existence of reflex action, without the participation of sensation, as a part of the normal series of actions taking place in the human system. There appears good reason, however, to believe that the contractions of the muscular coat of the œsophagus are partly of a peristaltic nature, and are called forth by direct excitation. The degree in which these two powers participate in the general result appears to vary in different animals; and will probably be found to be in harmony with the differences in the relative distribution of the striated and non-striated muscular fibre, which Mr. Gulliver has pointed out as having a constant existence amongst different animals,—the former being probably called into play by reflex action, the latter by direct excitation.

Like other experimenters upon the *cardiac* branches of the par vagum, Dr. Reid failed in procuring any very definite evidence of their functions. He found, however, that when rabbits were killed by suddenly and extensively crushing the brain with a hammer, after section of the pneumogastrics of one of them above the origin of their cardiac branches, the action of the heart was evidently slower and more vigorous in that animal than in the one whose pneumogastrics had been left entire; and this result was not a solitary one, but was noticed in three instances. Hence Dr. Reid concluded that the cardiac branches of the par vagum establish such a communication between the heart and the brain, as communicates to the former the influence of a *shock* experienced by the latter. In the appendix to this memoir he gives a lucid summary of the results obtained by other experimenters; which, so far as regards the asserted effects of irritation of the vagus in quickening the heart's action, are anything but satisfactory. Much more interest, however, attaches to the effects of excitation of the vagi by the electro-magnetic apparatus.

> "The brothers Weber state that excitation of both vagi by the electro-galvanic rotatory apparatus diminishes the frequency, and even arrests entirely, the movements of the heart, reducing it to a state of complete repose, so that the blood flowing to it distends its cavities; and it resumes its movements on the withdrawal of the excitants. The same effects follow electro-magnetic excitation of the medulla oblongata. Similar experiments, with the same results, have been performed by Budge. Electro-magnetic excitation of the medulla oblongata, though it throws the voluntary muscles into tetanic contraction, has no effect upon the heart when the vagi have been divided. Electro-magnetic excitation of the sympathetic nerve does not produce these effects upon the heart. The explanation of electro-magnetic excitation of the vagi and medulla oblongata upon the heart is still involved in doubt; but it seems to act upon the heart somewhat similarly to an extensive injury of the central organs of the nervous system, or of other parts of the body, producing a condition of the heart which forms the most important symptom in what surgical writers term *concussion*, and the *shock* from an injury or severe operation." (p. 281.)

Dr. Reid's elucidation of the functions of the *pulmonic* branches of the par vagum is, perhaps, the portion of his researches which has most completely set aside former erroneous doctrines, and substituted for them definite and correct views. He set himself to ascertain, in the first place, whether section of the vagus on one side only produces (as some previous experimenters had asserted) a constant change in the condition of the lung of that side; this being, as he justly remarks in his appendix, a point of considerable importance, in enabling us to judge whether lesion of the vagi acts *directly* or indirectly in inducing morbid changes in the lungs. No fewer than seventeen experiments were made by him to determine this question; the animals having been allowed to live for periods of from twenty-four hours to six months after the operation. The general result of these experiments is decidedly to *disprove* the idea that any morbid changes in the lungs directly result from section of the nerve; most of the animals, even that which had lived for the longest time, being found to have both their lungs in a healthy state; and only one rabbit having died of pneumonia, which, however, affected *both* lungs equally. This result is in accordance with the experiments of Dupuytren, Magendie, and Mendelssohn; and it seems fair to conclude, therefore, that in the cases recorded by others, in which disease did manifest itself in one lung after section of its pneumogastric nerve, such disease could not be the direct result of the operation. The first effect of the section of *both* pneumogastrics, has been shown by Dr. Reid's numerous experiments to be a considerable diminution in the *frequency* of the respiratory movements, which immediately fall to half their usual rate, or even less. Other experimenters had noticed the same occurrence; but the result is so liable to be disturbed by individual peculiarities in the animals operated on, and by the effects of the paralysis of the larynx, that it is not surprising that there should have been contradictory statements on this point. The opposite conclusion is open to another source of fallacy, which would scarcely have been anticipated.

> "In judging of the effects of the division of the vagi upon the frequency of the respiratory movements, it is absolutely necessary to reckon them; for, as these movements become more apparent, prolonged, and heaving, we may be readily deceived. From neglecting this I fell, in my earlier experiments upon the *vagus*, into the error of supposing that the respiratory movements were increased in frequency, especially in rabbits. It is in this manner that we can account for the statement of Dr. M. Hall, that, after division of the vagi, the acts of respiration immediately become much more frequent." (p. 181.)

We feel confident that no one can examine the details of Dr. Reid's experiments, without appreciating the care and sagacity exhibited in the avoidance of all avoidable sources of error, and the scrupulous accuracy with which the results are recorded. The number of experiments, *twenty-two*, is quite sufficient to entitle us to set down the only *two* instances in which there was no considerable difference in the frequency of the respiratory movements, as altogether exceptional. The occasional occurrence of such exceptions, however, is an important fact, as helping to explain the discrepant statements of other inquirers. As the pneumogastrics have no direct connexion with the muscles concerned in the respiratory movements, it is obvious that their influence must be reflex; in other words, that they must serve as excitors. That they are not the only excitors, however, is shown by the fact that the movements continue, though with diminished frequency, after section of these nerves; as well as by various other facts which indicate that the nerves of the general surface, especially the fifth pair, have a powerful action of the same kind. We are strongly inclined to believe that, as soon as the blood circulating in the systemic arteries is rendered venous by the insufficiency of the respiratory process, every cerebro-spinal nerve in the body will become an excitor of reflex movement. And we quite agree with Dr. Reid in the surmise put forth in his supplementary observations, that respiratory movements may have a centric origin, when the blood transmitted through the medulla oblongata is insufficient in amount or imperfectly aerated. It was stated by Dr. M. Hall, on the authority of Cruveilhier, that section of the vagi, after the destruction of the cerebrum, has the effect of instantly putting a stop to the respiratory movements; this assertion has been shown by Dr. Reid, however, to be incorrect, the movements being observed to continue, although at a greatly reduced rate. A still further reduction was produced by division of the spinal cord beneath the origin of the phrenic nerve, so as to cut off excitor influence from below. All these results have been confirmed by the subsequent researches of Volkmann, Flourens, Longet, and Van Kempen.

With regard to the direct motor influence of the pneumogastrics upon the muscular fibres of the trachea and bronchi, Dr. Reid was unable to obtain any satisfactory evidence by galvanic irritation of the nerve, although the phenomena of disease led him to suspect its existence. Dr. C. J. Williams, however, has been more successful; and his results are confirmed by those of Valentin and Longet. Dr. Reid also endeavoured to ascertain how far the sensibility of the lining membrane of the air-passages is dependent upon the sensory properties of this nerve; but without any very definite results,—the fact appearing to be, that the mucous membrane of the trachea and bronchi has, in the state of health, very little of that sensibility which is so eminently possessed by that of the larynx, and that it soon becomes tolerant of the presence of substances which at first excited the tendency to cough. Several other sources of fallacy are alluded to by Dr. Reid; and we cannot help suspecting that some of these may have attended the more positive results said to have been obtained by Krimer, Brachet, and Longet, who were probably less experienced than Dr. Reid in the demeanour of animals suffering under this lesion.

We cannot follow Dr. Reid through his admirable investigation of the cause of the morbid changes in the lungs, resulting from section of the pneumogastrics, which is one of the most directly practical points of the whole inquiry. The accuracy of his account of these changes is evident, from the accordance in the descriptions given of them by subsequent experimenters of eminence, who have especially studied this subject. They may be summarily stated to be "engorgement of a greater or smaller part of the lungs with blood, a variable quantity of frothy serum, frequently tinged with blood, in the bronchial tubes, and not unfrequently condensation of portions of the lungs from the effusion of the solid constituents of the blood, or from inflammatory effusions, with more or less of emphysema of other portions." (p. 286.) The order in which these arise, and their relation of mutual dependence, constitute a most important object of inquiry; which is capable, if satisfactorily determined, of affording valuable information as to the rationale of numerous morbid phenomena. We consider that Dr. Reid has effected this determination almost indisputably; and that he has thereby made a decided advance in our knowledge of the pathology of this function, as by his previous researches he had done in its physiology. The absence of any morbid changes, after section of a single pneumogastric, was sufficient to prove (as already remarked) that no *direct* morbific influence on the nutrition of the lungs, or on their functional activity, could be exerted by this lesion; and to indicate that we must seek our explanation in the diminution of the respiratory movements,—the primary and immediate result of section of both pneumogastrics. Now if the doctrine be admitted, that the due aeration of the blood is necessary for its free movement through the pulmonary capillaries, it will follow as a necessary consequence, that diminished frequency of the respiratory movements will occasion a retardation of the pulmonary circulation, which will tend to increase as the aeration is less and less effectual. This retardation involves congestion of the pulmonary arteries to their minutest ramifications; and such congestion cannot continue long in such an organ, without the occurrence of serous effusion, with more or fewer of the elements of blood. This effusion, blocking up the air-cells and smaller air-passages, will still further increase the congestion, by offering an additional impediment to the aeration of the blood; and it might be expected that, if the animal lives long enough, the congestion would pass into inflammation, and this perhaps even into gangrene. Now all this is so precisely the sequence of the phenomena, as displayed in Dr. Reid's experiments and post-mortem examinations, that it is impossible to conceive how any more complete *rationale* could be given; and we find in it a further confirmation, if any were needed, to the soundness of the doctrine on which it is based. The explanations offered by Mendelssohn and Traube, both of whom have experimented on the subject, are very insufficient; Volkmann had adopted that of Dr. Reid. Its applicability to the explanation of morbid phenomena is made evident by the details which he gives of two cases, in which a condition of the lungs, corresponding in character to that produced by section of the pneumogastrics, was evidently traceable to a marked diminution in the number of respiratory movements for some time before death,—this diminution appearing to have arisen in the one case from torpor of the nervous centres, produced by the accumulation of urea in the blood, as a consequence of kidney-disease; and in the other, from sanguineous effusion in the brain.

Scarcely less important and satisfactory has been Dr. Reid's determination of the relation of the gastric branches of the par vagum to the digestive function; although this determination gives a direct negative to assertions which had been previously accepted as physiological facts. The opinion most widely received, in this country at least, with regard to the relation in question, was that of Dr. Wilson Philip; who attributed the suspension of the digestive process, on section of the par vagum, to the stoppage of the supply of "nervous influence," which he imagined to be essential to the secretion of gastric fluid; and who even went so far as to assert that the establishment of an electric current through the nerve would re-excite that process. These assertions have taken such fast hold on the minds of physical philosophers, that we scarcely expect that any amount of disproof will dislodge them for some time to come; but it is quite time that the profession should relinquish both of them, as entirely inconsistent with the present state of our knowledge on the subject. The experiments of Breschet and Milne Edwards, and also of Brachet, had given some ground for the belief that the interruption to the due performance of the digestive functions, occasioned by lesion of the vagi, is the result, not so much of suspension of the secretion of gastric fluid, as of the suspension of the movements of the walls of the stomach. How far even this could be rightly attributed, however, to the want of "nervous influence," was rendered doubtful by the statements of the latter observer; who asserts that the mere incisions necessary to lay bare the vagi in the neck, though the nerves themselves were left uninjured, was sufficient, in some cases, to arrest the digestion of food previously taken into the stomach. There was, however, a general concurrence of opinion among physiologists, that in some way or other the digestive process is interfered with by section of the vagi; notwithstanding that cases were on record in which the long continuance of life after section of the vagi indicated that some return of the powers of the stomach must have taken place.

The researches of Dr. Reid, however, have conclusively established the fact, that the digestive process may be so completely re-established, as not merely to sustain life, but to improve the condition of the animal, which had at first suffered from inanition. The first effect of the operation is almost invariably to occasion vomiting and a loathing of food; and if food be taken, it is usually rejected after a time with little or no change. The animal usually dies, from the disorder of the respiratory function, before there is time to ascertain whether this be a temporary or a permanent derangement; and it is not surprising, therefore, that the idea of its essential connexion with the lesion of the nerves should have obtained currency. In four out of seventeen experiments, however, made by Dr. Reid for the determination of this question, the life of the animals was unusually prolonged; and it became apparent that, notwithstanding the proof subsequently obtained as to the complete division of the nerve-trunks, with loss of substance, on both sides, the digestive process was gradually re-established; the vomiting progressively becoming less frequent, the matters rejected having undergone partial solution, and the food being at last entirely retained and applied to the nutrition of the animals, as was evidenced by their recovery of flesh and strength, and by the turgescence of the lacteals when they were killed soon after a meal.—It is perfectly obvious, then, that the par vagum cannot supply any condition essential to the digestive process; and that the temporary disorder of the function is to be explained on some other principle than that of the absolute dependence, either of the secreting operation, or of the movements of the stomach upon nervous agency. The conclusions of Dr. Reid have been confirmed by those of Longet; and the fact must henceforth be regarded as sufficiently established. The *negative* results of a hundred experiments would not be adequate to overthrow the *positive* evidence drawn from half a dozen instances in which the digestive process had been unequivocally re-established, provided that due care be taken (as was the case in Dr. Reid's experiments) to ascertain that the nerves were effectually divided on both sides.

It is not asserted by Dr. Reid, however, that the vagi exert no influence either upon the movements or upon the secretions of the stomach. His experiments upon the former point showed that contractions may be frequently excited in the cardiac portion of the stomach, by irritation of these nerves in the neck; but he inclines to the opinion that these contractions are of a peristaltic character, originating in the movement of the lower part of the œsophagus, just as movement of one part of the intestinal tube is propagated onwards to another. The movements of the stomach may be habitually excited by this stimulation; and yet may be sufficiently independent of it to be able to take place without it, when it is suspended by section of the nerve. And something of this kind may probably be true of the secretion of gastric fluid. Indeed we would suggest whether this may not be in its turn partly dependent upon the movements of the stomach, as that of saliva is upon the motions of mastication.

We have dwelt so long upon the preceding Memoirs, that we can do little else than enumerate the titles of the most important of the remainder.

No. VII, on some points in the Anatomy of the Medulla Oblongata, is a paper of great value; as it was in this that the first demonstration was given of the *motor* character of the *olivary* column, as evidenced by its connexion with the attachments of the motor encephalic and spinal nerves; the attention of anatomists being also directed to the fact (previously announced by Rosenthal, but lost sight of, and independently discovered by Dr. Reid,) that the decussating portion of the *pyramidal* columns is continuous with the *middle* columns of the spinal cord, not with the anterior as usually represented,—these last being continuous with the olivary columns. No. VIII treats of the Anatomical Relations of the Blood-vessels of the Mother to those of the Fœtus in the human species. This paper appeared at a time when the greatest contrariety of opinion existed upon the subject; and the accuracy of the view taken of it by Dr. Reid —namely, that there is no real vascular communication between the fœtal and maternal vessels, but that the ultimate branches of the umbilical vessels form tufts which are bathed on their exterior by maternal blood, —is now universally admitted. No. XIII contains a series of valuable Tables of the Weights of some of the most important organs of the body at different periods of life; these furnish a collection of data, which, united with those obtained by others, enable us to deduce averages worthy of considerable reliance. A large proportion of the latter half of the volume is occupied by Dr. Reid's Reports (Nos. XIX and XX) on the Epidemic Fever of Edinburgh in the years 1838-9; a document which, like too few other histories of the same kind, derives a permanent value from the fulness and accuracy of its pathological details. We can cordially agree in the high praise bestowed on these documents by Dr. Bartlett in his work on the Fevers of the United States. No. XXIV is an excellent digest of the various researches that have been made on the effects of the entrance of Air into the Veins; its value being greatly increased by the author's practical acquaintance with the subject.

The last three papers contain anatomical and physiological observations of great interest upon several marine animals, which the author's residence at St. Andrew's has afforded him special opportunities of studying. Our space will not admit of any other reference to them, than to mention that Dr. Reid's observations fully coincide with those of Sir John Dalyell, as to the persistence of the base of the polype-larva, and its return to the polypoid state after the detachment of the medusa-buds,—a fact of much importance, as we formerly showed (Vol. I, p. 203) in regard to the interpretation of the phenomena;—and to state that Dr. Reid's very careful researches completely negative the assertions of Steenstrup, as to the medusan structure of the original polype-larva.

In taking our leave of Dr. Reid, we can only reiterate our estimate of the high value of his labours, and assure him that we shall rejoice to welcome any new contributions to scientific truth, from one who has proved himself so skilful, as an experimenter, so philosophic as a reasoner, and so honest, single-minded, and trustworthy as a man.

ART. III.

Guy's Hospital Reports. Second Series. Vol. VI. Part I. October, 1848. pp. 200. With Four Plates.

THIS is a small number, and by no means a favorable specimen of the work. The paper of most pretensions in it is one which should scarcely have found a place there at all; and the general character of the others barely comes up to the standard of former years. This is not as it should be. The opportunities at the hospital are great, and the staff of observers is abundantly sufficient to take advantage of them, for their own and the public good. We trust that the succeeding numbers may regain the old position of utility and interest.

I. *Analysis of fifty-four cadaveric inspections of fatal pneumonia;* by H. M. Hughes, M.D.

This paper, an addendum to a former one, by the same author, published in the Seventh Volume of the Reports, presents some points of interest, and has the great advantage of exhibiting the results of the investigations of a single and well-qualified observer, who has carefully excluded from his tables everything of doubtful character or import. The former paper contained a digest of 101 cases of pneumonia observed during life, and of the morbid appearances found in 145. So that in the two combined, we have the results of 300 observations.

In regard to the general question of the influence of *Age* on the liability to pneumonia, we obtain no trustworthy information from these tables, because the number of children admitted into the hospital is very limited; so far as they go, however, they bear out the deductions from preceding inquiries, that the greatest number of cases are met with between the ages of 20 and 30; and they also show that the largest proportion of fatal cases is found above the age of 50.

Sex. Of the total number of cases, 208 were males, and 82 females, the remaining 10 being children, of whom the sex is not noted. This is quite in accordance with the general opinion; but we entirely agree with the author, in his belief that the result depends upon the different degrees of exposure to the ordinary exciting causes, rather than on any constitutional peculiarities.

The side affected with pneumonia. The right side alone was affected in 111 cases, the left side alone in 88, both in 98. But it is remarkable that, although there was so decided a preponderance of the right over the left lung, in respect of the proneness to disease, the fatal cases were precisely equal, being 59 in both. Dr. Hughes inquires whether this may not indicate that pneumonia is really more fatal, when it attacks the left than when it attacks the right side; but professes himself unable to decide the question. We conceive that much more extended observations would be necessary for the determination of this point, which is one of considerable interest; and we find, from an appended note, that the following was the ratio of mortality of dextral, sinistral, and double pneumonia occurring in 24 fatal cases observed during life, and examined after death.

Pneumonia of the right side	was fatal in the ratio of	27	per cent.
,, left	,,	10	,,
,, both sides	,,	37	,,

which is apparently quite opposed to the above-mentioned idea.

Parts of the lung or lungs diseased.

The upper lobe was alone diseased in	25	cases.
The centre ,,	11	,,
The posterior ,,	14	,,
The base or bases ,,	125	,,
The disease was general in the right lung in	25	,,
,, ,, left ,,	23	,,
,, ,, both ,,	41	,,

We have here a marked confirmation of the prevalent opinion, that the base of the lungs is peculiarly the seat of pneumonia. Dr. Hughes believes that this is more especially true of the primary form of the disease; and in reference to the numbers in the table, remarks:

> "But seeing that a large number of the cases in the fatal columns represent pneumonia of a secondary form, which I regret my inability to separate from those of a primary form; and seeing also, that secondary pneumonia is more inconstant as to the part of the lung which is attacked than that of the primary form, it may be presumed that even these numbers do not adequately represent the amount of disparity between the upper and the lower lobe when the disease is not preceded by other important ailments. This presumption is confirmed by noting the corresponding numbers in the column representing the living cases,[*] from which it will be seen that while the base or bases were attacked in 62 instances, the upper lobe or lobes were attacked in only five instances, or in rather less than one in twelve."

The bearing of this on the diagnosis of phthisis is abundantly evident. But it must be remembered, that other observers have met with a larger proportion of pneumonia of the upper lobe. Thus Grisolle found 101 cases of this kind out of 264.[†]

Our author's experience is at variance with M. Louis' observation, that the age of persons affected with pneumonia of the upper lobe, is generally greater than that of those in whom the lower is the primary seat of disease; for in his cases the average age of 25, who had pneumonia of the upper lobe, was 37½, while of 116 who were affected in the lower lobe, the average age was 34—a difference so very trifling, as to be of no value in regard to the influence of age. Moreover, the average in the females (a small number, however) was 26 for the upper lobe, and 31½ for the lower lobe.

II. *Case of hydatid disease of the liver, cured by operation;* by G. Owen Rees, M.D., F.R.S.

This is a very interesting case. The subject of it, a railway navigator, of intemperate habits, four years before admission, had his thigh broken by the falling-in of some earthwork, but did not recollect to have received any blow on the body at the time. Two years afterwards he felt a slight pain in the right side, and then, for the first time, observed a small swelling in that position. This continued to increase, and he suffered from nausea and slight cough. On admission (Oct. 13, 1847), a distinct tumour was found occupying the whole of the scrobiculus cordis and right hypochondriac region, encroaching towards the left side, about two inches beyond the central line of the abdomen. Inferiorly, the mass could be felt extending over the upper third of the umbilical and right iliac regions. There was no jaundice. He felt nausea more especially about three hours after taking food, but was seldom free from the sensation. On percussing the region of the liver, there was dullness somewhat above the usual line in the thorax. The tumour yielded a dull sound, and communicated a peculiar thrill to the finger, opposed to it as a pleximeter. Distinct fluctuation could be felt, but of a character indicating considerable tension of the containing sac. The general health was as yet but little affected. Urine natural in every respect.

---

[*] In the table above we have only given the grand totals.

[†] Vide Walshe, Diagnosis of Diseases of the Lungs, p. 243.

He was put upon the use of iodine, internally and externally; and this treatment was continued, but without benefit, up to the 4th of December, when vomiting and cough being very troublesome and the health failing, it was determined to tap the tumour, repeated examination having convinced Dr. Rees that it was really connected with the liver. The evidence on which most reliance was placed, it may be well here to observe, was the fact, that on no occasion could the presence of the transverse colon or other viscus be detected in front of the tumour, which might have been expected, had it been connected with the kidney.

The instrument used in the operation was a trochar and canula, not larger than an ordinary exploring needle; and, before it was introduced, the tumour was secured in position by means of straps of plaster, passed completely round the body, and drawn tightly, so as to exert pressure upwards. Thirty-eight ounces of clear fluid were thus removed, and the tumour became flaccid, and appeared to contain no more. The patient was then carefully bandaged, and placed in bed on his back, in which position he was to remain, and to be kept perfectly still. A grain of opium was also given immediately. No bad symptoms followed.

On the 7th of January he was tapped again in the same way; but now pus of a very offensive odour escaped, to the amount of only ten ounces, the canula becoming obstructed. On the 9th, it was determined to tap the tumour with a full-sized trochar and canula, and to maintain the opening. This was accordingly done, an elastic gum tube being passed through the canula, which was then withdrawn over it. About twenty-four ounces of fetid pus escaped, mixed with membranous flakes and partially destroyed hydatids. The elastic tube was withdrawn on the 11th. The patient had no symptoms of peritonitis. His health was maintained by good diet, stimulants, and tonics. The discharge gradually ceased; and, by the 11th of April, the orifice had healed, and he was stouter and looked better than on admission. The sides of the abdomen appeared perfectly symmetrical. On manual examination, no very distinct tumour could be detected; but, on considerable pressure being practised, a body could be felt below the right lobe of the liver, apparently about the size of a walnut.

The clear liquid first drawn off from the tumour possessed all the usual characters of hydatid fluid. It was of specific gravity 1010, feebly alkaline, not coagulable by nitric acid nor by heat. It contained alkaline carbonates, and traces of caseous matter. The few membraneous shreds which were observed floating in this fluid, presented no structure, when viewed under the microscope.

The purulent fluid, the result of the second tapping, contained teeth of echinococci, creatures now known as inhabitants of hydatids, owing to the recent researches of Dr. Eugene Livois.

III. *Ophthalmic cases;* by John F. France.

In the last volume of the Reports, Mr. France narrated two cases, which appeared to support Valentin's theory, that the dilatation of the pupil is effected through the influence of the ophthalmic nerve; not, indeed, by its own original components, but by fibrils from the superior cervical nerves, which are conducted to it (the ophthalmic) by means of the superior cervical ganglion and carotid plexus, and are conveyed on by it to the ciliary ganglion and iris. In this paper he records three additional cases,—two decidedly favouring, and one apparently contradicting, the theory in question.

Our readers will remember that the sensibility of the conjunctiva depends on the integrity of the ophthalmic branch of the fifth pair of nerves. In two of the cases here described, that sensibility was impaired or lost, and the pupil was incapable of dilatation.

In the first case, the patient was the subject of partial hemiplegia of the right side. Sensation was greatly impaired over the entire right half of the head and face, including the conjunctiva, which permitted the free contact of the finger without the least inconvenience or spasm. The membrane was as moist as that of the left side. The skin of the right side of the neck was also numb. Neither speech nor the motions of the face had been affected, and the intellect was clear. On admission, the patient could not see to read at all. Subsequently vision improved, so that she could read a large print with either eye; but the right continued much the more dim of the two. The irides were bright and free from adhesions, and the other structures of the globe also appeared perfectly healthy. The right pupil was, in a remarkable degree, smaller than the left; insomuch, that, if the left eye was closed, the stationary diameter of the right pupil, at its utmost expansion, scarcely exceeded the semidiameter of the left, when contracted to the utmost. If both eyes were opened, the right pupil became slightly more contracted still; so that, if the patient quietly regarded the examiner in an evenly-diffused, moderate light, the right pupil exhibited a diameter hardly equal to half that of the left.

The next case presented phenomena very similar, though in a less marked degree; and it was observed that the contraction of the pupil was coincident with the development of anæsthesia of the conjunctiva.

In the third case, however, there was total insensibility of the left side of the face, including the conjunctiva; and yet the iris was perfectly free and active in its motions, until impaired by a severe attack of inflammation. There was also deafness, and paralysis of the abductor muscle of the right eye.

In reference to this, which is seemingly in entire contradiction to the former, Mr. France observes:

"This case, wherein total anæsthesia of the conjunctiva co-existed with a healthy condition of the pupil until the onset of inflammation, seems at first utterly incompatible with the conclusion favoured by the four previously narrated. A more attentive analysis, however, certainly shows that it is not irreconcileable with that deduction; for, as the entire fifth nerve was affected, and as the auditory of the same, and the abducens of the opposite side, were involved, setting aside other cerebral symptoms—there was strong presumptive evidence, not only that the cause of paralysis was posterior to the Casserian ganglion, but also that the brain itself was the seat of mischief. Now, if this were so, or if merely the source of disturbance were but situate within the cranium, posterior to the entrance of the carotid, with its accompanying nervous filaments, no reason can be shown (admitting Valentin's theory), why such of these as were destined for the iris should not continue to discharge their function, without detriment, though bound up for a space in the same neurilema with a paralytic nerve; for such fibrils would not necessarily participate in the damage suffered by the ophthalmic prior to their junction. The occurrence of inflammatory changes in the globe of the eye forms no argument against the disease having been thus limited, as might at one time have been supposed; for it has been shown by Mr. Dixon,* that such changes may occur from palsy of the trigeminal, alike, when the Casserian ganglion is involved, and when the disease is confined to the portion of nerve between that ganglion and the brain.

"But, granting the last case to have been a strictly cerebral one, in other of these cases, also, it may be objected, the brain was manifestly the seat of disease as well. Be it so: the only difference which it is needful to conceive between them, in order to satisfy the requirements of the rationale of their differing symptoms now offered, is this;—that, where the pupil remained unaffected, then the disease was confined to parts (brain or nerve, membrane or vessel) posterior to the connexion between the carotid plexus and the fifth nerve; when the pupil was affected, there, whether disease existed at the former situation or not, at least it did exist between the point of that connexion and the iris."

We commend this subject to the investigation of all who have a good field of observation in paralytic cases: it is one of much interest.

IV. *Observations on healthy and morbid articular tissues;* by John Birkett, F.L.S., F.R.C.S., &c. With four plates.

The object of this communication is to show, that articular cartilages are possessed of a higher energy of nutrition and development than is usually supposed; and to point out how the nutrition of this tissue is effected, and how it is destroyed in disease.

The author directs attention to the analogy which exists between the skin and subcutaneous cellular tissue, and the cartilage and its supporting bone. The subcutaneous cellular tissue is represented by the cancellous structure. The corium and basement membrane is represented by a very peculiar and remarkably hard lamina between the cancelli and the cartilage; and the epidermis has its representative in the cartilage layer. Analogy may even be carried further; for we find marrow occupying the cancelli as fat does the areolar spaces of the subcutaneous cellular tissue, and the blood-vessels ramifying on the osseous lamellæ, in the same manner as in the areolar laminæ, continuing even until they form reflected loops, just as occurs in the corium.

On examining a thin vertical section of articular cartilage under the microscope, it will be found that the cartilage cavities or corpuscles near the bone have thin, long axes in various directions; but, as they approach the free surface, they assume a definite and uniform course, being arranged parallel to the articular surface of the bone. These corpuscles originate in the blastema, poured out by the looped capillaries of the bone.

---

* Medico-Chirurg. Trans., vol. xxix. p. 135.

The hard, thin lamina which intervenes between the cancellous structure and the cartilage, is thus described:

> "It does not present the usual characteristics of bone at all. It is true that there exist in it, as in bone, lacunæ, and that these lacunæ are occupied by nuclei, but I do not find the beautiful arrangement of canaliculi leading from these lacunæ. The disposition of these spaces is very regular; and the substance of this tissue, which is extremely dense and hard, varies very considerably over the irregular surface of the true bone. It may be described in either of two ways: in the one, that this hard material is deposited between the projecting capillary vessels inclosed in their osseous canals, or that these osseous canals, with their vessels, extend into the hard substance."

In disease, the destruction of the articular cartilage depends chiefly upon a diseased state of the capillary vessels, whereby its nutrition is suspended, and then disintegration simply, not ulceration, takes place. This disintegration is mainly attributable to a metamorphosis or conversion, first of the nuclei, and then of the intercellular or hyaline substance, into fat. As the disintegration of the cartilage-tissue advances, a fibre-tissue, in which capillary vessels become developed, supplies the loss, and increases in consequence of the nutrition supplied by the neighbouring vessels. But after the destruction of the cartilage, the thin, delicate, and peculiar lamella of the articular extremities of bones must be destroyed before synostosis (anchylosis), or perfect osseous union, can result.

V. *On opacity of the cornea;* by J. Bateman Wilson.

This is the narrative of a case cured by the internal use of iodine, when other plans had failed. We cannot see that it is of importance sufficient to warrant a place in these Reports.

VI. *Case of tubercular disease of the brain, with remarks;* by Thomas Salter.

The case is interesting and well reported, but does not present any points to call for particular comment.

VII. *On the physical conditions affecting the distribution of life in the sea and the atmosphere, and the laws of aquatic breathing;* by Thomas Williams, M.D., Lond.

At the termination of this long discursive paper, the author informs us that its aim has been to range the multitudinous facts of marine zoology under the established principles of organization, and to exhibit, in a connected form, the unity of the physical, chemical, and vital laws by which terrestrial and aquatic animals are governed. We confess that we have been unable to discover the accomplishment of this purpose, or to draw from its multifarious details any fixed principles whatever. This may be the fault of our own obtuseness, or it may in part depend upon the style adopted, which is by no means the clearest; but whatever be the reason, the fact is as we have stated, and we must therefore beg leave to refer such of our readers as are curious enough, to the original itself, merely remarking that it appears to us somewhat misplaced among Hospital Reports.

VIII. *The anatomy of the ophthalmic ganglion in the horse;* by Alfred Poland. With a Plate.

The parts are clearly described, and well figured.

IX. *Second septennial Report of Guy's Lying-in Charity;* by John C. W. Lever, M.D., and Henry Oldham, M.D.

This, and the kindred reports which appear from time to time, will afford valuable materials for future reference, but are, from their very nature, insusceptible of analysis.

X. *On the alleged production of phosphate of lime and iron in the egg during incubation;* by Alfred S. Taylor, F.R.S.

It was long a difficulty with physiologists to account for the presence of certain chemical elements, as azote, phosphorus, carbon, lime, &c., in the bodies of animals whose food, according to the then belief, either does not contain them at all, or not in apparently sufficient quantities; and also to determine the source from which the phosphate of lime and iron existing in the newly-hatched chick could be derived; seeing that the former was supposed not to exist in the albumen, from which all the parts of the future bird are formed, while none of the latter had been detected in an egg before incubation.

The researches of modern chemists have thrown much light upon these formerly obscure points, by showing that the assumptions on which they were founded are erroneous; and in this paper Mr. Taylor narrates some carefully conducted experiments, which bear out the following conclusions.

"1. That the chemical composition of vegetable food is such as readily to account for the introduction of carbon, phosphorus, sulphur, and nitrogen, into the animal system, and to render it unnecessary to assume that those elementary principles are products of the animal organism.

"2. That the fluid parts of the egg contain iron and phosphate of lime in some unknown state of combination at all periods.

"3. That iron and phosphate of lime are contained in the freshly-laid egg, as well as in that which has undergone the process of incubation; and that from the application of tests in qualitative analysis, there is no reason to believe that the proportions of these two bodies are greater at one time than at another.

"4. That iron is contained in the albumen as well as in the yelk of the egg, and probably in equal proportion.

"5. That the shell of the egg contains no iron, and but very slight traces of phosphate of lime."

XI. *Select cases of strangulated hernia;* by Edward Cock.

The first case narrated belongs to the class of obscure and difficult forms of hernia, of which some examples were recorded in a former volume.

The patient, a female, æt. 51, had been the subject of femoral hernia for twelve years. Strangulation took place on the morning of June 30th, 1848, and she was admitted on the evening of July 2. Two grains of opium were administered immediately to allay the irritability of the stomach, and ice was applied to the tumour. In a few hours afterwards the contents of the sac were easily returned. This having been accomplished, our author continues:

"My attention was then drawn to an abnormal fulness above Poupart's ligament, in the position of the external ring. This likewise yielded to pressure, and appeared to retire into the inguinal canal. The ring was large and open, readily admitting the end of the finger, and its margins were sharp and distinct. She expressed herself greatly relieved, and was therefore left for the night.

"On visiting my patient the next morning, I found that all the symptoms of strangulation had returned, or perhaps I should rather say that no relaxation of them had taken place. The fulness and protrusion from the external ring, noticed the previous evening, was increased rather than diminished; it could be pushed back into the canal, but returned on the slightest abdominal effort. It was evident that some intestinal obstruction still existed; and after consulting with my colleague, Mr. Hilton, I determined to explore the inguinal region.

"The first incision through the integuments and fasciæ, exposed what appeared to be a very thin hernial sac protruding through the external ring; and on opening this, a quantity of fat resembling omentum presented itself. The finger was readily passed between the sac and the fat which it contained, but the boundaries of the cavity appeared to be ill defined, and I was unable to recognise, as I approached the internal ring, any neck or opening of communication with the belly. It became necessary to explore farther; and, after cutting through the tendon of the external oblique, fresh portions of fat rose into view. It was now evident that the supposed omentum was nothing more than the adipose tissue surrounding the round ligament, assuming a lobulated form, and surrounded by a cellular cyst. Turning these lobes aside, I reached the internal ring, which occupied its normal position, and brought into view a small peritoneal sac protruding through the opening. Before opening the sac I attempted to return its contents; but although I could easily push the entire hernia into the abdomen so as to cause its complete temporary disappearance, I was unable to empty it; an incision was therefore made into the sac, and a small knuckle of intestine exposed. The constriction was deeply seated and very firm; but, by drawing the gut forward, the neck of the sac was brought into view, and then easily divided. Some difficulty was still experienced in returning the intestine, and this was only accomplished by holding the edge of the sac on each side with forceps, to prevent its retirement while the bowel was pushed back into the abdomen. During the operation, some small portions of the fat had become completely detached, and were removed before the wound was closed. She was ordered to take a grain of calomel and a grain of opium every few hours."

The case progressed favorably, and she left the hospital well on the 12th of August. It is exceedingly creditable to the surgeon.

The remaining cases are narrated chiefly with the view of enforcing the advantages—

"1. Of securing total rest and a temporary cessation of the ordinary function carried on by the alimentary canal, in all instances where the intestine has received an injury, whether such injury be inflicted by external violence, as by a blow or stab, or by strangulation during its retention for a longer or shorter period in a hernial sac. The latter class of injury, it should be remembered, is almost necessarily enhanced and aggravated by the fruitless manipulations of the surgeon, and by the exposure and the violence offered to the bowel during the progress of an operation for strangulated hernia. The principle which should guide us in the treatment of an injured intestine, is analogous to that which we follow in the management of a bruised or broken limb.

"2. I was anxious to draw the attention of my readers to the salutary effects of opium, which, in such cases of injury, seems to act as a pure sedative, without exhibiting its ordinary injurious effects as a narcotic; producing calmness, quietude, and even cheerfulness, without impairing the appetite, or interfering with the secretions of the body.

"3. The foregoing cases in some measure tend to illustrate the benefit of opium, wherever inflammation is either present or threatened as a consequence of intestinal lesion or injury; inasmuch as it keeps the organs concerned in a quiescent state, and gives time and opportunity for nature to repair the mischief which accident has occasioned."

In these very judicious remarks we most fully coincide, believing that the hasty administration of aperients is decidedly the worst practice that can be pursued, in cases of this nature.

XII. *On the use of bichloride of mercury in hypertrophy and induration of the uterus, with some remarks on its retroversion and retroflexion;* by Dr. Oldham.

Without concurring in all the opinions contained in this interesting paper, we believe that it is calculated to do much good, by calling attention to the necessity of caution in the use of those powerful local agents which have been so much employed of late, in the treatment of induration of the uterine neck. "I have myself found," Dr. Oldham remarks, "after the use of the actual cautery and the Vienna paste, very copious hemorrhages came on, when the sloughs separated, seriously impairing the patient's strength, and requiring the plug and astringent applications to stop them. Metritis, with pelvic inflammation and abscess, I have known to occur; and from the migratory habits of hospital and private patients, I have, on several occasions, seen females on whom these operations have been performed by others, with abundant traces of injury in the upper part of the vagina and cervix uteri. The latter I have felt decidedly contracted, I might say strictured, by the use of potassa fusa, applied eighteen months before; and adhesions, and puckerings, and radiating cicatrices in the upper part of the vagina have, to my knowledge, resulted from the fluid escharotic, escaping beneath the edge of the speculum, which, in spite sometimes of the greatest care, it will do." These are grave accidents, and the possibility of their occurrence should not be forgotten, though we do not conceive that their frequency is such as to preclude the judicious use of the agents in question. We do, however, object very strongly to the employment of fluid escharotics; every purpose desired can be equally well, and with infinitely greater safety, effected by the potassa fusa; and in the application of dilute acetic acid, after the cauterization, we have the means of limiting and arresting its action with the most perfect readiness.

Dr. Oldham trusts almost entirely to the internal administration of the bichloride of mercury; but the cure in this way is formidably tedious, many months being required in some instances for its completion. He also applies leeches occasionally, and uses vaginal suppositories made up of the ung. hyd. nit.

We do not find anything very novel in the rules laid down for the diagnosis of retroversion. Dr. Oldham disbelieves in the existence of retroflexion excepting as a complication of retroversion with a curvature of the cervix, agreeing in this respect with Dr. Ashwell, in reviewing whose work we have recently dwelt on this subject (see p. 159).

In replacing a retroverted womb, Dr. Oldham trusts chiefly to his own fingers, pressing the posterior wall upwards, and drawing the cervix downwards and backwards. This plan may succeed in some cases, but it will often fail, even when there are no adhesions.

He has been singularly unfortunate in his experience of the uterine sound, having seen metritis follow its use in two cases, in one of which there was also abscess: and he would accordingly restrict its use within very narrow limits. The experience of others, however, seems more favorable than this. In the northern metropolis, and other parts of the country, it is in daily requisition, without the occurrence of any such unhappy results. We have used the instrument ourselves, and have seen it frequently employed by others, but have never even heard a complaint of its giving pain, and have certainly never witnessed any evil consequences. Mischief may undoubtedly be, and indeed often has been, done to the bladder both by catheters and by sounds, but this is no reason why the surgeon should abandon such helps, and endeavour to diagnosticate stone by other means.*

Dr. Simpson's pessaries are equally discountenanced by our author, who appears to place his chief trust in an abdominal supporter and perineal pad. These may be of use in cases of simple prolapsus, but they will not keep a retroverted uterus in its place. We saw a case, not long ago, in which one of the pessaries, above noticed, had been worn for many months without injury, or rather with the greatest comfort, but whenever it was withdrawn the uterus fell back again into its abnormal position. An external pad could have done nothing here.

ART. IV.

Die Hautkrankheiten durch anatomische Untersuchungen erläutert, von Dr. GUSTAV SIMON. Mit 8 Kupfertafeln.—*Berlin,* 1848. 8vo, pp. 394.

Diseases of the Skin elucidated by Anatomical Investigations. By Dr. GUSTAV SIMON. With Eight Copper-plates.—*Berlin,* 1848.

DR. GUSTAV SIMON is well known as an accomplished microscopist, who has particularly devoted himself to the investigation of the structure of the skin and its appendages in their healthy and diseased conditions. We are, therefore, especially pleased to receive from him, at length, a systematic account of the results of his inquiries. He states, in his preface, that the object of his work is threefold. In the first place, he desires to give publicity to a series of observations on the structure of the skin in health and disease, which have occupied his attention for a number of years, and which he has carefully recorded and illustrated by some excellent engravings. But, secondly, he has chosen to extend the work beyond the limits of his own personal investigations, wishing to present the student with a manual of dermatology, which may better correspond with our actual knowledge of the subject than do its predecessors. With this view he has made his book include an account of all the manifold varieties and sub-varieties of cutaneous eruptions enumerated by Willan and Bateman; for our author adopts, at least *pro tempore,* the arrangement of the English nosologists. Many, however, of these varieties he regards as vain, useless, and unphysiological refinements; and mentions them only for the purpose of exposing the futility of that extravagant multiplication of terms and minute diagnosis, in which certain pretentious writers delight to indulge. The work opens with an elaborate anatomical description of the healthy skin;—a description which embraces the latest researches on the subject, and which, though it exhibits little originality, is yet full of interest, dis-playing, as it does, the opinions of a most industrious and accurate microscopist. The next section is occupied with the morbid changes of the cutis and epidermis; including 1st. *Hypertrophies*, as callosities, corns, horny excrescences, warts, ichthyosis, elephantiasis arabum, conditions of hypertrophy of the skin allied to elephantiasis. 2d. *Atrophies.* 3d. *Hemorrhagies.* 4th. *Inflammations;* α, exanthematic fevers, which run a typical course, scarlatina, morbilli, variola, varioloides, varicellæ, vaccinia, febris miliaris, erysipelas, urticaria, erythema, roseola; β, inflammations which run no definite course; αα, *papular*, lichen, prurigo; ββ, *vesiculæ*, eczema, herpes, sudamina; γγ, *bullous*, pemphigus, rupia; δδ, *pustular*, impetigo, ecthyma, equinia; εε, *squamous*, pityriasis, psoriasis, pellagra; ζζ, *furuncular*, boils, carbuncles, ulceration and mortification of the skin. 5th. *Morbid growths* in the skin; formations consisting of areolar tissue; soft warts; nævus mollusciformis; molluscum simplex vel non-contagiosum; condylomata, acuminate and broad; nævus spilus; nævus lenticularis; ephelis; formation of pigment in scars, and after chronic exanthems; benignant melanosis of the skin, or melasma; lipoma; cholesteatoma; telangiectasia; bony concretions in the skin; cutaneous cysts; carcinoma, fibrosum, medullare, melanodes, telangiectodes; ivory cancer, chimney-sweeper's cancer, epithelial cancer, elephantiasis græcorum tuberculosa, elephantiasis græcorum non-tuberculosa; lupus. 6th. *Parasites; α, animals;* lice; sarcoptes hominis, and scabies; acarus folliculorum; *β, plants;* favus (true porrigo); mentagra (sycosis); chloasma (pityriasis versicolor); plants of alopecia circumscripta (porrigo decalvans of W. and B., herpes tonsurans of Cazenave); fungi of plica polonica. The third section is devoted to the consideration of morbid changes affecting the cutaneous glands, hair sacs, hairs, and nails. 1. Changes in the perspiratory glands. 2. Changes in the sebaceous follicles and hair-sacs; as excessive secretion of sebum; scanty secretion of sebum; comedo; larger cysts arising from dilated cutaneous sacs; concretions in the cutaneous sacs; acne. 3. Changes of the hair; excessive formation of hair; deficient formation of hair, as congenital baldness, acquired baldness, porrigo decalvans, herpes tondens; anomalies in the hair-pigment; changes of the hair, as regards its form and consistence; plica polonica. 4. Changes in the nails; α, congenital abnormality, as deficiency, redundancy, abnormal implantation of the nails; β, non-congenital abnormality, as atrophy, hypertrophy, changes in form, in texture of the nails, inflammation of the nail-matrix.

---

* The Editor thinks it undesirable, in order to secure an apparent consistency throughout his pages, to restrain the honest expression of opinion on the part of writers well qualified to form a judgment; and therefore leaves the foregoing statements to have their due weight, notwithstanding the adverse views formerly expressed. (See pp. 149 and 160 of the present Volume.)

Such is a rapid summary of the contents of the work,—a work so rich in original observations and accumulated anatomical details, that little else is left to the reviewer than the task of transferring passage after passage to his pages.—Various opinions have been, and are still held, respecting the intimate constitution of corns; the following is the account of them given by Dr. Simon.

"Corns are callosities which present a prolongation penetrating into the cutis. The prolonged portion fills up a depression in the cutis of from ½ to 1 line in depth, and of a diameter of from one to several lines, and frequently projects upwards through the callosity as a kind of nucleus, which is more or less clearly distinguishable from the remainder of the substance of the corn. Even upon the external surface of the clavus, the nucleus is often recognisable as a round, clear, or dark spot, sharply defined from the surrounding substance, above which it is occasionally elevated.

In a vertical section of a corn, we perceive that its lateral portion, that is to say, the mass surrounding the nucleus, is formed by a thickening of the epidermis. The deeper soft layers of the cuticle do not contribute to this hypertrophy, but remain of their ordinary thickness; while, on the contrary, the quantity of the cells forming the external harder layers of the epidermis is more or less considerably increased. The cells are of the same nature, and stratified in the same manner, as in the normal cuticle. These lateral portions of the corns may also frequently be seen to be traversed by perspiratory ducts. The central mass of the clavus, above designated as the nucleus, is generally harder and more translucent than the rest of its substance; and frequently, either without further preparation, or after brief maceration, it becomes separable as a distinct body, or one only loosely united to the surrounding texture. This nucleus consists of epidermic cells, which in appearance are undistinguishable from the normal cells, but which deviate from them in their disposition; that is to say, most of them stand with their long diameter oblique, and sometimes almost at a right angle, to the surface of the cutis. The direction in which the cells lie always corresponds exactly to the depression exhibited by the cutis under the clavus. From the above-mentioned causes, in fine vertical sections, instead of the horizontal markings otherwise observed in the epidermis, the central portion of the corn presents a multitude of arched lines which run parallel with the margin of the depression in the cutis; and on pressing or tearing fine lamellæ, the thickened cuticle here always divides into accurate curved pieces. Where the perpendicular cells of the nucleus border on the normally stratified margins of the clavus, the union between both portions is often only loose, and the nucleus is then easily separable from the rest of the mass. The characters just described are best marked in those large corns which deeply penetrate the cutis; whereas in those which are but slightly developed, the nucleus is less distinct from the rest of the mass. Sometimes two nuclei are found in one corn. As regards the depression in the cutis, its depth varies much in different cases. Where it appears but shallow, the cutaneous papillæ of the depressed spot are often still distinctly recognisable. In other cases these are absent, and not unfrequently a more or less considerable portion of the other layers of the cutis disappears. Where the prolongation of the corn is very considerable, the corium is found reduced to an extremely thin lamella. Some observers, as Rayer, assert that they have occasionally found this membrane completely perforated; a condition, however, which I have never been able to perceive. In the case of suppuration under a corn, complete perforation might very well occur. Frequently the blood-vessels under the corn are completely filled with blood-corpuscles—a circumstance which permits us to infer a state of congestion or inflammation during life. Small extravasations of blood, also, are occasionally found in the tissue of the corium or on its surface. Moreover, between the cuticular cells which form the clavus, little desiccated masses of blood are occasionally observed, which unquestionably have been elevated from the surface of the cutis by the growth of the epidermis. In the same way, Rükkert has remarked desiccated pus-corpuscles lying between the epidermic cells. The corns which are formed between the toes, have a somewhat different appearance from those which are formed in other places. The epidermic callosity is preserved in a soft state by the constant cutaneous secretion; and therefore corns in these parts appear white and soft. Frequently the margins project beyond the central portion; and for this reason,—that the nucleus, on account of its peculiar formation, does not dilate when saturated with moisture equally with the lateral portions. In other respects, corns in this situation resemble those in other localities. It has often been maintained, that the cutaneous synovial sacs, the so-called bursæ mucosæ, participate in the production of corns. Brodie may be considered among those who uphold this view; for he asserts that in all perfect corns he has found a bursa, exactly resembling the ordinary synovial sacs, which, when it becomes inflamed, and afterwards swollen with fluid, or filled with pus, causes the painfulness of a corn. This writer assumes that the bursa alluded to is a new formation beneath the clavus, and that it is seated between the epidermis and cutis; propositions which are undoubtedly incorrect.

The relation of bursæ to corns may, I believe, be thus explained. On the dorsal surface of the toes there lie, as is well known, in the region of the articular extremities of the bones beneath the cutis, and between this and the tendons, little synovial sacs, which Schreger has delineated in his well-known work. Over these spots, corns are also most easily produced; a fact which depends for its explanation, not upon the presence of these bursæ, but upon the circumstance that these points are most frequently compressed by the coverings of the feet. That the bursæ have no necessary connexion with the origin of clavi, may be deduced from the fact that frequently no bursa can be found beneath a corn, since corns often arise in places where no synovial sacs lie, as, for example, between the toes. In the case of corns which had formed over bursæ, I could never detect in the latter any change as regarded their size or contents, wherefore, I believe, that as a rule, no special relation exists between the callous epidermis and the bursa lying beneath the corium. Only in those cases might the presence of the bursa in question come under consideration, when a violent inflammation of the cutis beneath the corn is set up. The accumulation of fluid exudation within the sac may then give rise to painful tension, and occasion suppuration, and rupture of the bursa externally. The existence of a large bursa on the ball of the great toe, appears especially to be the cause that it is often so difficult to induce the openings produced by suppuration beneath a corn in this spot to heal............Erasmus Wilson has lately given a description of clavus, which differs greatly from my own account. He asserts that they are formed by a morbid increase in size of the cutaneous papillæ, and that every hypertrophic papillæ is inclosed in a sheath of thickened epidermis. Relying upon this discovery, he asserts that they are nearly allied to warts. Either Wilson has taken the central portion of the corn for magnified papillæ, or instead of a corn he has described and figured a wart. As regards the mode of origin of corns, they always begin, in all probability, as ordinary callosities, (mere epidermic hypertrophies). If continuous pressure be made upon such callosity, the subjacent cutis will be partly compressed from above downwards, partly pushed aside, and so give rise to the above-mentioned depression. Where this is deep, an atrophy of the cutis, produced by such pressure, must always be assumed; for, were the tissues of the corium in such cases only separated from each other, the dislocated and compressed papillæ would always be distinguishable beneath the lateral portions of the clavus. Since this as a rule is not the case, partial atrophy of the tissue of the cutis must occur. To the unusual position of the epidermic cells in the middle portion of the corn, the pressure from without upon the cuticle, as well as the altered direction of the surface of the cutis, may contribute." (pp. 29-34.)

Dr. Simon has examined microscopically two museum preparations, the one presenting an example of ichthyosis cornea, the other of ichthyosis simplex; and is able to confirm the ordinary view of the pathology of that disease. He found every constituent of the skin, save the cuticle, apparently healthy. In one preparation, however, beneath some of the scales, he found little depressions upon the surface of the cutis. He conjectures that in the recent state these depressions may have been filled with blood.

It has been imagined by many authors that the phenomena of Purpura and Petechia may result, not from a local hemorrhage consequent upon the rupture of a minute blood-vessel, but from a mere effusion of serum stained with hematin. In refutation of this view, Dr. Simon gives the following account of the microscopical appearances of petechia:

"On making a section of the skin, we remark that the collection of blood is sometimes situated on the surface of the cutis, between this and the epidermis; in which case the undermost layers of the epidermis are commonly also infiltrated with blood. In other cases, the ecchymoses are seated in the tissue of the corium, and in others again in the subcutaneous areolar tissue. Extravasations, lying beneath the cutis, are only perceptible as dark spots in the uninjured skin, where the corium is very thin, as on the eyelids. Where the corium is thicker, they are not evident upon its external surface. Not unfrequently, collections of blood appear in the same spot in the corium and the subcutaneous areolar tissue. I have often investigated in the dead subject cutaneous extravasations which have appeared in various diseases, as purpura simplex and hemorrhagica, typhus fevers, and the last stages of wasting diseases. The extravasations (which presented the form of petechiæ) always depended upon exudations of blood into the tissue of the corium; and sometimes appeared in the superficial layers of this membrane, whilst they sometimes extended through its whole thickness. In all petechiæ which I have investigated, the blood-corpuscles were distinctly recognisable. The red coloration of the tissues either depended upon these corpuscles alone, or the tissue of the corium was coloured blood-red for a short distance around little collections of blood-corpuscles. On the lower extremities I have commonly found these petechiæ so disposed, that every spot inclosed a hair issuing from the skin. In a vertical section of the skin, it appeared that the effused blood encircled the hair-sac. Dr. Virchow has communicated to me that he also has often remarked the existence of little extravasations, which surround the hair-sacs; and Becquerel, in a case of scurvy, has observed the same thing in the petechiæ on the lower extremities. In more extensive extravasations, such as occur especially in the subcutaneous areolar tissue, clots of larger or smaller size, consisting of coagulated fibrine, are often found." (p. 67.)

From the description of purpura, Dr. Simon passes on to discuss the subject of Inflammation—the "still-vexed Bermoothes" of medicine. Dr. Simon commences the description of those alterations of the skin, which form the basis of the classification of Willan and Bateman, with an account of Maculæ. These he describes in the ordinary way, but afterwards proceeds to remark that in those maculæ which disappear by resolution, as a rule, no kind of inflammatory exudation is perceptible; yet that such in small quantity must have been poured out, we may infer from the fact, that frequently, some time after the disappearance of the maculæ, the epidermis desquamates. This exfoliation can scarcely arise from any other cause than the accumulation of a small quantity of exudation between it and the surface of the cutis.

A second alteration of the skin, produced by inflammation, is the appearance of papulæ. Concerning the immediate mode of origin of papulæ, doctors differ. Henle says that they are produced by small collections of inflammatory exudation, in single circumscribed spots of the skin. Rokitansky also declares that the formation of papulæ is an inflammatory process, in which an inflammatory product is deposited in the parenchyma of the corium, and indeed in that of the papillary body or in its deeper layers. Rosenbaum, Lessing, Klencke, and Hebra assume that papulæ are produced by disordered sebaceous follicles. Wishing to set this question at rest, Dr. Simon determined to institute a careful investigation of the subject. With this view he performed the following experiment:

"I rubbed into the inner surface of the thigh of some dogs (after the hair had been shaved off) a pretty strong antimonial ointment, which in these animals, after a few days, commonly causes an eruption of pustules. In one of the dogs thus treated, there arose after the second inunction, on the third day, little swellings in several places, which exactly resembled the papulæ on the human skin, that is to say, they were collicular elevations, about a line in height, and which presented about the circumference of a lentil. They were redder than the neighbouring skin, and somewhat harder to the touch. The animal in which this alteration of the skin appeared, was killed by a blow on the head, and immediately subjected to examination.

The little swellings immediately after death had become smaller, and manifested, in vertical section, the following appearance: the epidermis was neither thickened nor otherwise altered; it was not separated, but held everywhere firmly to the subjacent texture. The papulous swelling was produced by an elevation of the cutis, which formed at the spot where the papulæ existed a little eminence, and appeared at the same time reddened. The redness was caused by single blood-vessels, recognisable by a magnifier, and was situated in about the upper half of the corium. The tissue of the cutis, in the site of the papulæ and its immediate neighbourhood, was observed to be much infiltrated with fluid, as was especially evident on squeezing the skin.

"The fluid was as clear as water, and of somewhat thicker consistence than the serum of the blood. Under the microscope, with the exception of a few molecules, $\frac{1}{1000}'''$—$\frac{1}{5000}'''$ in size, and single globules of fat, nothing could be detected. In thin vertical lamellæ of the diseased spot of the cutis, under a higher magnifying power, the vessels were seen to be gorged with blood-corpuscles, but in other respects the tissues of the skin appeared normal. In pieces of skin torn asunder, the fibres of the cutis seemed to possess a perfectly regular constitution. The hair-sacs and sebaceous follicles were unaltered. I had, at a later period, an opportunity of examining a papular eruption in a dog, which had not been artificially produced. The papulæ were partly broad and flat, partly smaller, and more acuminate, and had exactly the same characters as those produced by art, except that the redness and infiltration of the cutis with fluid were less marked. My observations on the papulæ of the human body accord exactly with the results of my investigations in dogs; but in the papulæ of men, I could perceive still more certainly than in those of dogs, that they are not caused by an increase in size of the cutaneous papillæ, as many maintain." (pp. 86-8.)

Equally interesting is the account which this excellent observer has given of Bullæ, Vesications, and Vesicles. By the term bullæ, he remarks, we indicate collections of fluid exudation beneath the epidermis, which is thus separated to a considerable extent from the cutis, and rises as a collicular elevation. The manner of origin of bullæ may be easily observed by watching from the commencement the operation of a blister. We then remark that at first inflammation of the skin is set up, which we recognise by the redness and other tokens of the inflammatory process. At a later period there arise, in consequence of the separation of the cuticle by the exudation accumulated beneath, little vesicles, which gradually increase, then coalesce, and form large bladders. When the production of bullæ occurs in a locality where the hair-sacs stand far apart, we sometimes see that the epidermis remains longer in connexion with the corium at the points where the hairs come from the skin, than at those spots which lie between the hairs. The surface of the growing bulla, upon which the openings of several hair-sacs are always visible, then presents, in the region of these openings, depressions and elevations in the intervening spaces; which arise from this circumstance, that the hair-sacs united with the cuticle prevent the elevation of the latter. As the collection of fluid, however, becomes greater, the hair-sacs are torn through; and the elevation of the epidermis becomes uniform. If the separated cuticle be examined on its under surface, we find there a multitude of little appendages, the upper ends of the hair-sacs. The contents of artificially-produced vesications present at first a clear fluid, which at a later period becomes opaque. More exact investigations have shown that the contents of these vesications, soon after their formation, have not the same chemical composition as at a later period. For if the vesications be opened soon after their appearance, their fluid contents are found to accord in composition with the serum of the blood; but if we wait longer, this fluid contains, in addition, fibrine, and, for the most part, does not coagulate until it has been evacuated. Still single flakes, which resemble coagulated fibrine, are often found within the vesications. According to an analysis by Margueron, the fluid of a vesication produced by a blister, contained—78 water, 18 albumen, 2 chloride of sodium, 1 natron, 1 phosphate of lime. The same constituents, but in somewhat different proportions, were found by the same observer in the vesications excited by burns and sinapisms. At a very early period there also commences in the exudation of a vesication the development of cells, which completely agree with those of pus. Vogel found them completely formed, upon examining at mid-day the serum of a vesication which had been produced by a blister applied in the morning. Nevertheless, we cannot draw perfectly safe conclusions from the presence of these cells in the contents of vesications, as to the time required for the development of pus-globules, since these latter are not distinguishable with certainty from the cells of the deepest layers of the epidermis.

But it is extremely probable that pus-globules are rapidly formed in vesications, since Heine, Müller, Reinhardt, and others have seen new cells, which entirely resembled pus-globules, originate in the surface of wounds (where it is not so easy to confound cells newly formed, with those previously existing) within the space of a few hours. In vesications more advanced in age, a multitude of pus-globules are always found; and the turbidity of the originally clear fluid principally arises from this cause. When the latter contains a very large quantity of pus-globules, it appears yellowish, and resembles thin pus. On account of the thinness of the epidermis, the vesications commonly at length rupture; and the elevated epidermis, with a portion of the contents of the vesications, dries into a laminar scab, beneath which proceeds the development of a new cuticle. Vogel and Lebert have observed more narrowly the manner in which the reparation of the cuticle, destroyed by the vesication, is effected. From vesications produced by blisters, they removed the elevated epidermis, and traced the process going on upon the denuded surface of the cutis. Vogel found that, after about four hours, a thin, soft, whitish membrane had formed, which consisted entirely of cells, and completely resembled pus-globules. Lebert, who instituted his examination thirty-six hours after the removal of the cuticle, perceived a similar membrane. This contained cells, which agreed with pus-globules; and in addition, others which possessed a simple nucleus filled with a granular substance, and exhibited a diameter of $\frac{1}{167}'''-\frac{1}{143}'''$. These cells were already adapted to one another pavement-wise. Lebert remarked also both kinds of cells in a tolerably transparent fluid, which existed beneath the membrane. The cells described were, after some days, flat and polygonal, and composed a membrane which resembled the normal epidermis. At a later period, this cuticle exfoliated several times, and was replaced by newly-formed layers; but at length this irregularity also disappeared.

Vesicles differ from vesications only in point of size. They originate from a similar cause, and pursue a similar course of development; yet there are one or two circumstances which require more particular notice.

"Many writers are of opinion that in vesicles coagulated fibrine is often present.

For we may often remark that the inner layer of the cuticle which forms the wall of the vesicle, as well as the cutis at the base of the vesicle, is covered by a layer of white substance. This mass is commonly regarded as a false membrane, consisting of coagulated fibrine, to which, at first sight, it bears a resemblance. But, investigated more closely, it is found to contain single flakes and fibres, which, under the microscope and towards reagents, comport themselves as coagulated fibrine, but which consist, notwithstanding, in greater part of the inferior broken-up layers of the cuticle. And when, after removal of the adherent white mass, we examine the epidermic wall or covering of the vesicle, we may convince ourselves that we have before us only the superior layers of the cuticle. Single little fibres, which appear to be indeed coagulated fibrine, are also found occasionally in the fluid contents of the vesicle, suspended between the pus-globules. I have convinced myself of these properties of vesicles by frequent investigations of the vesicles of variola." (p. 93.)

Two peculiarities which characterise particularly the vesicles of variola, have engaged the attention of pathologists, and given rise to much altercation. Many clever theorists have attempted to account for the multilocular interior and umbilicated apex, which confer upon the variolous vesicle its individuality. Thus, Cotugno, Deslandes, Petzoldt, and Dr. Heming thought that the umbilical depression corresponded with the mouth of a sebaceous gland, and that, as a consequence of the firmer attachment of the epidermis at this spot, it remains longer in connexion with the cutis. Eichhorn, on the other hand, imagined that the central depression arises from the desiccation of the fluid earliest exuded. In this manner, he thought, a little horny crust forms between the epidermis and cutis; and this is the cause that, upon the occurrence of more abundant exudation, the cuticle is not elevated at the spot where the crust lies. That the contents of the vesicle are contained in cellular spaces he admits, but places these spaces between the layers of the corium. According to Rayer, there exists in the smallpox pustule, between the cutis and cuticle, a false membrane, which in shape resembles a truncated cone, and has a thickness of half a line. This false membrane consists of a dull white, tolerably firm, but friable substance, which is firmly connected with the under surface of the epidermis, but less closely with the surface of the cutis. In vesicles advanced in development, between the external surface of the corium, and this newly-formed white layer, several little cellular spaces filled with serous fluid, or a tortuous canal containing serous fluid, are described by this anatomist. In a vesicle, therefore, presenting this structure, Rayer supposes the central depression to depend upon the false membrane above described. Erasmus Wilson holds nearly the same opinion—

"When a well-formed and mature pustule," says he, "is examined by dissection, it is found to be divided in its interior by a transverse septum into two chambers, both containing pus. The upper chamber is the larger of the two, and they communicate with each other, to a greater or less extent, by the rupture of the transverse septum around its marginal border. The epidermis forming the superficial boundary of the pustule, is the segment of a sphere, and is continuous by its circumference with the cuticle covering the adjoining skin. The transverse septum is a layer of false membrane, of a whitish colour, which was deposited on the dermis at an early stage of the pustule. Subsequently, this layer becomes separated from the dermis, and raised by the formation of pus beneath it; and at the same time it is broken around its edges, and permits the pus of the deeper cavity to communicate with that already contained in the superficial chamber. In consequence of the peculiarity in the mode of its production, this layer of false membrane generally retains permanently the umbilicated form of the primitive pustule, and is thinner at the centre than towards its circumference. When this septum is removed, the deep chamber is brought into view, and the depressed, and sometimes ulcerated base of the pock exposed. The surface of the base is of a bright or purplish red colour, and highly vascular............I am disposed to believe the umbilicus of the vesicle to depend on the close adhesion subsisting between the epidermis, and the first-affected papilla of the dermis. The inflammation of this papilla has almost completed its course before that of the surrounding papillæ has taken on the effusive stage, the central papilla consequently occupies a different pathological position to the rest of the papillæ involved in the variolous inflammation. This opinion seems confirmed by the multilocular character of the vesicle at its height, the only period when its true character can be determined. Thus Gendrin compares its appearance, when examined by means of a transverse section, to a spice-box; while Bousquet likens it to a severed orange." (Treatise on Diseases of the Skin, p. 59.)

It will be seen from the following quotation, that Dr. Simon views these appearances in a different light:

"I have often," he remarks, "had an opportunity of investigating variolous pustules in living persons, and also occasionally in the dead subject. I have found that they are not always constituted entirely alike. In many cases where a central depression clearly existed, the epidermis was entirely raised from the subjacent cutis; and only at the spot corresponding with the umbilicus were both membranes united by a thin, whitish cord, which, as the microscope evidently showed, was a hair-sac. Sometimes several closely-approximated hair-sacs existed beneath the depressed spot. Upon the under surface of the epidermis, and for the most part also upon the upper surface of the cutis, was found a thin layer of a whitish mass, which, when looked at with the naked eye, possessed the characters indicated by Rayer. The layer fixed to the epidermis was not connected with that lying upon the cutis; nor when this latter layer was absent, with the cutis itself; but the hair-sacs, ascending from the cutis to the epidermis, alone connected the two membranes. It cannot be doubted but that in pocks of this nature, the hair-sacs are the cause of the umbilicus; for, except these hair-sacs, nothing exists which can interfere with the uniform elevation of the epidermis. In localities where very large sebaceous follicles, in connexion with small hair-sacs, exist,—as, for example, in the face,—the efferent duct of these glands will very probably detain the epidermis in the same manner as I have just described with respect to the hair-sacs; but these localities I have never examined.

"Other vesicles manifested a structure somewhat different from the above. In these also there appeared beneath the epidermis a white substance: but this, at the spot where externally the umbilicus was visible, adhered to the surface of the corium, so that the epidermis seemed fixed to the cutis by this white mass alone. Sometimes, indeed, in the depressed spots of such vesicles, neither hair-sacs nor sebaceous follicles could be found, which could contribute to detain the epidermis. In other vesicles of the same kind I saw, on the contrary, one or more hair-sacs passing through the white mass. Frequently also hair-sacs existed beneath the elevated borders of the vesicles. This was observable in vesicles in which hair-sacs existed in the neighbourhood of the umbilicus, as well as in those in which no hair-sacs were seen at this spot. The hair-sacs found beneath the elevated borders of the vesicle, were frequently still united by their upper extremity with the elevated epidermis, but occasionally they were torn at some distance from the cuticle.

"Not unfrequently I could still clearly recognise the sebaceous follicles in union with the hair-sacs; they were either uninjured, or so torn that only the upper end of the glands remained in connexion with the hair-sacs, which, by the ascent of the cuticle, had been somewhat drawn up from the cutis.

"In like manner I could frequently perceive the openings of the perspiratory canals, when I examined the epidermic covering of the pock with a magnifier, or under the microscope. In many cases also, the perspiratory canals themselves became manifest in a fine section of the vesiculated skin. But in the same vesicle the openings of the canals and the canals themselves were almost always distinguishable not only in the depressed spot, but also in the margins of the vesicle.

"Now as regards the white mass beneath the epidermic covering of the vesicles, and often also apparent upon the surface of the cutis, which most modern writers take for a false membrane, it consists, for the most part, of the deeper softened layers of the cuticle; for we recognise most externally, large flat cells; beneath these cells, others less flattened, provided with manifest nuclei; and next to the cutis, the cells and nuclei of the so-called rete Malpighii. Besides these constituents belonging to the cuticle, certain others are found. For, first, we discover cells which, in their round form, granular appearance, and manifold nuclei, entirely agree with pus-globules. These, in fact, originate in the fluid contents of the vesicles, in which, even while they are still clear, pus-globules are discoverable. In addition, there were remarked smaller granules, whose size varied extremely, since their diameter fluctuated between 0·0007‴, and 0·002‴. The majority of the corpuscles of this kind were round; others had an irregular form, which, however, approximated to the spherical. Many of the larger ones were finely granular, and all were insoluble in acetic acid. The smaller of these corpuscles were certainly new formations; and of part of the larger corpuscles the same may be assumed with probability, although the latter cannot be discriminated with complete certainty from the normal constituents of the deepest layers of the epidermis. Occasionally also, I saw a number of the granules in question imbedded in little cords and plates; which, when pressed or torn, manifested the same fine fibrillation as coagulated fibrine; and agreed, moreover, with the latter in this, that, on the addition of acetic acid, they became so clear and transparent as to be hardly visible, whereupon the granules became so much the more distinct.

"Now, as we are accustomed to give the name of false membranes to layers of coagulated fibrine, we cannot include the above described white mass under this appellation; for, as I have before shown, the epidermic elements always form its major part. And the truth of this proposition appears from the fact, that if we examine the epidermic covering of the vesicle, having previously removed the adherent white mass, we find only the external and superficial layers of the cuticle—the deeper layers being absent.

"In many cases extremely little of the white mass is present. I have seen this quite recently in a child that died of variola in the Smallpox Hospital of this city. This deviation might arise from the circumstance that, on account of the very rapidly supervening separation of the epidermis, its under layers did not break up so completely as usual; and that, at the same time, the newly-formed substances found between the epidermic particles, had been produced in very small quantity. Probably, however, great thinness of the epidermis contributes most to the effect in question. If this be the case, the white mass must always be thinner in children than in adults. Judging from a very small number of observations which I have had the opportunity of making on children, this indeed appears to be the case.

"The fact that the described white mass, at certain spots of the vesicle, is prolonged uninterruptedly from the under surface of the epidermis to the cutis, while at other points this connexion is interrupted by the dissociation of the under layers of the epidermis, or by the complete separation of the cuticle from the cutis,—this fact, I repeat, is the occasion of the little divisions or compartments, mentioned by most authors. These are usually of unequal magnitude and irregularly arranged; but sometimes I have seen them arranged with tolerable regularity. In cases of the latter kind, the white mass extended itself from the middle of the vesicle in the shape of little septa, like the rays of a circle, to the periphery, so that six or eight chambers of tolerably equal size were formed. In the middle of the vesicle, from which the irregularly arranged septa proceeded, a hair-sac was sometimes found. Frequently no separate compartments at all existed. In vesicles of this sort the epidermis was connected to the cutis in the middle by a thin white cord, or over a larger space; and around this centre ran a canal, into which the white mass

> extended, in the form of little clusters or lamellæ; but the canal was at no point interrupted by a complete partition. Rayer appears to have observed the same thing." (pp. 124-8.)

That the production of an umbilicus is not dependent on the presence of a hair-sac or sebaceous follicle, is easily proved; for it is well known that neither hair-sac nor sebaceous follicle exists in the hollow of the hand and foot; yet in children, in whom the thin epidermis permits it to appear, the central depression is frequently excellently marked. A remarkable peculiarity, first noticed by Rayer, characterises the variolous vesicle, formed beneath the horny cuticle, which, in the adult, invests the palm of the hand and sole of the foot. On removing the epidermic cap of the vesicle and wiping off the fluid collected beneath, he observed that in the centre of the denuded corium a little elevation existed, while the circumference was visibly depressed below the level of the neighbouring healthy cutis. Dr. Simon has examined these points microscopically; and he finds that the central elevation consists of a file of papillæ of normal or nearly normal size and condition, while the depressed margin is paved with papillæ, bent down or flattened upon each other, as the standing corn is bowed by a high wind. The explanation of these phenomena is simple. At the centre of the vesicle, an organic connexion (from some yet unexplained cause) exists between the cutis and cuticle, and at this point no fluid is effused; but around this centre, exudation of fluid occurs without impediment, and tends to force the cutis and cuticle asunder. In other localities the cuticle yields, and rises everywhere except at the umbilicus of the vesicle; but here the texture of the cuticle is so dense, and presents such great resistance to the distending power, that the cutis itself yields first, and sinks below its ordinary level. Dr. Simon tells us, that we may readily study the constitution of the epidermic cap of vesicles, which appears in these situations, by carefully examining the crusts formed on desiccation.

> "The epidermic covering of the dried pustule commonly cracks spontaneously, or, if this is not the case, easily admits of division. Beneath this is formed a round, brownish, lenticular lamina, which by its middle is often still adherent to the skin, but which is easily separable. If this lenticular body be softened in water, we see that it consists of two lamellæ, commonly connected by their margins with each other. The inferior of the lamellæ has in its centre a round hole; the superior is not perforated. Between both lamellæ the microscope makes manifest a granular mass, the desiccated pus of the pustule. That both lamellæ consist of epidermis is clearly perceptible; their lamellæ are often seen traversed by perspiratory canals; and, by sufficient maceration, we may easily isolate and bring to view the individual epidermic cells. The superior lamella belongs to the middle layer of the epidermis, the inferior to the deepest. Unquestionably the inferior lamella represents the mass which, in the variolous vesicle, we find ordinarily as a thin, whitish layer, overlying the surface of the cutis; while the superior lamella arises from the desiccation of the white substance adherent to the under surface of the epidermic cap. The inferior lamella is perforated, because it surrounds the middle elevated portion of the cutis, which remained in connexion with the epidermis." (p. 132.)

Of the anatomy of Pustules, Dr. Simon remarks, that the pus frequently exists between the cutis and cuticle, according to the popular notion; the process of suppuration commencing upon the surface of the cutis; but that, frequently, also, the suppuration begins in the tissues of the cutis, extending thence beneath the epidermis. In dogs, anointed with antimonial salve, he often remarked little collections of pus in the substance of the cutis, the cutis and cuticle remaining adherent. In a dog, also, which suffered from a papular eruption, not artificially produced, he found, on bisecting a little cutaneous tumour, which resembled a papule, at some distance from the surface of the cutis, a minute abscess. Similar appearances were occasionally detected in incipient acne-pustules in men.

The distinction between the phlygacium and psydracium, Dr. Simon regards as insignificant and valueless.

The peculiar wheals (*quaddeln, pomphi*) which characterise the eruption of urticaria, he declares to depend upon inflammation; for the following reasons: The swellings are frequently reddened, and connected with increased heat and abnormal excitement of the nerves of sensation, as well as the presence of an exudation in the interstices of the cutis. For, if a wheal be punctured with a fine needle, the puncture extending as deep as the cutis, a few drops of a clear serous fluid will exude from the little wound.

The anatomy of the skin in measles has been investigated by our author in a very original manner. He remarks: "As I was not aware that an exact examination of the skin in measles had ever been instituted, and as I had no opportunity of undertaking the investigation on the dead subject, by means of a bent pair of scissors I cut out of the skin of a boy a rubeolous pimple, together with a small portion of the subjacent cutis." With great *sangfroid* he continues: "Since this little operation produced very slight pain, and gave rise to no inconvenience of any kind, I repeated the experiment at a later period upon several other persons." This was indeed a truly German idea. In the absence of dead bodies to cut up, why not amuse ourselves by cutting up the living? Shall we allow our scissors and our microscope to lie idle? Now, we confess that such a little operation, attended with no pain, nor productive of the slightest inconvenience, frightens us. We shall take especial care in our illness not to fall into the hands of the microscopico-pathological physician. Shivering in the cold stage of ague, our tremors would redouble at the aspect of our physician, approaching with his scissors, and intent upon procuring a thin section of our skin, to be submitted to the microscope. Nay, had we merely a pimple on the nose, we would beware of Dr. Simon; for we could by no means share the stoical indifference with which, while engaged in slicing our little excrescence into thin lamellæ, he would assure us that, for the benefit of science, we ought to disregard minor considerations. If, however, in spite of our protestations, Dr. Simon's example should prove contagious, and the scissors, as a means of diagnosis, should become to cutaneous diseases what the stethoscope is to affections of the chest, we fear that the profession will incur the obloquy of the indiscriminating public; for we may imagine, for example, what feelings would be roused in a mother's bosom, when the medical attendant, anxious to distinguish between a roseolous and a rubeolous eruption, should draw a pair of scissors from his pocket and snip out a small portion of the infant's cuticle. Such a time, should it ever arrive, would surely be the quack's milennium. Let us, however, follow Dr. Simon further, and see what he made of the amputated pimple. Having brought a thin, perpendicular section under the microscope, he found that the epidermis was unaltered and continuous with the cutis. The projection was caused by swelling of the cutis. With the exception of a very few, small, round molecules, insoluble in acetic acid, no foreign ingredient was perceptible. The papillæ and areolar fibres of the corium were unchanged. But he thinks, that, when we remember that the artificially-produced papillæ in dogs depended upon the presence of an interstitial exudation, we may readily conceive that the much smaller papulæ of measles depend also upon an exudation much smaller in amount. He concludes, from the appearances observed, that neither the hair-sacs nor the sebaceous follicles have any influence in the production of the eruption.

As regards the vesicles of Varicella, Dr. Simon believes that the reason why they present neither the umbilicus nor the multilocular character of the variolous vesicle, is simply this,—that, in varicella the fluid is poured out with greater rapidity than in variola; that the cuticle therefore escapes the continued maceration necessary to separate its constituent laminæ; and the central depression is absent, because the stage of fluid effusion is not preceded by that stage (whatever be its nature), in which adhesion between the cutis and cuticle is effected.

The central depression in the vaccine vesicle, Dr. Simon believes to result from that adhesive inflammation, the result of the puncture, which precedes the supervention of the specific inflammation. In accordance with this theory, he has observed that the form of the depression always closely corresponds with the form of the puncture. If vaccination be effected by a simple puncture, the resulting umbilicus will present an orbicular appearance. If a cut be made into the skin, the vesicle produced will be oval in form, and manifest a long umbilicus, the deepest point of which will exactly correspond with the incision. That the umbilicus is entirely independent of the hair-sacs and sebaceous follicles, is proved by the following case, related to the author by Mr. W. Cosson, of the Vaccination Institute at Berlin. In 1845, a child, during the process of vaccination, was accidently wounded by the vaccine-needle in the inner surface of the hand. At this spot a vesicle formed, which ran a similar course with that upon the upper arm, and presented a manifest umbilicus. Now, it is well known that neither hair-sac nor sebaceous follicle exists in the inner surface of the hand.

In the names, by which certain so-called varieties of lichen are designated, Dr. Simon wishes to introduce a few changes. Thus, he would follow Hebra in altering the name of Lichen lividus to Purpura papulosa; Lichen urticatus he would term Urticaria papulosa; and Lichen pilaris he considers a form of acne.

It was well known to Willan, that the circumscribed swellings of the skin in Prurigo depend upon little accumulations of a clear serosity; for he remarks, that, if the papulæ be rubbed or scratched, a limpid fluid will exude. But the German pathologists, unwilling to rely solely upon this evidence, have adopted the ingenious expedient of stabbing the pruriginous pimples with needles, and thus satisfying themselves by scientific experiment that no fallacy lurks, like a snake in the grass, under the natural experiment of rubbing and scratching. First Hebra, and then Simon, have pursued this plan, and with entire success. They always find, when they can get any one sufficiently philosophical to endure the process, that every papula in a patient suffering from prurigo, may be proved by acupuncture to contain a serous exudation. Simon has gone farther. By means of his admirable invention—the dermatic scissors—he has clipped little bits of cutis and cuticle from his astonished patients, and analysed them by the microscope. He has discovered that the exudation does not lie between the corium and epidermis, but is seated, like that of lichen, in the substance of the former membrane.

Dr. Simon makes the following remarks upon the nature of Pityriasis versicolor, or Chloasma, as it has recently been called:

> "In this disease, yellowish or brown spots of various size appear; upon the surface of which desquamation of the cuticle, in the form of thin scales, commonly occurs. Eichstedt has shown that, in this affection, parasitic fungi are formed upon the skin; for, if we examine the scales thus formed, which are easily rubbed off the skin, we see that they are composed of epidermic fragments, between which lie groups of fungi. These fungi resemble those of favus, for we recognise cells and thallus-threads. The cells are commonly round, and lie together in heaps. Of the thallus-threads, Eichstedt asserts, that they are finer than those of favus, since they have only two thirds of the diameter of the latter; moreover, they appear for the most part short, serpentine, and rarely branched. I also have examined several persons suffering from Pityriasis versicolor, and have in all cases perceived the fungi described by Eichstedt. The cells possessed, according to my measurement, a diameter of 0·0008 — 0·002‴, the threads a thickness of 0·0012‴. I have never seen them serpentine, as Eichstedt has described, but commonly running a straight course, and divided in a forked manner. From the constant appearance of these fungi in Pityriasis versicolor, Eichstedt believes that they are nearly related to the essence of the disorder. He also quotes an example, in which the affection was transferred from a man to two others. I regard it, nevertheless, as unproved, that the fungi play so important a part in this disease; for it must first be proved more exactly that the pityriasis is indeed produced by them, and that the desquamation of the cuticle does not arise from another process,—for example, the process of exudation,—the fungi subsequently developing themselves among the scales." (pp. 311-12.)

It is generally imagined that the peculiar coloration of pityriasis depends upon the genesis of new pigment in the rete Malpighii, but the researches of our author render this hypothesis very questionable. He examined in one case the skin, which was strongly brunette, and found everywhere, in the rete Malpighii, pigment-granules; but, in the diseased tissues, these pigment-granules were neither darker in colour, nor present in greater abundance, than elsewhere. In other cases of blond skin, in which the disease existed, no pigment-granules of any kind could be detected beneath the altered cuticle, but the scales themselves were distinctly seen to be spotted with yellow stains. Where these yellow stains existed, there also the fungi were found in abundance. Moreover, the single epidermic cells, overlying the yellow stains, examined separately, appeared of precisely normal character, and did not exhibit any unusual contents. Dr. Simon supposes, therefore, the yellow spots of Pityriasis versicolor to depend upon the fungi, developed in that affection.

Dr. Simon, it is well known, has particularly studied the minute constitution of warts and other allied excrescences, and he has been led to adopt certain views, which differ somewhat from those ordinarily entertained. He describes, indeed, the common hard or horny warts, in much the usual manner; regarding them as hypertrophies of the cutaneous papillæ; but, of the soft warts (*verrues charnues* of the French), he takes quite a different view. These he considers to be new formations of areolar tissue.

"Under the name of soft warts are known those little appendages to the skin, which commonly have the circumference of one or more peas, are much softer than hard warts, and are connected with the skin either by a pedicle or broad base. They are invested by a cuticle, which entirely agrees in structure with the normal epidermis, and which, in respect of thickness, differs little or not at all from the neighbouring normal epidermis. It is never so strong, hard, and horny, as in ordinary warts. Not unfrequently we find, in the deepest layers of this cuticle, pigment-granules, which impart a yellow, brownish, or blackish hue to the swelling. The surface of the warts, especially of those which are coloured, is sometimes studded with hairs. Beneath the cuticle lies the substance of the wart, which, divided transversely and regarded with the naked eye, appears uniform, and not, as in Verruca vulgaris, composed of a number of erect aggregated cylinders. Frequently, especially in the larger warts of this kind, we recognise a looser texture in its middle part than in the more external layers; wherefore many regard these excrescences as saccular eminences from the corium, into which projects a growth from the loose subcutaneous areolar tissue. On investigating the substance of these warts under the microscope, we find that, disregarding the cuticular envelope, they consist of areolar tissue, which commonly contains, among many completely-formed and fasciculated fibrillæ, additional constituents, which are regarded as earlier stages of development of this tissue, as cell-nuclei, and caudate corpuscles. In those cases in which the external portions of the tumour are thicker than the internal, we may perceive that, in the former parts, the fibrils of areolar tissue are more intimately interwoven, after the manner of the cutis; while, as we approach the centre of the tumour, they are less firmly connected, and may therefore be more readily torn from each other. The stalk by which these warts are frequently united with the skin, consists also of areolar tissue. Not unfrequently a vascular rete is distinctly perceived to traverse the substance of the wart. Sometimes we find, in the tissue of these excrescences, some hair-sacs and sebaceous follicles, which not unfrequently are blocked with sebum, and converted into comedones. The appearance of these follicles is not constant." (pp. 216-17.)

With these soft warts, which are usually congenital, Simon classes those larger congenital tumours, which have been named *Nævi mollusciformes*, or *N. lipomatodes*, and cases of *Molluscum simplex*. He asserts that they differ one from the other merely in size, consisting all of them essentially in a new development of areolar tissue. All congenital areolar-tissue tumours, he proposes to include under the name devised by Fuchs, of *Ochthiasis:* acquired areolar-tissue tumours he would call *Molluscum simplex*. Nearly related to these formations are the fig-warts (fici, condylomata), which present two varieties, the broad and the acuminate. The broad or flat variety is almost universally considered of syphilitic origin. It has been variously denominated *Pustule plate* (Cullerier), *Tubercule syphilitique plat* (Rayer), *Pustule muqueuse* (Devergie), *Papule muqueuse* (Ricord). Concerning the origin of the acuminate condylomata, there is less agreement among authors.

"The acuminate condylomata," says Simon, "resemble ordinary warts in this respect, that they are almost always composed of a number of single papillæ; only in rare cases does a condyloma appear to consist of a single thread-like or clavate appendage to the skin, which is not branched (Krämer). In fig-warts, which are formed of several papillæ, these latter vary greatly in length. Sometimes, on the external free extremity, they are perfectly acuminate, sometimes they are rounded off, sometimes button-headed or clavate. The papillæ lie partly above, partly beside each other, and are connected at their base. Sometimes the base is tolerably broad, sometimes the excrescence is only connected to the skin by a slender stalk."............ (p. 222.)

"If thin pieces be taken from an acuminate condyloma, which it is extremely easy to do, when the wart has been macerated a short time in water, the external layers of the excrescence are seen under the microscope to consist of flat cells, disposed after the manner of pavement-epithelium. More internally the cells are found to be flattened also, but smaller, since the cell-wall embraces the nucleus more closely. In the most internal layers of the papillæ, comprising the condyloma, or in the central mass into which the papillæ are inserted, we frequently find little round cells, in which the cell-wall is only at a very small distance from the nucleus, besides caudate corpuscles and the fibrils of areolar tissue. In the mass formed of these elements appear blood-vessels, which extend nearly as far as the external layers of flat cells. In many acuminate fig-warts all the above-described ingredients appear. In such, the quantity of fibrils and caudate cells is commonly small in proportion to the number of round cells, sometimes also somewhat greater, but never very considerable. I have besides lately examined fig-warts, in which, even when they were torn into minute morsels, and examined bit by bit, no fibrils nor caudate cells were perceptible, but where beneath a layer of large flat cells only small round ones could be detected.

"The flat condylomata consist of little tuberculoid elevations, flattened or even depressed upon their surface, which rarely exceed a line in height; while their diameter varies from that of a lentil to that of a groschen. They have a pale red or darker dirty red colour; their surface is smooth, or marked by slight elevations and depressions, but they are never composed, like the acuminate condylomata, of a number of single papillæ. Not unfrequently they ulcerate superficially, but in other cases they diminish gradually in size, and then vanish without ulceration, leaving commonly behind blue-red or reddish-brown stains which last for some time. The spots in which the broad condylomata appear most frequently are the labia majora, the space between these and the labia minora, the entrance of the vagina, the scrotum, the internal surface of the prepuce, the neighbourhood of the anus, the upper part of the inner surface of the thigh. Less frequently they appear on the lips, the interior of the mouth, between the toes, &c. I have investigated the broad condylomata in various individuals, from whom I have amputated these excrescences with the scissors. They were almost all taken from the neighbourhood of the anus.

"Their structure nearly resembles that of the acuminate condylomata. Two different substances are distinguishable with the naked eye, namely, an external covering which invests the whole excrescence, and an internal substance which consists apparently of a homogeneous mass. The investing substance exactly resembles the epidermis; in the internal substance we find fibrils of areolar tissue, which exactly agree with those acknowledged to be mature areolar tissue, little cells closely enveloping their nuclei, free nuclei, and caudate cells. The latter were deficient in some condylomata, but the fibrils of areolar tissue were always present. Sometimes I have perceived single elastic fibrils. Like the acuminate, the broad condylomata are provided with blood-vessels, which, in a transverse section, are instantly recognised as red points, from which blood may be squeezed out" (pp. 229-30.)

It will be perceived that we have quoted largely from the work before us; but we cannot imagine that the reader will impute it to us as a fault, but rather wish that our critical bucket had descended yet more frequently into the pure waters of the German well:—into the pure waters, we repeat, for rich as they undoubtedly are in genuine fact and close observation, and logical inference, we find mixed up with these none of that querulous self-assertion, nor depreciation of the labours of others, which too frequently dishonours even the most valuable productions of the votary of science.

ART. V.

Observations on Hospital Gangrene; with reference chiefly to the Disease as it appeared in the British Army during the late War in the Peninsula, with Prefatory Remarks. To which are appended, Cases, showing how extensively applicable the Antiphlogistic Treatment is to other Diseases of the Army. By JOHN BOGGIE, M.D., Surgeon to Her Majesty's Forces. —*Edinburgh*, 1848. 8vo, pp. 151.

THIS work is a republication, with additions, of an essay on 'Hospital Gangrene,' written twenty-two years ago, and published in the Transactions of the Edinburgh Medico-Chirurgical Society; and its especial purpose is to advocate bleeding and other antiphlogistic measures, as the general mode of treatment in the peculiar form of gangrene in question. The following extract well explains the views of the author, and gives an interesting account of the duties devolving upon military surgeons during war time, duties of which but little adequate idea can be formed by those exclusively engaged in the more peaceful walks of the profession:

"I lost no time in repairing to the Cordileria, and found Mr. Widmer, Surgeon to the Forces, a very zealous and experienced officer, in charge of that hospital, which contained nearly 1000 wounded men. Dr. Hennen was expected in a few days, on whose arrival Mr. Widmer was to remove to another hospital (the St. Marmes), where the gangrene also prevailed. The Brownonian treatment was enforced at the Cordileria, and had been so, I understood, from the time that the hospital was established. On the arrival of Dr. Hennen, who succeeded to the charge of that hospital, Mr. Widmer took his leave. Dr. Hennen was a man of great talents, and an excellent operating surgeon,—indefatigable and enthusiastic in his profession,—of considerable standing in the service, and seemed quite familiar with the disease in question. I had conceived great expectations from his acuteness, intelligence, experience, and discernment, in so great an emergency, and hoped that a termination would soon be put to the dreadful sufferings I was every day witnessing. The same practice, however, to my astonishment and disappointment, was still continued, as in Mr. Widmer's time, without any improvement, as far as I was able to discover. I soon saw that the means in use would not answer; for, although, as already mentioned, I had never before witnessed the gangrene in Army Hospitals, and, consequently, was unacquainted with the mode of treatment usually adopted there; yet, by attending closely to the symptoms, I was convinced that the principle acted upon, at least in the present instance, was quite erroneous. In the first place, the accompanying fever was not typhus, as was generally believed, but of a different character. It was the synochus, or rather the synocha, of nosologists, attended, in some cases, with hepatic or bilious symptoms: neither did the exciting causes of the disease appear to be at all understood; for, although they might be various, and more than one may have been in operation at the same time, yet that to which I attached the most importance, it being the most obvious, was over-excitement, occasioned by the too free and indiscriminate administration of tonic and stimulating remedies, such as bark, opium, wine, spirits, &c., which appeared to me quite sufficient to produce and to perpetuate the disease. No change of treatment, however, seemed ever to be contemplated. The disease was making rapid progress every day, till at last the whole hospital was overrun by it. As it was thought that no other general measures could be of any use, amputation was had recourse to, and great reliance was apparently placed on that; but, unfortunately, it was of little avail, for, in most cases the disease attacked the stump, so that in not a few the operation had to be repeated; and, if I am not mistaken, there were instances where it was performed three times on the same individual.

In one month, in the Cordileria, one hundred and fifty men had each a limb amputated.

"When things were in this state, Dr. Hennen received an order to proceed to Vittoria on a particular duty, which detained him there two months, during which time the charge of the hospital devolved upon me.

"Seeing that all the means hitherto used had failed, I resolved to take the responsibility of carrying my own views into effect, notwithstanding the prejudices and opposition of all my professional brethren: for, unless a totally different practice were pursued, I was convinced that a very great proportion of the wounded in the Cordileria, which at that time contained more than eight hundred men, must lose their lives, or be rendered useless as soldiers, by the amputation of their limbs. Under such circumstances I could not but feel much anxiety for the safety of these men, seeing that, from the 31st of July to the 20th of October, before the progress of the gangrene was checked, not fewer, I think, than two hundred and fifty deaths had occurred in that hospital, and more than one hundred and fifty amputations had been performed; and I felt assured that had a modified antiphlogistic treatment been adopted from the commencement, the gangrene might have been in a great measure, if not entirely, prevented, and the number of amputations and deaths been comparatively few. But that alarming state of things was soon changed, as will be seen immediately, when the antiphlogistic treatment was *fully established.*

"I commenced by causing a number of cases to be selected, by way of experiment. The antiphlogistic treatment was rigidly enforced,—modified, however, according to circumstances. Bloodletting was used in some: but when the symptoms were less urgent, or there was anything to contraindicate it, that remedy was dispensed with, and the result was most satisfactory. The same treatment was tried in others with equal success. Prejudices at last began to give way, and, in a short time afterwards, the *newly adopted practice became general.* The benefit resulting from that change was soon apparent,—the great mortality almost immediately ceased, and amputation was seldom necessary. Numbers, after a time, were enabled to join their corps, and again take the field, who, had they remained in hospital under the old treatment, must, in all probability, have lost a limb and become a burden on the state, if they were so fortunate as to escape with their lives. In consequence of our success at the Cordileria, the same treatment was adopted in all the other hospitals at that station, and the result was equally favorable." (pp. 6-11.)

Such are the statements of Dr. Boggie, but they must be qualified by a few obvious considerations. Dr. Boggie himself shows afterwards that the disease assumes a different form in the summer and winter seasons. Dr. Walker, in his Report, quoted at page 19, says, that "since the setting-in of the cold weather, the type of the fever seems to have changed entirely, and to have become inflammatory." Dr. Hennen, quoted in the following page, says, that, "about the middle of October, a change of type began to show itself very distinctly, which naturally induced a change of practice." Dr. Walker again confesses that, from the immense duty the medical officers had to perform, sufficient attention was not paid to the "symptoms of particular cases," and that the practice was "too much generalized." Before pursuing, however, the reflections to which these statements give rise, it may be as well to give Dr. Boggie's description of the disease he observed, as it is evidently that of a careful practical man. He says:

"When a wound or ulcer is affected with contagious gangrene, it becomes painful and swollen, loses its healthy florid appearance, and the granulations, which were small and distinct, become flabby, and appear sometimes as if they were distended with air; at other times, vesicles containing a watery-coloured fluid, or bloody serum, have been observed, and the sensation in the sore has been described as resembling the stinging of a gnat. The secretion of pus is suspended; the wound is dry, and covered with a tenacious, viscid, ash-coloured matter, which adheres firmly to the surface. When this morbid state has existed for some time, a discharge takes place, of a thin, ichorous matter, of a very peculiar smell, the pain increases, the edges of the wound are reverted, and in general assume a circular form; an erysipelatous redness surrounds the wound, and sometimes extends to a great distance, even over a whole limb; the neighbouring glands, as those of the axilla or groin, swell, inflame, and sometimes suppurate; febrile symptoms become apparent; the pulse is accelerated, full, and strong; the heat of the surface is much increased; the patient complains of nausea and thirst; the tongue is covered with a whitish or brown crust; and the bowels are in general constipated. The inflammation goes on increasing, the thin ichor continues to be discharged in great quantity, and a thick slough, apparently of coagulable lymph or fibrine, like melted tallow, covers the whole surface of the wound; the fetor becomes intolerable, and the pain quite insupportable. In the last stage, there is, in general, an oozing of blood from the surface of the wound, and not unfrequently distinct hemorrhage, from the corrosion or destruction of the larger blood-vessels. Sphacelus takes place to a greater or less extent; the strength of the patient fails; the pulse sinks; his countenance becomes collapsed and altered; the skin is bedewed with a clammy sweat; and a diarrhœa with hiccup coming on, the scene very soon terminates.

"Though this is the most common form of the disease which I have seen, as it occurs in a recent wound, and in strong, healthy men, who are the ordinary subjects of it, yet I am well persuaded that the fever which accompanies hospital gangrene is not always of so phlogistic a character. It has been often observed to partake more of a typhoid type; and it is of the utmost consequence in practice to attend to this distinction, as it will be found, that what would be a valuable remedy in the one case, might, if carried to any extent, be very pernicious in the other. The not attending sufficiently to this circumstance, that is, to the phlogistic, or to the typhoid type of the fever, has, I am convinced, often led to fatal mistakes, and seems to be partly, at least, the cause of that great diversity of opinion among medical men, regarding the best mode of treating this very dangerous affection.

"The other form, in which the gangrene usually manifests itself, is more of a chronic nature; appearing then seldom in a recent wound. Most of the cases which I have seen of it have been in those of some standing, and in patients who had been long in hospital, many of whom had suffered attacks of that disease in the more acute form; after recovering from which, and when the wound was to all appearance doing well, the granulations healthy, secreting good pus, and sometimes even nearly cicatrized, a small, dark-coloured spot or ulceration has appeared, most commonly on the edge of the sore, varying in its dimensions from the size of a millet-seed to that of a split pea.

"This little ulceration was, in general, of a circular shape, its edges ragged, its bottom unequal and excavated, and secreting a matter of the same peculiar smell as in the sloughing form already described. Ulcerations of the same kind not unfrequently appeared in other points, which, spreading in all directions, united, and soon extended over a great part of the wounded surface. At times, this ulceration has been known to go on, and to cause very considerable destruction of parts, without the system appearing to be much affected by it; but most frequently after it had spread to a certain extent, symptoms denoting constitutional irritation became apparent; these were, nausea and loss of appetite, thirst, foul tongue, restlessness, a small and quick pulse, and heat of skin. After the febrile symptoms had appeared, the progress of the ulceration was more rapid, and very often extended beyond the limits of the original sore; the discharge became bloody, and the fetor peculiar to this affection more offensive. Sphacelus in many instances took place, and some time before death the same train of symptoms occurred, already described as taking place in the last stage of the more acute form of the disease. This is the depascent, or phagedenic, form of hospital gangrene, or what may be called Phagedæna gangrænosa.

"It would appear, that, in the milder cases of this disease, the skin and cellular membrane are the parts originally and principally concerned, and that it may sometimes be confined to these textures; but that, in more violent cases, one structure is destroyed after another, so that the muscles, ligaments, and tendons, together with the blood-vessels and nerves, are involved in the disease, become disorganized, and slough off; even the periosteum is not exempt from its influence, and the bones, being deprived of their covering, sometimes become affected with caries." (pp. 42-8.)

Dr. Boggie states that he has seen some frightful cases of this gangrene after military punishments. Surely this fact alone should be sufficient to cause the abolition of this remnant of barbarism among us. Medical men should make a decided stand on this question, and show how much greater punishment is often inflicted than the sentence awards, and how impossible it is to foretell the after consequences of the injuries caused by the lash even in what is termed *moderation.*

Dr. Boggie appears to regard the disease more in the local than the constitutional view. He speaks of it as "a peculiar inflammatory action attacking wounded surfaces," (p. 54,) varying in its character according to the "type of the accompanying form," &c., but does not discuss at any length the important question, whether the local condition of the wound is merely one of the effects of fever produced by epidemic causes, or by inhalation of the air of crowded hospitals; or, on the other hand, whether a morbid poison acting upon wounds, either by direct contact, or by the contact of putrid exhalation, affects first the wound and then the whole system, as in inoculation of smallpox. That the latter is often the case, experience proves; for the contact of a sponge imbued with fetid pus from one sore, even after careful washing, has conveyed the disease to another and previously healthy sore. Nor is it unreasonable to suppose that the same effect may be produced by special causes. Hospitals, where the disease has most prevailed, especially at Brussels, have been situated in places most exposed to malaria; and epidemic fever has, sometimes, though not invariably, prevailed among those not wounded, when hospital gangrene has been most fatal to their less fortunate companions. When a person affected with a wound or ulcer becomes the subject of typhus, every one knows how apt gangrenous action is to supervene; and, lastly, the foul air of a crowded, ill-ventilated hospital, must have a most injurious effect upon the inmates. Brugmans, of Leyden, long since showed, by an analysis of air in the wards of such hospitals, that it contains a peculiar animal matter highly disposed to putrefaction, the proportion of oxygen being diminished, of nitrogen and carbonic acid much increased, sulphuretted hydrogen being also detected. The inhalation of such a mixture must necessarily produce an unhealthy condition of the blood; and it is almost impossible that nutrition, far less any reparative process, could proceed, even although the peculiar animal matter may not be the actual morbid poison which analogy would lead us to suppose it is. Dr. Hennen showed that partial changes take place at different elevations in the same ward, by his observations that elevating the bedsteads improved the condition of their occupants. Sir D. Dickson observed the same fact in an hospital ship in Egypt. Dr. Boggie, although he does not lay much stress upon a "specific contagion" or morbid poison, as a cause of hospital gangrene, allows, that when once produced, it acquires, like the Egyptian ophthalmia, "the property of extending itself by contagion or by infection." (p. 77.) The causes first producing it in healthy wounds, he thinks, are high temperature, inattention to cleanliness, stimulating food, intemperance, and motion or mechanical irritation. We believe this to be mainly correct. From one or a combination of these causes a wound becomes gangrenous, and the putrid matter from this wound may by contact, or by charging an ill-ventilated room with noxious gases, cause a similar disease in others. Of course, when several persons are affected at the same time, the danger of extension in this manner should be necessarily increased. Our own experience is in favour of this view of the question; our observation, however, being limited to a form of the disease different from that described by Dr. Boggie, namely, the sloughing ulcer which is still prevalent at times in ships of war, and which was formerly so fatal when seamen suffered from the effects of bad and insufficient food, and were far more intemperate than at present. We have known as many as seventy men in one ship affected with the well-known circular sloughing ulcer, while no such disease existed in any other ship of the same squadron; and it was only by clearing the ship of all suspicious cases, that a check was placed upon its extension. The origin of the disease in this case was distinctly traceable. A man affected with a small indolent ulcer on the leg, went ashore in a warm climate and remained three days indulging in all the riot of a sailor's holiday. He was brought off in a state of beastly intoxication, and soon afterwards came upon the surgeon's list with his ulcer in a gangrenous condition. Perfect cleanliness and separation is almost impossible on board ship, and from this man the gangrene extended to others, and ultimately not only affected almost every wound and ulcer in the vessel, but on some of the men being sent on shore to a large well-ventilated hospital, the same disease attacked four men belonging to other ships, previously patients in the hospital, although the nurses stoutly denied any contact of person or sponges. Two of these patients had open buboes, and were both under the influence of mercury at the time; and it is singular enough that Dr. Hennen relates a similar fact with reference to the disease described by Dr. Boggie.

But we must hasten on to speak of the treatment recommended by our author. He says that the British soldiers were generally in a state of bodily and mental excitement, the fever being inflammatory, and that bloodletting is required in proportion to the violence of the inflammation, and the age and strength of the patient.

> "No precise rule can be laid down regarding the quantity to be taken; but in severe cases I have had occasion to take one, two, three, or even four pounds of blood before the inflammatory symptoms were subdued. In men, however, of a less robust constitution, who may have lingered long in hospital, or suffered much from ill health, we must act with the greatest caution. Bloodletting in such, is either altogether inadmissible, or should be used very sparingly. The same may be said of those who are the subjects of hospital gangrene in civil hospitals. They are, for the most part, poor people, worn out with age, or whose constitutions have been broken by poverty and hard labour; in such the disease assumes a different character, and will require a corresponding treatment. As an auxiliary to general bloodletting, Dr. Trotter has proposed the local detraction, by cupping and scarifying; and though I have not employed this myself, I can see no real objection to its use. The danger of the punctures becoming gangrenous, appears to me not to be

very great. The same objection has been made to general bloodletting; but though I have bled many in this disease, I never saw a single instance of gangrene supervening on the operation.

"Dr. Thomson, in his Lectures on Inflammation, p. 494, remarks, that the effects of spontaneous hemorrhages in this disease have either not been accurately observed, or at least not fully recorded, and he conceives that they might be a guide of great importance in practice. To this I beg leave to say, that, as far as my observation goes, in the early or inflammatory stage of hospital gangrene, spontaneous hemorrhage seldom occurs, and it might then, if not too profuse, be very beneficial. But in the later or more advanced stages of the disease, I have always considered it a very alarming, dangerous, and, most frequently, a fatal symptom, denoting that great destruction had already been done, and that the gangrene was still making progress. Many of our patients at Bilboa were carried off in that way. My sentiments respecting this are very different from those of Dr. Hennen. He says: 'but what more than all convinced us of a change of type, and pressed on our consideration the propriety of bloodletting, was, that spontaneous hemorrhages which formerly sunk the patient's strength, were now attended with obvious relief.' (Military Surgery, p. 223.)

"Those who trust chiefly to local remedies in this disease, and who disregard the constitutional symptoms, as being of secondary importance, and those also who consider the accompanying fever to be always of a typhoid type, object strongly to the use of the lancet, and say that they have sometimes found it do incredible mischief. To this I answer, that if bloodletting be used indiscriminately in this disease, there can be no doubt that it will often disappoint the practitioner, and even be productive of mischief. This I can myself bear witness to; for at Bilboa, after bloodletting had been acknowledged to be of the greatest benefit in those cases to which it was really applicable, I had sometimes the mortification to see that remedy brought into discredit, by being used in cases where it was evident, beforehand, that it must have been injurious; and this will always be the case, where due regard is not paid to the circumstances under which any particular remedy is prescribed.

"It has already been said, that in that form of gangrene named phagedæna, a very great destruction of parts may sometimes take place, without the constitution being much affected by it. Now, if bloodletting be used in such a case, or even in the advanced stage of the true inflammatory gangrene, when disorganization has taken place, and the system is sinking under the consequent debility, the vital powers being nearly exhausted, the result must be obvious; but when it is used with caution, and in cases where it is really applicable, it will be found to be a most valuable remedy. (pp. 90-4.)

"But although the use of wine and other stimulants, in the early stage of hospital gangrene, while there is great vascular action, as well as much local inflammation, cannot be too highly reprobated, yet there are states of this disease, in which it will be found, not only not injurious, but very beneficial; such, for example, as in the advanced stages of hospital gangrene, or in that which may occur in poor, old, infirm people, or where the patients have lingered long in hospitals, and their health has been broken by previous disease, or where the fever is evidently from the first of a typhoid character." (p. 101.)

There is sound practical sense in all this, and by no means that exclusive recommendation of venesection which the title and early portions of the work would lead one to suspect; but still we think too much stress is laid upon mere medical treatment, and results ascribed to it which were probably in a great measure owing to a change of season, with increased cleanliness and ventilation. Dr. Boggie speaks at some length upon local applications, and thinks well of the red precipitate in some cases, and of arsenical solution in others. Our own opinion is, that lint wetted with tepid water, and frequently changed, is cleaner and more agreeable than any other after the slough has separated; and before that, undiluted nitric acid is the most effectual local means of preventing extension of the gangrene. The grand object, however, is to get the patient out of the sphere of action of the causes productive of the disease: isolation and ventilation as complete as possible, perfect cleanliness, wholesome nutritious food, with a fair proportion of fresh vegetables or lemon juice, are the means most to be depended on; and if these can be fully carried out or obtained, we suspect the loss of a few ounces of blood on the one-hand, or the swallowing a little bark and wine on the other, would not make any serious difference in the ultimate result of the case.

Dr. Boggie gives tabular views of the relative mortality under different methods of treatment, but we do not extract them, as the season was gradually becoming colder as his experiments were made. To afford satisfactory evidence of this nature, we should require a series of observations upon similar cases, under identical circumstances, treated at the *same time* by different methods, not at different periods of an epidemic.

The appendix upon the antiphlogistic treatment of pneumonia and gunshot wounds, is of little value. Louis has investigated the former question, not only in a far more philosophical spirit, but with infinitely greater opportunities for observation, and his conclusions are completely opposed to those of Dr. Boggie. Only two cases of gunshot wounds are related; an account of an epidemic ophthalmia successfully treated by venesection is given; and these, with some remarks upon the "modified antiphlogistic," or mild mercurial plan of treating syphilis, make up the appendix.

On the whole, we think the essay on Hospital Gangrene a good one, and likely to be useful if proper discrimination be observed in adopting remedies to the peculiar condition of each patient, and to the character of any prevailing epidemic; but we would advise those who wish to obtain a more intimate knowledge of the disease in question, to read the very able section of Dr. Hennen, in the last edition of his 'Military Surgery.'

ART. VI.

On the System of the Great Sympathetic Nerve. By C. RADCLYFFE HALL, M.D. M.R.C.S. &c.—8vo, pp. 240. With Twelve Plates. (From the Edinburgh Medical and Surgical Journal, 1846-1848.)

NOTWITHSTANDING the rapid advance of Neurological Science in general, and our improved acquaintance with the elementary structure and anatomical connexions of the Sympathetic system, it must be confessed that our acquaintance with the functions of this part of the nervous apparatus has not made a corresponding progress. In fact, such knowledge as we have gained on the subject has hitherto been rather negative than positive; the more definite views of nervous action in general, and of the essential self-dependence of the functions of organic life, enabling us to correct and limit many of the crude notions formerly entertained as to its influence; but the real nature and extent of that influence being still undetermined. No one who possesses the least smattering of anatomical and physiological knowledge can be at any loss to understand the obstacles which environ any attempt to determine the functions of the sympathetic system, or of any subdivision of it, by the usual methods of experimental research. It is enough to advert to the wonderful complexity of its distribution, its innumerable inosculations with the cerebro-spinal nerves, and the impossibility of completely dividing its plexuses without also cutting across the trunks of the blood-vessels, in order to appreciate the most obvious anatomical difficulties; whilst the difficulty of discerning any obvious and definite results from such operations, even when we may believe them to have been effectually performed, renders it certain that the function of these nerves must consist rather in a constant and almost imperceptible influence, than in the propagation of changes at all comparable in rapidity and energy with those, to which the ordinary motor and sensory nerves are subservient.

Before we proceed to introduce our readers to the laborious researches of Dr. Radclyffe Hall, we shall make a few remarks on the present state of our knowledge of the elementary structure of the sympathetic system, which has been, during the last few years, a subject of grave discussion among the neurologists and microscopists of Germany. Since the time of Bichât, there has been, until lately, a general concurrence of belief in the independent character of the sympathetic system; its nerve-trunks being supposed to bear the same relation to its ganglionic centres, as those of the cerebro-spinal nervous system do to the brain and spinal cord. This doctrine appeared to derive additional support from the microscopic researches of Remak, who was the first to point out the existence, in the sympathetic nerves, of a set of fibres, different both in size, colour, and structure from the ordinary cerebro-spinal nerve-tubes, and now known as the "gelatinous" or "grey" nerve-fibres. These fibres were asserted by him to issue from the ganglionic corpuscles of the sympathetic ganglia; and he maintained that *they* were the proper organic fibres, whilst the ordinary white tubular fibres contained in the sympathetic system are derived by anastomosis from the cerebro-spinal nervous centres. On the other hand, the grey fibres, which are found sparingly in most of the cerebro-spinal nerve-trunks, and which are particularly numerous in the fifth pair of cephalic nerves, were regarded by him in the light of an extension of the sympathetic system into the cerebro-spinal; being partly derived by inosculation from the true sympathetic ganglia, and partly connected with the ganglia on the sensory roots of the spinal nerves, which he ranked in the same category with them.

This very ingenious view was strenuously attacked by Professor Valentin and others; who maintained that the so-called "grey" fibres are not nerve-fibres at all, but filaments of areolar tissue; and it was even asserted by Valentin, that the sympathetic system of nerves has no distinct and independent existence, its fibres being entirely derived from the cerebro-spinal, and only functionally modified by passing though the sympathetic ganglia. His experimental researches confirmed this view to a certain extent; for, whilst he proved that various non-striated muscles,—as the muscular coat of the alimentary canal, the bladder, and uterus, the muscular walls of the larger arteries, the muscular coat of the gall-bladder and bile-ducts, the muscular coat of the ureter, &c.—may be excited to contraction by irritating the sympathetic nerves which supply them, he also showed that the same effect might be produced by irritating the roots of the cerebro-spinal nerves above the origin of the fibres which pass into the sympathetic ganglia, whose branches proceed to the parts in question.

By Volkmann and Bidder, again, the nervous character was denied to the "grey" fibres of Remak; but, so far from agreeing with Valentin as to the absence of any distinct sympathetic nerve-fibres, they attempted to prove that the sympathetic system essentially consists of a variety of nerve-tubes of smaller diameter than those proceeding from the cerebro-spinal centres, and less distinctly presenting the characteristic double contour of the latter. These "fine" fibres are especially abundant in the trunks and branches of the sympathetic system; but are very numerous in many of the cerebro-spinal nerves also.

Of late, however, the tide of opinion has flowed more in the original direction. It has come to be generally acknowledged that the "grey" or "gelatinous" fibres are really nervous in their character; bearing somewhat the same relation to the white tubular fibres, that the non-striated bears to the striated muscular fibre. And the "fine" fibres of Volkmann and Bidder are now generally admitted to be nothing else than white or tubular fibres of less than the average diameter; their continuity with undoubted cerebro-spinal fibres being frequently apparent. The relation of the gelatinous and of the tubular fibres to the ganglionic cells has now been demonstrated by the researches of Hannover, Kölliker, and others, to be precisely the same; and the differences in their respective characters are found not to be inconsistent with a connexion between them by means of transitional forms. Indeed, it would appear that an undoubted tubular fibre may present the "gelatinous" aspect in some parts of its course. "Thus," says Dr. Sharpey,* it is no uncommon thing for a tubular fibre of the most typical form, in approaching its termination, to decrease in size, lose its double contour, and present the faint outline and finely-granular aspect of a gelatinous fibre: we have seen that this change always occurs when a fibre enters a Pacinian body; and in the tadpole's tail, nervous tubules are continued into fibres, which are marked with nuclei, and wholly agree in appearance with the gelatinous fibres." It has been pointed out by Messrs. Todd and Bowman,† that the branches of the olfactory in the nose are almost wholly made up of fibres, bearing nuclei, and having all the outward characters of the gelatinous fibres.—These last facts, whilst they establish the title of the gelatinous fibres to the neural character, also prove that we cannot, with Remak, regard them as exclusively belonging to the sympathetic system. There is much reason for regarding the gelatinous nucleated fibre as an earlier stage of development of the tubular; and we may perhaps consider the "fine" tubular fibres as intermediate between these and the larger tubules. Certain it is, however, that the grey fibres and the fine tubules are most abundant in the sympathetic system, as well as in those cerebro-spinal nerves which are most intimately connected with it; and that they may be distinctly traced into connexion with the ganglionic cells of the sympathetic ganglia: so that, although we may not regard every "grey" fibre or "fine" tubule which we meet with in cerebro-spinal nerves, as appertaining to the sympathetic system, there can be no reasonable doubt that there *is* a set of true sympathetic or organic fibres, and that these are partly distributed through the cerebro-spinal nerve-trunks, whilst cerebro-spinal fibres pass into the sympathetic system, and are distributed, with its branches, to the muscular structures which they supply, and to the parts which become sentient in disease, if they are not so in health.

---

* Introduction to Quain's Anatomy, p. ccxxviii.

† Physiological Anatomy, vol. ii, p. 9.

The aim with which Dr. R. Hall sets out, is to ascertain how much precise and definite information respecting the function of the sympathetic system can be obtained from minute anatomical investigation, and from experiment; and for the following reasons he selects the ophthalmic ganglion as the one best suited for his purpose:

> "The lenticular ganglion represents the ganglionic system in miniature. What can be proved with respect to it, may justly be taken as a safe index to the rest. From being comparatively accessible, and from its connexions being limited and well defined, this ganglion is the most eligible for the purposes of experiment. And from the effects produced on the eye being in great measure open to inspection, the results obtained are as little conjectural as the nature of such inquiries will admit of." (p. 1.)

In order to discover a reason for the discrepancy between the results of experiments on the ciliary nerves in different animals, Dr. R. Hall first inquires into the anatomical connexions of the ganglion. He finds that, contrary to the statement of some anatomists, the lenticular ganglion does exist in rabbits, guinea-pigs, &c.; and, though it is exceedingly small, possesses its usual connexions with the third and fifth nerves and with the iris; but, in the rabbit, the ciliary plexus receives two filaments from the sixth nerve, which do *not* pass through the ganglion;—a fact of considerable importance. The following is his general summary as to the anatomical relations of the ophthalmic ganglion in different animals:

> "1. That its size is always in direct proportion to the activity of the iris, which in turn always bears a direct relation to the strength and acuteness of vision, and to the nocturnal habits of the animal, and implies a proportionate development of the internal vascular apparatus of the eye.
>
> "2. That it is always more intimately connected with the third nerve than with any other, and always with that division of the third nerve which supplies the inferior oblique muscle.
>
> "3. That the size of the short root is in direct ratio to that of the lenticular ganglion (the long sensory root observing no such relation); the ganglion being sometimes a mere swelling on the trunk of the inferior division of the third nerve itself, which can then be scarcely said to possess any short root.
>
> "4. That it is always connected with the sympathetic, but that the nerves, along which these communicating fibres pass, vary.
>
> "5. That it is always connected with the Gasserian ganglion, receiving filaments from the sensory, never from the motor division of the fifth nerve; these filaments being few in number, and never terminating in the lenticular ganglion, which they enter, either separately and distinct from the third nerve, as in man, or bound up within the same sheath with the third nerve, as in several animals.
>
> "6. That there always exist other ciliary nerves from the fifth, which have no connexion with the lenticular ganglion, although they ordinarily apply themselves to nerves which proceed from that ganglion." (p. 6.)

A number of experiments upon the ophthalmic ganglion and upon the nerves connected with it, performed on different animals, are then detailed; and the following is the author's general summary of results, which we present in full, because they are to serve as the foundation of his subsequent inferences as to the physiology of the sympathetic system in general:

"1. The third nerve is the only direct motor nerve for contraction of the pupil in dogs and cats.

"2. The action of the third nerve, as far as concerns the iris, is mainly under the control of the visual nervous tract.

"3. As both the third nerve and some portion of the visual tract must inevitably be injured in performing the experiment of dividing the fifth nerve after Magendie's plan, the experiments of that gentleman cannot be considered as evidence that the fifth nerve is, either directly or indirectly, the nerve which presides over dilatation of the pupil in dogs and cats; nor over contraction of the pupil in rabbits and guinea-pigs.

"4. In animals, in which division of the fifth nerve causes contraction of the pupil, it does so by excito-motory action through the sixth nerve, which, in these animals, supplies the iris, in conjunction with the third. The sixth nerve, however, not entering into the formation of the ophthalmic ganglion, which in these animals is extremely small.

"5. In these rodentia, pain of any kind, whether produced by irritation of the fifth, or of any other sensational nerve, will cause more or less contraction of the pupil at the instant, attributable, probably, to excito-motory action of the iridal fibres of the sixth nerve.

"6. Irritation of the fifth nerve, or of any other sensational nerve in the cat, and dog, and pigeon, so long as it does not affect the brain to the extent of producing vertigo, *nor the visual sense in any other way*, has no immediate influence over the size of the pupil. Hence, although the fifth is an excitor nerve to the *sphincter iridis* in the rabbit, it is neither directly an excitor nor a motor nerve to it in the dog and cat.

"7. As the third nerve can always, under favorable circumstances, be made to influence the movement of the iris, *immediately* on the application of irritation, and as the iridal portion of the third nerve in all animals, in which the iris is active, passes through the ophthalmic ganglion, it follows that *the ophthalmic ganglion offers no check to the transmission of motor influence along the motor nerve-fibres which pass through its substance.*

"8. As irritation of the fifth nerve does not in any animal affect the action of the iris *after* division of the cerebral connexions of all the other ocular nerves, and as some portion of the fifth nerve always enters into the ophthalmic ganglion, it follows, *that the filaments of the fifth nerve have not the power of affecting those of the third nerve during their course together through the substance of the ophthalmic ganglion*; or, in other words, *that the lenticular ganglion is not a centre of excito-motory action to the iris.*" (pp. 24-5.)

Dr. R. Hall then proceeds to inquire whether there is any motor nerve for causing *dilatation* of the pupil; and after a careful consideration of the results of experiment and pathological observation, he comes to the conclusion that there is no special nerve, either encephalic or spinal, for this purpose,—the dilatation of the pupil being the result of suspension of the action of the contracting nerve, and the radiating fibres of the iris not (in his opinion) possessing a muscular character. We cannot regard this question as settled by his researches, opposed as his conclusions are to those of many distinguished neurologists who have specially investigated the question; but the following argument, adduced by our author, must be admitted to have considerable weight. The uniform result of irritation of the nerves proceeding from the ciliary ganglion is the production of *contraction* of the pupil. Now, if dilating nerve-fibres are bound up in these filaments, why are *they* not excited to action at the same time, so as to antagonise the influence of the contracting fibres? It cannot be shown that they reach the iris by any other channel; for these supposed dilating filaments, proceeding from the cervical ganglion (and presumed by Valentin to be derived from the superior spinal nerves), can only enter the orbit through the communicating branch of the sympathetic, which passes into the ophthalmic ganglion.

Having established to his own satisfaction that the fifth nerve is neither a motor, nor (as Valentin and others have supposed) an excitor nerve to the iris, Dr. R. Hall proceeds to discuss the rationale of the alteration in the movements of the iris, which, as pathological observation abundantly proves, is frequently dependent on injury or disease of that nerve; and to inquire into the purpose served by the transmission of the third and fifth nerves, not directly from their cerebro-spinal centre to the part supplied, but through the intermediation of the lenticular ganglion, where they come into relation with the nerve-vesicles and fibres of the sympathetic. Several *possible* hypotheses are discussed by him; and the long theoretical discussion, which we are not sure that we correctly understand, terminates in the following not very definite or satisfactory conclusions, the former of which might, we think, have been anticipated from the first, whilst the latter seems to us entirely hypothetical:

> "1. That when encephalic, spinal, or cerebro-spinal nerves pass through the substance of a ganglion, it is to associate the influence of the organic nerves of that ganglion with their own.—2. But that, on the other hand, during ordinary excitement, the ganglionic nerve-vesicles have not the power of exciting to action the nerve-tubes which lie among them, in their passage through the ganglion." (p. 39.)

The author next examines various hypotheses respecting the functions of the ganglia,—as, that they impede or cut off sensation,—that they prevent the transmission of the influence of the will,—that they are essential to sensation,—and that they are the motor centres of the involuntary muscles. These in turn he sets aside, and, as we think, on sufficient grounds. Having thus cleared the way by determining what the functions of the sympathetic nerve *are not*, he next attempts to determine what they are; and commences by showing that the commonly received opinion as to its connexion with the *organic* processes is in accordance with all the evidence we can derive from anatomical sources. He excepts, however, the nutritive operations of the nervous and muscular tissues themselves,—though upon what grounds he does so, we must confess ourselves unable to discover. To us it appears as if the regulating influence of the organic nerves would be peculiarly needed for the preservation of the due relation between the nutrition of these tissues, and the waste or disintegration resulting from their functional activity; and the fact that many muscles in most constant use contain a large number of sympathetic ganglia imbedded in their substance, whilst in others (as the tongue) fibres derived from the sympathetic ganglia are copiously intermingled with those of the cerebro-spinal nerves, and are distributed with them, would seem to support this view. We have already seen that Dr. Hall regards the influence of the sympathetic centres and nerves on the molecular changes, as dependent in the first instance on the stimulation which they receive by the passage of impressional (or excitor) and motor fibres through their ganglia. That such constitutes a part of their action, we think that there is strong reason to believe. Thus, anything which excites the functional activity of a sensory surface, occasions an increased determination of blood to the part, and an augmented energy in the molecular changes which go on in it: so that, if the surface be that of a mucous membrane, we have increased secretion; whilst, if it be one more especially adapted to receive sensations, we have an increase in its sensitiveness. The mere direction of the attention to a part seems to increase the flow of blood towards it, as Dr. Holland has well pointed out, and augments or otherwise modifies its functional activity. In these cases, as well as in that of the influence of the emotions on the organic functions, we think that Dr. R. Hall's doctrine of the "synergetic" action of the sympathetic nerve-cells and fibres, excited by the passage of the cerebro-spinal nerve-tubes through the ganglia, is a very probable one. But we cannot see what ground he has for thus rendering the action of the sympathetic system altogether dependent upon that of the cerebro-spinal; to us it appears that there is fully as much ground for considering that the several ganglionic centres of the former may mutually stimulate each other by their communicating fibres, as there is ample reason for believing those of the cerebro-spinal system to do; and that the sympathetic system is thus concerned in that wonderful co-ordination of the organic functions, so remarkable in those higher organisms in which this system is most distinctly and elaborately developed.

Although Dr. R. Hall is strongly impressed with the belief that the action of the true sympathetic fibres is purely *organic*, we are not to infer that he upholds the unphilosophical doctrine of the absolute dependence of the organic functions upon its agency. On the contrary, he upholds the possession, by every living molecule, of endowments peculiarly its own; and puts forth the following as his creed on the subject of the influence of the sympathetic nerves upon the nutritive functions:

> "As an hypothesis, therefore, capable of explaining all phenomena referable to the nervous system, so far as physical action is concerned, and apparently open to fewer objections than any other, we infer that the nervous force can stimulate every living molecule endowed with a certain function, to the more active discharge of that function; that it can consequently excite, variously modify, or greatly derange, all vital actions; that muscular contraction, cell-metamorphosis, the evolution of animal heat, are influenced by the nerves in precisely the same manner, the difference in result being due to inherent differences of vital endowment in the molecule subjected to the nervous influence; that the varied fulness, and therefore the calibre of the capillary blood-vessels is secondary to, and dependent upon, the activity of the molecular changes going on in their contents, and under the control of the nerves only in so far as these affect the vital actions of the blood." (p. 53.)

These being our own views on the subject, we need not stop to discuss them.

The inferences drawn by the author from his study of the ophthalmic ganglion, and applied by him to the sympathetic system in general, are afterwards tested by the examination of the anatomical and physiological relations of the spinal or intervertebral ganglia, and of the several ganglia connected with the fifth pair. He considers (with Remak) that these ganglia are centres of action to the portion of the sympathetic system blended with the cerebro-spinal; and that, as they are in relation with afferent or impressional fibres alone, their function is to keep the nutrient functions of the various parts from which these nerves arise, in harmony with their functional activity. He seems to think that the skin and other sentient surfaces are the parts chiefly influenced in this mode; but we would remind him that impressional fibres proceed to the spinal cord from the muscles also, completing the "nervous circle" requisite for the regulation of voluntary motion by the "muscular sense;" and we cannot see why these should not be regarded as influencing the nutrition of the muscles through the organic fibres, in the same manner as the impressional fibres from the skin are supposed to affect the nutrition of that texture.

From the final summary of the inquiry into the relations of the fifth pair and its ganglia, we shall extract the conclusions which seem to us of most interest.

"1. The organic actions of the parts supplied by the trigeminus are invariably associated with the impressibility of the same parts. Whether or not the subsidiary ganglia coexist, the Gasserian ganglion is invariably present.

"2. The Gasserian ganglion is quite competent to, and sufficient for, the superintendence of the organic actions going on in the organs of the special senses, as well as in all other parts supplied by the fifth nerve, so long as these several parts are not directly connected and associated with the action of special muscles. *e. g.*, The fish, possessing an eye, ear, and nostril, perfect of their kind, has none of the sympathetic ganglia belonging to the fifth. The eye is without its muscular iris, the ear without its tympanic muscle, the nostril without its moveable palate.

"3. Wherever we do find a distinct and essential connexion and association between the organic actions of a part and the contraction of some special muscle or muscles, there we also find a distinct sympathetic ganglion superadded to that of the fifth. *e. g.*, The ophthalmic ganglion exists only when the organic actions of the eye need to be associated with the contraction of a *sphincter iridis;* the otic ganglion, when the organic actions of the middle ear with contraction of the *tensor tympani* and of other muscles; the acoustic ganglion, when the organic actions of the internal ear with contraction of the *stapedius;* the spheno-palatine ganglion, when the organic actions of the *nares* and *velum*, with movement of the soft palate; the salivary ganglia, when the organic activity of the glands requires association with those muscles of the oral cavity which are supplied by the facial nerve.

"4. But the converse does not hold good. The ganglion is not essential to the muscle. A muscle, for which a particular sympathetic ganglion is usually present, may yet exist without the ganglion. *e. g.*, In birds and reptiles, an internal tympanic muscle exists without any otic ganglion. In them, the organic actions of the tympanum are not associated with the contraction of the muscle. In all those animals which possess an otic ganglion, they are. Hence the sympathetic ganglion is needed, not for the muscular contraction alone, nor for the organic action alone, but for combining the two." (Part iii, pp. 75-6.)

We have thus attempted to lay before our readers an outline of Dr. R. Hall's researches, so far as relates to their main purpose,—the determination of the functions of the sympathetic system. That the results are not more definite and satisfactory, must, we think, be regarded rather as a consequence of the difficulties of the subject, than as proceeding from any want of patience or determination on the part of our author. It may be that we have failed to bring into sufficient relief the points on which he lays the greatest stress: if we have done so, we must excuse ourselves on the ground of a certain prolixity and wordiness which too frequently obscures his meaning; and also from the want of due connexion, which results from the publication of his Researches in the form of detached memoirs, the sequence of whose contents, though very probably clear to his own mind, is not readily obvious to the reader. Numerous inquiries of much interest proceed as branch lines from the main road;

and we must not pass over some of these without notice. Thus we find in the Third Memoir, one of the earliest confirmations of the statement of Messrs. Todd and Bowman as to the continuity of the caudate prolongations of the nerve-cells with the commencement of the fibres, or with those of other nerve-cells. He states the opinion also, as the result of very careful examination, that the original statement of Ehrenberg, as to the *varicose* condition of some of the nerve-tubes in the nervous centres, is more correct than the assertion of other microscopists, that this varicose character is the result of pressure, manipulation, *post-mortem* change, &c. In one of his plates, he figures a partial decussation of the sensory tract in the medulla oblongata, which closely corresponds with that described and figured by Mr. Solly in the second edition of his work on the 'Brain.' A new ganglion has been discovered by Dr. R. Hall, at the point where the auricular branch of the *vagus* joins itself to the *portio dura* within the fallopian canal; this small swelling, which contains grey nerve-cells interposed among the white tubuli, does not involve all or even the greater part of the facial nerve, but that portion only with which the fibrils from the vagus intermix, and from which the *corda tympani* takes its rise. The function of this ganglion, as deduced from a consideration of its connexions, is presumed by Dr. Hall to be to associate the secretion of the parotid gland with impressions made on the tongue, with the state of the stomach, and with that of the system at large; and also to connect together activity of secretion and motion of the excretory ducts.

We might easily extend this notice by quoting some of our author's ingenious explanations of the peculiarities in the anatomical connexions of the various ganglia connected with the fifth pair; but we must content ourselves with simply referring to them as all bearing out his general position, that these ganglia form a part of the sympathetic system, and that their offices are, to bring the organic or molecular changes, in the organs with which they are severally connected, into harmony with those changes which constitute their respective functional manifestations. They give, too, a plausible explanation of the intricacy of the nerve-connexions about the base of the skull; and afford a reason for the plurality of ganglia on many nerves, not explained before. This, of course, does not prove the correctness of his theory; but it warrants the adoption of it, until a better is offered,—at any rate as a clue, which may reasonably give us some confidence in the existence of a definite purpose in the complex arrangements in question, and which may conduct us in time to the comprehension of it.

We anticipate much from Dr. R. Hall's future labours, should they be directed towards some more attainable object; for he has shown an aptitude both for experimental research and for scientific reasoning, which would have been much more obvious if the obscurities of his investigation had not aided the verboseness which is the fault of his compositions, in concealing it. We would counsel him, on the next occasion on which he comes before the public, to study conciseness as much as possible; and to digest his whole subject thoroughly before putting forth any portion of it. By so doing, we are satisfied that he will gain the attention of many readers, who at present leave his scientific contributions unnoticed.

Art. VII.

1. *Lectures on Electricity and Galvanism in their Physiological and Therapeutical Relations, delivered at the Royal College of Physicians, in March,* 1847. By Golding Bird, a.m., m.d., f.r.s., Fellow of the College, Assistant-Physician to, and Lecturer on Materia Medica at, Guy's Hospital. (From the London Medical Gazette.)—*London,* 1847. 8vo, pp. 43.
2. *On the Efficacy of Electricity, Galvanism, Electro-Magnetism, and Magneto-Electricity, in the Cure of Disease, and on the best Methods of Application.* By M. Donovan, formerly Professor of Chemistry to Apothecaries' Hall. (In the Dublin Quarterly Journal of Medical Science, Nos. 4, 5. New Series.)—*Dublin,* 1846-7.
3. *De l'Influence de l'Electricité Atmospherique et Terrestre sur l'Organisme, et de l'Effet de l'Isolement électrique considéré comme moyen curatif et preservatif d'un grand nombre de Maladies.* Par Emm. Pallas, Officier de l'Ordre Royal de la Légion-d'Honneur, Docteur en Médecine de la Faculté de Paris, &c.—*Paris.* 8vo, pp. 356.

On the Influence of Atmospheric and Terrestrial Electricity on the Organism, and on the Effect of Electric Isolation considered in its curative and preservative Application to a great number of Diseases. By Emm. Pallas, d.m.—*Paris.*

4. *Elements de Physique Terrestre et de Météorologie.* Par M. Becquerel, de l'Académie des Sciences de l'Institut de France et Professeur Administrateur de Muséum d'Histoire Naturelle, &c.; et par M. Ed. Becquerel, Aide Naturaliste au Muséum d'Histoire Naturelle, &c.—*Paris,* 1847. 8vo, pp. 706.

Elements of Terrestrial Physics and Meteorology. By M. Becquerel and M. Ed. Becquerel.—*Paris,* 1847.

In the present article, we shall strictly confine ourselves to the therapeutic value of Electricity in its various forms. Those who are conversant with the literature of the latter half of the eighteenth century, must be aware of the fact, that, during that period, a large number of works appeared on this subject; nor can they be ignorant of the gross falsehoods, mingled with grains of truth, that pervaded the great majority of them. It is doubtless in a great measure owing to the misrepresentations of the early writers on the therapeutic value of electricity, that regularly-educated physicians were disinclined, until quite a recent period, to give it a fair trial. There are, however, other reasons, which have contributed to the same end. Mr. Donovan, in the able essay whose title is prefixed to this article, conceives that one of the causes which led to the disuse of ordinary electricity (and it is to this form that we shall first devote our attention) was the inefficiency of the means of employing it. A notion prevailed, he observes, that, as a small electrical machine is capable of charging a Leyden phial to such an intensity as to give a powerful shock, there is no use in using larger, especially as those of great power were very costly, and are of course liable to accidents. Those who are conversant with the powers of electricity, and the various methods of applying it, are aware that there are many cases in which the spark and its modifications can be employed with better success than the shock, and that the former are only useful when communicated by a machine of very high power. Another point, that has contributed to check the general employment of large machines, is the difficulty of managing them. Every one is aware that the ordinary electric machine (whether the plate or cylindrical form) is so affected by a damp or cloudy atmosphere, as, unless in very expert hands, to be rendered almost wholly useless.

The following remarks on the nature and mode of managing the electrical machine, will prove useful to those who have had but little experience in its use. They are written by one, who has happily shown that a profound acquaintance with physical science is far from incompatible with the successful prosecution of the science and practice of medicine:

> "The electrical machine consists of a revolving cylinder, or plate of glass, submitted to the friction of cushions, or rubbers. It matters very little what form of machine is employed. As a general rule, a plate machine is, for equal size, of far higher power than the cylinder. The arrangements of the latter are, however, simple, and are, perhaps, more easily managed by the uninitiated. There is also an advantage on the score of economy, as old cylindric machines are readily to be procured at low prices; and, as a general rule, a well-worn cylinder is far preferable to a new one. Plate machines are, on the contrary, less common, and consequently must generally be purchased new. Whichever form is employed, it is useless using a plate with a less diameter than a foot, or a cylinder less than five or six inches.
>
> "There is some little tact required to elicit the full power of an electric machine, and, from want of this, you will frequently find some persons quite fail in exciting any amount of electricity, even from the best constructed machines. This art is, however, soon acquired. When the machine is required for use, the prime conductor and rubbers should first be removed, and the machine placed sufficiently near a good fire to become completely dry and warm. The surface of the glass should then be slightly rubbed with a piece of tow or flannel soaked in olive-oil, any adhering black spots from old amalgam being scraped off. By means of a dry and warm linen cloth, the oil should then be wiped away, and the polished surface of the glass is thus left clean and free from moisture. The cushions, if covered with amalgam, are then to be rubbed with a piece of brown paper, so as gently to remove the oxydised surface; but, if not sufficiently covered, a little amalgam (made by melting together zinc two parts, tin one part, with mercury six parts, made into a paste by triturating it in a mortar with a little lard) must be rubbed into the surface of the cushions with the handle of a knife, or piece of smooth wood. The silk flaps are to be wiped clean, and the rubbers adjusted to the plate or cylinder. On revolving the latter, a rustling noise will be heard, accompanied, in a darkened room, by vivid flashes of blue light, whilst a strong, phosphorus-like odour of *ozone* becomes perceptible. The prime conductor is next to be replaced, taking care that its insulating support is perfectly dry, and even slightly warm: the instrument is then fit for use. You will, however, not unfrequently find, that, although you may have taken the precaution to connect the rubber with the table or floor by means of a metallic conductor, still that little or no electricity is obtained on revolving the glass. This will generally be found to depend upon the badly conducting table, or floor, by which a sufficiently ready means is not afforded for the complete restoration of the electric equilibrium of the rubber, when destroyed by the friction of the revolving cylinder or plate against its surface. This difficulty is best overcome, in London and large towns, by connecting the rubber, by means of a long copper wire, with a branch of the leaden pipes, through which the house is supplied with water. By this plan a ready communication is afforded by a good conductor with the great reservoir of electricity—the earth." (Dr. Golding Bird's Lectures, p. 28.)

In addition to the machine itself, a pair of directors, or brass rods, with a ball at one extremity, and a glass handle at the other, together with a few yards of slender brass chain, are required to connect the patient with the machine, or to convey a charge through a given part of the body: and an insulating chair or stool is also indispensable. The practitioner may easily construct an insulating chair for himself, by simply placing the legs of an ordinary chair in thick glass cups. Dr. Golding Bird recommends thick, round saltcellars, in the rough state in which they are sent from the glasshouse. In this manner, at the expense of a couple of shillings, we may readily obtain an excellent insulating support.

There are, however, other modes of applying medical electricity; namely, the electro-magnetic apparatus, and ordinary galvanism. While, by the common electrical machine, we obtain small quantities of electricity in a state of high tension, by the electro-magnetic apparatus we obtain large quantities of a much lower tension, but yet far higher than when evolved from the galvanic trough.

Galvanic electricity is now seldom employed; and, indeed, may be said to be almost entirely superseded by the electro-magnetic current, one of the brilliant discoveries of our illustrious countryman, Faraday. The great drawbacks to its utility are, the bulky and unmanageable form of apparatus required for its excitation in a state of even moderate tension, and the irregularity of the current in regard to strength (the tension and quantity rapidly sinking from the period at which the acid is added); and to these we may add, the damage that may be caused by the ejection of the acid from the trough from too violent action.

Dr. Golding Bird explains, with much clearness, the great principle of electro-magnetism, and describes two or three of the best forms of electro-magnetic machines. From much experience in its use, and from a very distinct and unpleasant recollection of the facility with which other machines, even in the hands of those who know how to use them, get out of order, we cordially concur with him in recommending the instrument of Mr. Neeves, of Broad Street, Holborn. It is an automatic apparatus, which alone is no slight advantage: it is simple in its construction, easily applied, and the quantity and intensity of the inductive electricity are regulated with great facility. Moreover, its price is only thirty shillings; while those of a more intricate construction, as those of Clark and Noad, vary from six to ten guineas. The only objection to Neeves's instrument is, that we cannot obtain by its aid a series of positive and negative currents in a definite direction, each of the conducting wires conveying alternately negative and positive currents. On this account, as Dr. Bird observes, however useful this apparatus is when we want a mere stimulant action, the simple shock of the electric agent, yet it is likely to fail in many cases of paralysis, in consequence of our not being able to transmit by its aid the positive current in the direction of the nervous ramifications.

Dr. Bird concludes his remarks on this part of the subject with a description of a more complicated electro-magnetic machine, by which the current may be transmitted in a definite direction; the currents being separated in the same way as we get them in the galvanic battery. His description would, however, be unintelligible without a figure.

Having explained, sufficiently for our present purpose, the nature of the requisite apparatus and the method of using it, we shall now give a brief sketch of the history of ordinary electricity, regarded as a therapeutic agent: and for the greater part of our information on this subject, we must acknowledge our obligations to Mr. Donovan. From the investigations of that gentleman, it appears, that "the first attempt at rendering electricity available in the alleviation of human suffering, was made by Kratzentein, in 1744, who tried it in several diseases." He was speedily followed by numerous writers, who, like certain of the medical electricians of the present day, whose successes are blazoned forth in the most startling advertisements, mingled a little truth with much that was marvellous and altogether imaginary. Foremost amongst them we may place Signor Pivati, of Venice, who used to electrify his patients by cylinders filled with balsams and various other medicines. The following cases, honoured by a place in the 'Transactions of the Royal Society,' afford sufficient evidence of the degree of credit his evidence deserves, and of the facility with which, in former and less enlightened times, even the Royal Society might be led to countenance absurdities.

He had the honour and pleasure of curing Donadoni, Bishop of Sebenia, aged seventy-five years, of gout. The case is graphically described:

> "The bishop came with his physicians and friends; the disease had stiffened his fingers and knees, so that he could not bend them, and he was obliged to be lifted into bed every night. Pivati filled his cylinder with discutients, and with it electrified his reverend patient. In a few minutes the bishop felt an unusual commotion in his fingers; he was relieved; he opened and shut both his hands, and gave a hearty squeeze to one of his servants; he rose up, walked, smote his hands together, helped himself to a chair, sat down, walked down stairs with the agility of a young man, and was so amazed that he thought it must be a dream."

The results of electrifying with a cylinder containing Peruvian balsam, were very singular, but, like the results of certain experiments on chloroform, recently instituted on a large scale at evening parties in the northern metropolis, were not altogether unobjectionable; for we read, that a person electrified in this manner, became in half an hour more lively and cheerful than usual; but the company perceived a smell, for which neither he nor they could account, until at length he discovered that it proceeded from himself.—The following case, recorded about the same period by Dr. Giuseppe Bruni, and quoted by Mr. Donovan, may perhaps tend to elucidate the phenomenon to which we have referred: "A physician at Rome lined a cylinder with purgative matter, and electrified a gentleman with it, who immediately found on the spot the same effects as if he had swallowed the cathartic."

Pivati's reported cases naturally attracted much attention, and induced the Abbé Nollet to take a journey to Italy, with the idea of investigating the truth of the marvellous statements that had been published. The Abbé appears to have been one of those hard-headed men, who are resolved to sift everything thoroughly and take nothing on credit. The result of his visit was the discovery of the fact, that the poor bishop was as bad as ever, and that the cylinders which had produced such singular effects were broken, and had disappeared to the last fragment.

These illustrations are, we doubt not, sufficient to show, that the early advocates of electricity as a therapeutic agent, adopted so extravagant a course, as to make all sober-minded men distrust their statements.

The early history of medical electricity in England is, for the most part, to be found in scattered papers in the 'Philosophical Transactions.' If we have to regret the occasional scantiness of the details, we may at least congratulate ourselves, that our countrymen were free from the mendacious propensities of the Italian and German writers. On the whole, the earlier trials of medical electricity were far from being very successful, probably from the power in many cases being too feeble or improperly applied. Dr. Cheny Hart tried electricity in certain cases of paralysis; in one he was singularly unfortunate, for, "by giving shocks to a girl who had a paralytic arm, he rendered her universally paralytic, and when her disease was brought back by medicine to its original state, she again relapsed into universal paralysis when electricity was resumed." In the cases of paralysis which Dr. Franklin treated with electricity, there was an amendment for the first three or four days; but, after the fifth day, the patients became discouraged at observing no further apparent benefit, and refused to receive any more shocks. In these cases the power was too strong. This is obvious from his own account of the jars, and from the sensation described by the patients.

About this period the success of Mr. Patrick Brydone, in some severe cases of paralysis, attracted much attention.*

A year or two afterwards, we find a report of a case of tetanus, arising from worms, cured by Dr. Watson, by means of electricity; and this was speedily followed by another case of the same disease, cured in a similar manner by Dr. Spry.

The 'Philosophical Transactions' for the year 1778, contain an account of the following singular case of extreme muscular contraction, cured by electricity. The head of the patient was drawn over her right shoulder; the back part of it was twisted so far round, that her face turned obliquely towards the opposite side, by which deformity she was disabled from seeing her feet, or the steps as she came down stairs. The sterno-mastoid was in a state of contraction and rigidity. She suffered under many symptoms dependent on the same cause, and at times was slightly paralytic. This state, which she had endured for two years, was caused by going from a warm room into cold air; and it came on with a pain in the back of the head, which gradually contracted the muscles to a melancholy deformity. Medicine having proved utterly useless, Mr. Partington determined on trying electricity; and drew strong sparks from the parts affected, for about four minutes, which brought on very profuse perspiration, and this seemed to relax the mastoideus muscle to a considerable degree. This method producing some disagreeable symptoms, he communicated strong sparks to the muscle, by which the improvement proceeded progressively, without certain feverish symptoms which the former mode had occasioned. In a little more than a month, by daily electrization, her cure was accomplished. Latterly, both shocks and sparks had been used.

If we add a reference to Dr. Fothergill's cases of St. Vitus's dance, cured by electricity (1779), we shall have completed our sketch of the history of medical electricity, in so far as it is developed in the 'Philosophical Transactions.' We might follow out the same course with the various detached memoirs in the 'London Medical Journal,' which extended from 1781 to

---

* Philosophical Transactions, vol. L, pp. 392 and 695.

1790, if it were necessary. But we have already brought forward sufficient evidence to show, that, before the beginning of the present century, our knowledge of the therapeutic value of electricity was very vague, and that we had no correct ideas of the method of applying it.

We now proceed to notice the individual forms of disease, in which it is established, on more precise evidence, that electricity may be applied with advantage. We shall begin with—

Chorea. De Haen, in 1761, gives several cases of girls, who were subject to this affection, and who were all perfectly cured by being electrified; and we have already alluded to Fothergill's cases. From these times to the present day, this disease has been occasionally treated in this manner; but the evidence of its value is more fully and satisfactorily attested by the investigations of Dr. Golding Bird, than by those of any other physician. We extract the following table from his fourth lecture; and for further information must refer our readers to his "Report on the value of Electricity as a Remedial Agent in the Treatment of Diseases," in No. XII of the 'Guy's Hospital Reports.'

"Notes of 37 cases of chorea have been preserved: of these

37 cases, { 17 males, } 9 above 16 years.
{ 20 females, } 26 under 16 „

"Of these, in 25 the movements were universal
„ 5 limited to the right side
„ 1 „ left side
„ 2 „ both arms
„ 1 „ right arm
„ 1 „ left arm
„ 1 „ sterno-cleido muscles
„ 1 „ pterygoid muscles.

"The causes of the chorea in 17 were traced to terror
„ 3 „ amenorrhœa
„ 3 „ intestinal irritation
„ 2 „ intense cold
„ 1 „ rheumatic fever
„ 1 „ intense grief
„ 1 „ congenital
„ 1 „ mechanical injury
„ 4 „ no apparent cause
„ 4 „ complicated with epilepsy.

"Of these, 30 were completely cured
„ 5 were relieved
„ 1 refused to continue treatment
„ 1 uncured. This was a man, 61 years old, where there was a suspicion of spinal mischief." (Dr. Golding Bird's Lectures, p. 36.)

The following remarks on the mode of applying electricity are worthy of attention. As a general rule, sparks were drawn daily, or every second day, from the spine; and when chorea existed in girls, as a result of the disturbance of innervation from amenorrhœa, in cases where there is no anæmia, or where it has been cured by iron, we are advised to transmit a few shocks through the uterus, in addition to the sparks from the spine. In this way the catamenia are generally excited, and the rapidity of the cure is increased. We extract one of Dr. G. Bird's cases, illustrative of the success of this practice.

"E. R., aged 16, of previous good general health, menstruated, for the first time, three months ago. After the disappearance of the discharge, she became the subject of involuntary movements of the right arm and hand: these have increased in intensity up to the present time. She appeared at the electrical room in July, 1838: sparks were taken from the spine, and a few shocks passed through the pelvis. After the electricity had been applied five times, the catamenia occurred, and the chorea vanished. She continued well until September 19th, when, as the discharge had not appeared at its proper time, she again applied at the hospital. A few shocks through the pelvis excited the deficient functions, and she left quite well." (Dr. Golding Bird's Lectures, p. 37.)

The same writer observes, that he has never seen any good effects from the transmission of electric shocks along the affected limbs: "on the contrary, in every instance the involuntary movements have been increased, often to an alarming extent; and, if employed when the patient was convalescent, it has invariably aggravated every symptom."

Every practitioner must have occasionally met with cases of chorea, or an affection almost identical with it, in which a single limb, a particular set of muscles, or, occasionally, only a single muscle, is affected. Several cases of this nature are given by Dr. Bird, and seem to have yielded readily to the action of electricity. We extract two cases, scarcely deserving to be regarded as chorea, but interesting, as showing the power which electricity exerts over the involuntary movements of those muscles which are ordinarily under the control of the will.

"J. T., aged 40, accidentally dislocated his jaw in the winter of 1838; and, after its reduction, became the subject, upon the slightest excitement, and often without any apparent cause, of involuntary motions of the jaw, apparently referable to the pterygoid and the depressor muscles: these produced dislocation of the jaw, often several times in the day. On October 9th, 1840, this patient applied at Guy's Hospital, and was sent to the electrical room. Sparks were drawn over the affected muscles with remarkable effect, the involuntary movements diminishing so considerably, that dislocation of the jaw rarely occurred. On leaving off the electricity, the motions returned, and with them the spontaneous dislocation; but, whenever he recommenced its use, both these disagreeable symptoms vanished.

"In another very remarkable case, electricity was equally successful. The subject of it was a commercial traveller, who had been overtaken by a snow-storm on Salisbury Plain, and was nearly frozen to death. Soon afterwards, a curious form of partial chorea, affecting chiefly the sterno-mastoid muscles, appeared, and continued for a long time, the head being alternately carried, by a series of jactitations, from side to side with considerable violence, and he was accustomed to steady his head by holding his nose firmly with one hand. This man was long under treatment at the hospital, and ultimately recovered, on submitting him to the electrical treatment." (Dr. G. Bird's Lectures, p. 38.)

From the evidence we have given, it is sufficiently clear that electricity is a valuable agent in the treatment of this disease. "Of all remedies I have hitherto used," says Dr. Golding Bird, "except perhaps the sulphate of zinc, electricity seems most successful in chorea." It has this further advantage, that it interferes in no way with the internal administration of remedies—a remark that, of course, applies equally to all diseases in which it is employed.

If we were inclined to be hypercritical, we might stop to protest, in favour of the older authors we have cited, against Dr. Bird's assertion, that Dr. Addison was the first to suggest the employment of electricity in chorea; but, passing over this trifling oversight, we come to his description of the manner in which he supposes that the cure is effected. The conclusion at which he has arrived is, that it acts as a counter-irritant, and that it is more valuable than other remedies of this class, from its ready application, the intensity of its action, and the capability of receiving it daily. It exerts, he observes, a very important influence over the spinal nerves, and thus aids in submitting these to the dominion of the will. In addition to this, it seems to act by exciting powerful contraction of the muscles, and thus aids in overpowering their irritability.

Since the publication of Dr. Golding Bird's memoir, in the 'Guy's Hospital Reports,' Dr. Hughes's valuable "Digest of one hundred cases of Chorea" has appeared in the same work. After noticing the effects of purgatives, mineral tonics, as arsenic, iron, and zinc, vegetable tonics and antispasmodics, and antiphlogistic remedies, he observes, that the results of electricity are sometimes very remarkable in this disease. "When the body has been wasting, the mind apparently giving way, and the disease proceeding unchecked, if not increasing, electricity has, under my own observation and direction," says Dr. Hughes, "effected a really marvellous change." In the course of a week or ten days, the disease may be regarded as cured, if, at least, electricity is going to be of any service. There are cases, however, in which Dr. Hughes has seen, not only no benefit, but an actual aggravation of the disease from this remedy. In weak, nervous, timid children, it often increases the disease; and the cases in which he thinks it most applicable, are those occurring in young women with an hysterical tendency, and those protracted cases in boys, in whom other remedies have been tried ineffectually, and in whom the disease is dependent upon no obvious source of irritation.

Amenorrhœa is the next affection to which Dr. Golding Bird refers; and his remarks on the electrical treatment of this disease are, in our opinion, so sound, that we shall extract them for the benefit of our readers:

> "We have twenty-four cases of amenorrhœa reported; the youngest 15, the eldest 25 years of age, all unmarried;—
>
> "Of these, 4 were chlorotic,
> 6 but slightly so,
> 12 not at all so,
> 2 complicated with hysteria.
>
> "Of these, the remedy succeeded in all except the four chlorotic girls.
>
> "In electricity we possess the only really direct emmenagogue which the experience of our profession has furnished us with; I do not think I have ever known it fail to excite menstruation where the uterus was capable of performing this function. Disappointment will, however, most certainly result, if we have recourse to electricity merely because a girl does not menstruate; and we must never lose sight of the fact, that, after all, the large majority of cases of amenorrhœa depend upon an anæmic condition; and the patient does not menstruate, simply because she has no blood to spare. Nothing can be more ridiculous than applying electricity or any other local stimulant to the uterus when chlorosis exists; the first great indication will be, to restore general health, give iron to make up for the previous deficiency of that element in the blood, and then, and not before, think of stimulating the uterus. It is true that, in a large proportion of cases, the catamenia will appear as soon as the chlorosis is cured,—of course, in such cases, there will be no need of the employment of electricity; but still a large number will occur, in which, even after the complete relief of the chlorotic and anæmic condition, the uterus remains torpid and refuses to act. In such cases, a few shocks, transmitted

through the pelvis, seldom, if ever, fail in effecting menstruation. I have repeatedly known the catamenia, although previously absent for months, appear almost immediately after the use of the electricity; in more than one case the discharge actually appeared within a few minutes. The mode in which electricity has been generally employed, has been by transmitting a dozen shocks from an electric jar, holding about a pint, through the pelvis; one director being placed over the lumbosacral region, the other just above the pubes. In private practice, in which the employment of the cumbrous electric machine is very inconvenient, I have substituted with advantage the induced currents of the electro-magnetic apparatus, the conductors being placed as before. The alternating current may be used in these cases, as the electricity appears simply to act as a local stimulant, quite independent of exciting any uterine contraction." (Dr. Golding Bird's Lectures, p. 39.)

As far as we recollect, De Haen, in his 'Ratio Medendi,' the publication of which commenced at Vienna in 1758, in this, as in many other forms of disease, had the credit of first advocating electric treatment. He cautions us against using electricity with pregnant women, but thinks it excellent in obstructions; and remarks that it always promotes the catamenial discharge.

Birch, in 1779, published his 'Considerations on the Efficacy of Electricity in removing Female Obstructions;' and from these times down to the present day, the value of this agent has been more or less recognised.

In reference to the mode of applying electricity in these cases, we may observe that we have occasionally found advantage, in using the electro-magnetic machine, from applying the current more directly; which is readily done by placing one extremity of the wire of the electro-magnetic machine on the sacrum, while the other, terminating in a silver-electrotyped ball, and inclosed (all but the ball) in an elastic tube, is inserted in the vagina, so that the ball is in contact with the os uteri. In this way we have often seen great relief afforded in cases of dysmenorrhœa.

The strongest evidence, perhaps, ever published on the effect of electricity on the menstrual function, is exhibited in a case given by Dr. Le Conte, in the 'New York Journal of Medicine.' Five negroes were simultaneously prostrated by a single stroke of lightning on a plantation in Georgia, and were all taken up in a state of apparent death. Two of them gradually recovered their consciousness. One of them, named Sarah, was at least seventy years of age. Her clothes were rent; and, after a few days, marks of discoloration were manifested along the right arm and right side of the trunk. A violent paroxysm of vomiting followed the restoration to a state of sensibility. She complained of much pain in the region of the stomach and bowels, for at least a fortnight after the accident. It is unnecessary to enter into an account of the other symptoms she experienced, as it is sufficient for our purpose to observe that "the catamenial discharge which had, in accordance with the ordinary arrangement of nature, ceased for more than twenty years, was completely re-established!—at least a discharge from the genital organs, having all the obvious and sensible physical characters of the catamenia, and observing with rigorous exactitude its peculiar law of periodicity, has been established, and continues to recur, with the utmost regularity, up to the present time (August, 1844), after the lapse of more than a year! She has not missed a single menstrual period since she was struck by lightning." Her mammæ have become enlarged, and there are no signs of organic disease of the uterus.

Paralysis is the next affection to which we shall refer. We have already shown that it was in a great measure from the successful treatment of paralytic affections by electricity, that this agent was enabled to gain a permanent place in the *therapeia* of this country. Dr. G. Bird gives the following classification of cases of paralysis :

1. Cases of paralysis from the poison of lead.
2. „ rheumatic paralysis confined to the limbs.
3. „ paralysis limited to the portio dura.
4. „ paralysis following local injury to a limb.
5. „ hysterical paralysis.
6. „ paralysis dependent upon persistent cerebro-spinal lesion.

We shall briefly notice his remarks on these varieties. Of eleven cases of *paralysis from lead*, five were cured, four improved, and two not relieved. In cases of the dropped hands of painters, Dr. Bird finds that sparks drawn from the region of the cervical and dorsal vertebræ are generally useful adjuncts to medical treatment. He likewise directs them to be drawn from the paralysed parts ; and in recent cases small shocks, transmitted along the course of the affected nerves, have considerably accelerated convalescence. In chronic cases, Dr. Bird has often seen a cure effected by drawing sparks from the spine, after shocks had been passed along the paralysed parts in vain.

In reference to *rheumatic paralysis*, under which term we include all cases in which the palsy followed the sudden application of cold, independently of a spinal lesion, the same author refers to ten cases, of which five were cured, three relieved, and in the remaining two the result was doubtful. Three of the successful cases are given, and afford very favorable evidence of the electric treatment.

Paralysis of the portio dura next claims our attention. We give our author's graphic description of it, not that it contains anything new, or that we deem it likely any of our readers could mistake it for paralysis dependent upon cerebral lesion, any more than the audience Dr. Bird had the honour to address ; but yet we have seen the mistake made more than once. There is more likelihood, as recently pointed out by Dr. M. Hall (Med.-Chir. Trans. vol. xxxi, p. 155), that cerebral paralysis confined to the facial nerve may be erroneously attributed to lesion of the trunk of the nerve.

> "The history of these cases is sufficiently intelligible. A person previously in health exposes one side of the face to a little draught of air—as by sleeping near a window having a broken pane, or travelling in a railway carriage with a half-open window. The result of this is more or less pain and stiffness in the side of the face, followed soon by paralysis of the facial nerve ; the non-affected side being exceedingly distorted, from the antagonist muscles becoming palsied. Sensation is never influenced, the affection being strictly and exclusively limited to the seventh pairs of nerves. When consulted early in the affection, there may be some evidence of inflammatory irritation in the course of the *pes anserinus* and its branches, demanding appropriate treatment. Very soon, however, this irritation subsides, and, as is well known, the patient, if left alone, generally, with sufficient time, recovers. Still, however, the convalescence involves much time, and in many instances months elapse ere the symmetry of the face is recovered. In such cases the stimulus of electricity remarkably aids the cure: I say the *stimulus* of electricity, because in such cases it appears pretty certain that the agent in question acts merely as a local excitant, stimulating the paralysed muscular fibres, and

arousing their normal irritability, and once more placing them under the dominion of the will.

"In these cases it scarcely matters what form of electricity is applied, so long as it is sufficiently effective to induce contraction of the paralysed muscles under its influence: weak shocks from a charged jar, the passage of a series of currents from an electro-magnetic machine, or the direct irritation produced by drawing a series of sparks from the cheek (when the patient is insulated and connected with the prime conductor of an electrical machine), seem to answer equally well." (Dr. G. Bird's Lectures, p. 41.)

Three illustrative cases are given: we regard the last as the most important, in consequence of the very chronic nature of the case. A clergyman, who had suffered from paralysis of the seventh pair a dozen years previously, recently consulted Dr. Bird. The paralysis had never wholly disappeared. The face was not symmetrical, the saliva often flowed from one corner of the mouth, and his intonation was impaired. He sedulously applied the current from the electro-magnetic machine; and when Dr. Bird saw him some months afterwards, he was perfectly restored.

In the *paralysis following local injury,* we must not be very sanguine regarding the advantages to be derived from the application of electricity. When the structure of the nerve has been injured by the accident, no benefit can, of course, be expected from electricity or anything else; but if the nervous fibres are not actually disorganized (if, for instance, the paralysis has been the result of simple concussion, or of pressure), electricity is often of very considerable service. In these cases, however, we must use the single-current machine, alluded to in page 375; of course transmitting the current in the direction of the ramifications of the nerve. In the weakness which often follows the reduction of dislocations, Dr. Bird thinks that he has seen good results from this mode of treatment.

In treating of *hysterical paralysis,* Dr. G. Bird notices the extreme difficulty of distinguishing between the assumed and real form of palsy presented by hysterical women. He remarks, however, that if the patient is feigning, she can seldom resist the surprise of the shock, and the previously rigid limb (for this is the form of deception usually adopted) will generally instantly move; while, on the other hand, in hysterical paralysis, when the affection, however excited at first, is now influenced by the patient's will, Dr. Bird thinks that there are few curative remedies so important as the electro-magnetic current.

Paralysis dependent upon persistent cerebro-spinal lesion is the last form considered by Dr. Bird. No one, perhaps, has ever had greater experience than our author in the application of electricity in every variety of paralysis. Without entering into details, he gives us the following as the results of his observations:

"1st. When the lesion is recent, the cause producing the paralysis still active, electricity and its modifications not only do no good, but often do much mischief. I would give an especial caution where rigid arteries are known to exist, or ramollissement of brain suspected. In more than one example of these affections, I have known a fatal apoplectic fit quickly follow the use of the remedy in question.

"2dly. In paralysis accompanied by rigid flexure of the thumb or fingers, I have never seen electricity do any good.

"3dly. In cases of paralysis depending upon some physical cause, as effusion or pressure from other sources, when the original cause has been removed by time or treatment, or both, the palsy remaining, electricity, and especially the electro-magnetic form of it, is of the utmost value. These cases are certainly not uncommon, and to them the old adage of 'sublata causa, tollitur effectus' does not by any means apply. A patient has, for example, congestion, influencing chiefly one side of the brain; the arm, or leg, or both, become paralysed. After some time the circulation is equalised, the pressure is removed, and the paralysis, if the case be recent, disappears. If, however, the congestion has been of longer duration, the palsy does not disappear with the removal of the exciting causes, and then the passage of the single electro-magnetic current in the course of the nervous ramifications becomes invaluable. Often in a few days the patient recovers his power. In such cases of chronic paralysis, let me beg of you not to give up the electrical treatment too soon. Remember, that if the paralysis be long continued, some of the new tissue deposited in the palsied muscles, in accordance with the recognised laws of nutrition, has never contracted or moved under the influence of the will; and a patient's persistence in the electrical treatment will be necessary before the new fibres become roused into obedience to the *vis nervosa* propagated along the nerves by the volition of the patient." (Dr. Bird's Lectures, p. 43.)

We have thus followed Dr. Bird through his excellent description of the different forms of paralysis in which he has tried *electricity*. He has taken a more comprehensive view of the subject than any other British physician; at the same time it can hardly be said that he has brought forward any new facts, as may readily be seen by a reference to Mr. Donovan's Memoir; to the chapter on Electro-magnetism,* in the first edition of Graves's 'Clinical Medicine;' and to a brief paper by Dr. Neligan, "On the Therapeutic application of Magnetic Electricity in various forms of Paralysis," published in the 'Monthly Journal of Medical Science' for 1846.

No reference is made by Dr. Bird to the effects of electricity in paralysis of the bladder, an affection which, although often dependent upon disease of the brain or spinal marrow, or on some severe shock received by the system, not unfrequently comes on slowly, without any apparent cause, and, as far as we can see, without any other apparent change in the system at large.

In this latter form of paralysis, we have lately derived considerable advantage from the electro-magnetic current in two cases, and indeed regard it as the most efficient remedy we possess, with the exception of ergot of rye.†

A successful case of paralysis of the bladder, in a man aged 70 years, and in whom no remedy was resorted to but electricity, is given in p. 440 of Graves's 'Clinical Medicine' (first edition).

In the temporary paralysis often occurring after labour, it is also highly serviceable. The case of a lady, under the care of Drs. Goodwin and Radford, who, after her accouchement, was unable to evacuate the contents of the bladder, is described in the 'Provincial Journal' for December 24, 1844. All the usual remedies failed during a fortnight's trial; the catheter was employed two or three times a day, and could not be dispensed with. On Dr. Goodwin's suggestion, galvanism was tried; and the *first application proved successful.*

---

* In the Meath Hospital, it has been found successful in cases of amaurosis, ptosis, complete paralysis of the right lower extremity, partial paralysis of both lower extremities, complete paralysis of the right arm, lumbar rheumatism, sciatica, femoral and brachial neuralgia, œdema, catamenial suppression and irregularity, and spinal irritation.

† We observe that Dr. Day, in his recent work, 'On the Diseases of Advanced Life,' speaks highly of the use of ergot in these cases. He recommends that it should be given in the form of a strong tincture, prepared with six ounces of the ergot to a pint of spirit. A drachm of this tincture may be given three times a day in an effervescing draught of citrate of ammonia.

The effect of electricity in restoring tone to the bladder has been so long known,[*] that it is singular the remedy is not more frequently had recourse to.

Our limited space will not permit us to enter into any details regarding the use of electricity in amaurosis,[†] deafness,[‡] various forms of neuralgia, and epilepsy. Raciborski speaks very favorably of its use in neuralgia generally, but especially in that of the branches of the fifth pair, having witnessed many successful applications of this kind in the wards of M. Bouillaud. He has likewise found good effects from galvanism (or rather from galvanic acupuncture, which is a very painful process) in muscular rheumatism and lumbago; but the most fortunate results seem to be in the case of sprains. "Every one," he observes, "knows that a sprain often exacts much time for a cure. When it implicates the ankle or knee, it is not uncommon to see patients deprived of the use of their limbs for several months. It is the violent pain, felt upon the slightest motion of the parts (we are speaking of simple, uncomplicated sprains), which retards their cure. The other symptoms are of little consequence, and are speedily dissipated. Now, just as in lumbago, so in sprains, galvanism relieves this pain instantly, and allows the patient to walk without lameness."

A very old mode of curing lumbago, and other forms of muscular rheumatism, and one that we should much prefer to galvanic acupuncture, is noticed by Mr. Donovan, who has found it successful and speedy in its action. It consists in placing the patient on an insulating stool, and connecting him with the prime conductor. Three or four folds of flannel are then laid on the painful spot, and sparks drawn through the flannel. A copious eruption of red spots, or occasionally of whitish-red wheals, noticed almost a century ago by Kühn, in his essay *De Electricitate Medicamentosa*, is produced over the spot.

Galvanism is reported to have been successfully employed in *syphilis* by Krusell, of St. Petersburg. We believe that mild currents were passed across the surface of venereal sores, but are not acquainted with the details of the treatment.

---

[*] See Dr. Simmons's case of retention of urine, cured by electricity, published in the seventh volume of the 'London Medical Journal," 1786.

[†] We may refer the reader to a case of Mr. Potts, in the ninth volume of the 'London Medical Journal;' to the singular case of Samuel Leffers, who was restored to sight by a flash of lightning, and whose case is recorded by Professor Olmsted, in the twenty-ninth volume of the 'Annales de Chimie et de Physique;' to cases recorded by Rossi and Vassalli Eandi, in the fifty-sixth volume of the 'Journal de Physique;' to several cases in Loder's Journal; to numerous cases by Magendie, in the sixth volume of his 'Journal de Physiologie,' p. 156, &c.; to Grapengiesser's cases, recorded in his 'Versuche den Galvanismus zur Heilung einiger Krankheiten anzuwenden;' and to those of Clarke, in Graves's 'Clinical Medicine.' Dr. Bird, in a memoir, in the twelfth number of the 'Guy's Hospital Reports,' states, that, both in amaurosis and deafness, the results of the trials were too conflicting to afford any satisfactory conclusions.

[‡] In Priestley's 'History of Electricity,' there is an account of a woman, who, after being deaf for seventeen years, was cured in a short time by Mr. Wilson; but in six other cases he was unsuccessful. See the cases recorded by Sir William Blizard, in the eleventh volume of the 'London Medical Journal,' and by Carpue, in his work, 'On Electricity and Galvanism,' London, 1803; and those in Grapengiesser, op. cit. The early accounts of its effects in deafness differ extremely. Schaub, of Cassel, was said to have cured many patients. Pfingston (see Phil. Mag., vol. xv, p. 282) declared that galvanism is of very little use in deafness, while Sprenger succeeded in recovering no less than forty cases. Professor Grim of Liegnitz, in Silesia, also restored many persons who had suffered from deafness. More or less success attended the application of the current in the hands of Einhoff of Celle, Martens of Leipsig, Marcus of Bamberg, and Hellwag of Eutin. From a review of the collected evidence on this subject, Mr. Donovan concludes, "that galvanism greatly relieved almost every patient labouring under difficult hearing or deafness, and cured many." About two years ago, M. Bonnafont reported to the Academy of Sciences a case, in which complete deafness, of a year's duration, resulting from a comminuted fracture of the cranium, was cured by the action of galvanism and gaseous ammoniacal insufflations by the Eustachian tube.

We now turn for a few minutes to the work of M. Pallas. As we have resolved to confine ourselves to the consideration of the therapeutic action of electricity in this article, we feel compelled to restrict ourselves for the present to his chapter "On the effects of Electrical Insulation as a curative and preservative means in many diseases." We must premise that M. Pallas was physician-in-chief to the Military Hospital at Oran, where the French troops suffered much from dysentery and severe forms of intermittent fever. We shall allow him to explain his views in his own words:

> "I feel convinced that the great electric currents which exist, either in the atmosphere or on the earth, take an active part in the production of diseases in general, and especially in those of hot climates; and that by modifying the activity of these currents, which are always penetrating the human body, we shall be enabled to diminish, if not actually to destroy, the causes of the endemo-epidemic diseases of Algeria." (p. 262.)

Feeling convinced that his theory was correct, he proceeded to experimental demonstration in the following manner:

He had two beds constructed, whose legs rested on glass nearly a foot in thickness, so that they were completely insulated in reference to the ground. To one of the bed-posts there was attached a chain, with a glass handle at the free extremity, so that the bed might be completely insulated, or placed in communication with the ground at will.

His first trial of the effects of insulation was in the case of two patients suffering very severely from an aggravated form of dysentery. They were removed to these beds at noon, and in three hours' time there was a marked amelioration of their symptoms. One of them, who had had four bloody evacuations, with violent colic, shortly before noon, did not pass another stool till eight p.m.; and he improved so rapidly from the time he was insulated, that in five days' time he was in a state of convalescence. The other patient recovered from the period of his insulation, although not so rapidly.

It is worthy of remark that the pulse became considerably slower from the moment of insulation.

Two other patients—one suffering from a very intense attack of sporadic cholera, and the other from quotidian intermittent fever—were cured in three days by the above means.

These cases are followed by twenty-three others, in which the symptoms and treatment preceding insulation are fully detailed, and in which great benefit certainly seems to have accrued from this mode of practice. They embrace cases of acute and chronic dysentery, choleriform diarrhœa, intermittent fever, periodic epilepsy, acute articular rheumatism, visceral neuralgia, bronchitis, and meningitis.

From a careful examination of these cases—granting that they are faithfully detailed, which we have no reason to doubt—we cannot help acknowledging that insulation, in hot countries at all events, is a potent therapeutic agent in certain forms of disease. Its most marked and speedy effects were exhibited in cases of neuralgia. It always exerted a favorable influence on the number of the evacuations, and on the heart's action. Cases of intermittent fever—whether quotidian, tertian, or irregular—if not complicated with bronchial irritation, yielded to the sole influence of insulation, without the necessity of having recourse to quinine or any other medicine.

And here we must terminate our remarks on the interesting volume of M. Pallas. There are many parts of the subject, on which we have not touched; as, for instance, on the effect of electricity in the induction of uterine contractions, and on its uses in the hands of the surgeon in the treatment of aneurism, varicose veins, and ununited fractures. Its importance in cases of narcotic poisoning is very great, and several lives that would otherwise have been irremediably lost have been saved by its application. The following case, which we give in a very condensed form, will, we think, bear out this assertion. It occurred in Edinburgh about three years since, and was treated by a gentleman whose name is familiar to the readers of this journal, Dr. Martin Barry. A child, aged nine months, swallowed twenty-five minims of laudanum, which were administered by its mother at one a.m., with the view of procuring sleep. Soon after, the mother's attention was called to the child by its loud breathing; it was then applied to the breast, and fell asleep, and continued in this state for six hours. At seven p.m. the breathing was more oppressed, and the child insensible to all impressions: at eight "its countenance was pale, with an expression of deep and placid repose; eyelids closed; and the child appeared in a state of slumber, and if roused, it exhibited a tendency to relapse into sleep; breathing laborious, at times stertorous, at others accompanied with a distinct stridor; there was bronchial irritation and expectoration. An emetic of one grain of tartar emetic, with five grains of ipecacuanha in solution, was then given alternately until vomiting occurred. At half-past eight the mother confessed that she had given laudanum, when another emetic of three grains of antimony, and five of ipecacuanha was given; and flagellations and repeated agitations, together with cold affusion, were employed. The tartar-emetic solution was still repeated in half-grain doses every ten or fifteen minutes to relieve the chest, and vomiting was always attended with benefit. This treatment was continued for two hours; but the restored consciousness was always imperfect, and succeeded by coma when the stimulating influence was withdrawn. At one o'clock p.m. congestion of the brain had increased; all means failed to rouse the patient; the coma was augmented; and the vital energy decreasing rapidly." Dr. Barry now had recourse to the electro-magnetic machine; and it was not till the greatest degree of power was used, that signs of perfect recovery ensued. The application was continued for five hours. We believe there is no other case on record in which a child under one year of age recovered from so large a dose.

With this instructive case we close our remarks on the application of electricity, in its various forms, to the treatment of disease.

M. Becquerel's volume is one of high interest, and well deserving the attention of every medical man who is desirous of making himself acquainted with the present state of our knowledge of physics in the widest acceptation of the term. As, however, it does not bear very directly on the subject of this article, we postpone for the present any detailed account of its high merits.

ART. VIII.

Traité des Fractures et des Luxations. Par J. F. MALGAIGNE, Chevalier de la Légion-d'Honneur et du Mérite Militaire de Pologne, Membre de l'Académie Royale de Médecine, Chirurgien de l'Hôpital Saint-Louis.—*Paris*, 1847. Tomes II.

A Treatise on Fractures and Dislocations. By J. F. MALGAIGNE, Member of the Royal Academy of Medicine, and Surgeon to the Hospital Saint-Louis. *Paris*, 1847. 2 vols. 8vo. Vol. I (*On Fractures*), pp. 842.

THE prolific pen of M. Malgaigne has again been at work, and we have before us the First Part—and a goodly instalment it is,—of a systematic treatise on 'Fractures and Dislocations.' The present volume is accompanied (as its fellow is intended to be) by an Atlas, containing a series of drawings, very well executed on stone, and illustrative of the letter-press. Altogether, this division, which we propose reviewing, and which comprises fractures only, is very complete; and is a good specimen of the style of work in which our continental neighbours excel us.

On turning to the preface of our author, for the purpose of ascertaining whether he had any especial object in the present publication, we were surprised to find that he purposed to fill up a void in the surgical literature of his country;—France not possessing, as do England and Germany, even one systematic treatise on the injuries of bones. We were certainly startled by this announcement; but, on consideration, we could not gainsay it. This branch of surgery has, indeed, received contributions, and not unimportant ones, from the pens of many French authors, both in the form of monographs, and in their comprehensive systems of surgery; but we are unacquainted with any work in the French language, of compass equal to the present, which is exclusively devoted to the consideration of fractures and dislocations.

M. Malgaigne by no means professes to limit himself to the product of his own observation and experience, in the compilation of the present work. On the contrary, it is destined to present to the reader an historical assemblage of "doctrines and ideas," on the subject of which it treats, from the earliest period to the present day; and the Museums of the Hospitals, Val de Grace, and Dupuytren, have been severally laid under contribution, to assist in supplying the materials for nearly one hundred figures, which are contained in the sixteen plates of his Atlas. Further, a general and arranged bibliography is promised at the close of the second volume.

The volume before us is divided into two parts, of which the first, occupying nearly half the work, is devoted to "Fractures in general." The divisions and subdivisions of this section are numerous, and comprise, under various headings, the causes, varieties, diagnosis, treatment, progress, &c., of fractures generally. The second part treats of "Particular fractures,"—that is, the fractures of particular bones.

It is scarcely necessary to observe, that it would be entirely beyond the compass of a review, to present to our readers a complete, or even an imperfect, analysis of *all* that the volume contains. We shall content ourselves with merely a brief notice of many parts, whilst we select others for more careful consideration; trusting thus to put those who may honour us by the perusal of our article, in a fair way of judging for themselves of the excellencies or deficiencies of our author.

Our author's definition of fracture is,—"a sudden or violent division of bone or cartilage;" though he admits that the vulgar interpretation of the word, or rather its synonym in common language, conveys so appropriately a just and correct impression of the nature of the injury inflicted, that a risk is incurred of complicating, instead of simplifying, the technical term, by attempting to define it. Under the head of "Etiology," our author considers the following subjects: certain general predisposing causes apart from disease; morbid affections which act in this way; and determining causes. The influence of age, sex, and seasons, are combined under the first of these subdivisions.

The materials from which M. Malgaigne's conclusions have been drawn, in relation to the influence of age as a predisposing cause to fracture, are derived from the records of the Hôtel Dieu, during a period of eleven years. The number thus afforded for this statistical purpose is 2377. As such tables as this are valuable, we give the general conclusions derived from them: they are as follows. Fractures are most frequent between the ages of 25 and 60 years, and become more rare as the age recedes in either direction from these extremes: fractures, therefore, are least frequent in the very young and very old, the number being identical (in the tables of the text) as regards the annual average, between the ages, severally, of from 5 to 15 years, and from 75 to 80; but the average of these injuries between 2 and 5 years, rather exceeds that of the period of life above 80. The present, however, is an instructive exemplification of the caution which is required in drawing conclusions from apparently sound statistical data; and the source of error here is pointed out by our author, who remarks that, by referring to the census, he discovered that individuals between the ages of 5 and 15 constitute one fifth of the whole population; whereas, the returns prove that only one twenty-third part of the total amount of fractures occur between these ages, &c. Another table represents the relative frequency of fractures in young children, the materials being supplied by the records of the Hospital for Children. This shows that fracture is most frequent between the ages of 2 and 4, and least frequent between 4 and 5; but again increasing, though in a diminished ratio, up to 7. The final conclusion from these investigations is, that, taking the age of the population into account, fracture is most rare between 4 and 5 years of age, and of most frequent occurrence between 55 and 80. The following are M. Malgaigne's suggestions as to the causes of these results:

> "In searching for an explanation of these facts, one may suppose that, between the ages of 2 and 4, falls are more frequent, from the tottering gait of the infant, and more serious, from the skeleton possessing as yet but feeble powers of resistance. After 4 years of age these causes diminish, but are replaced by others, such as games, running, fighting, &c. At puberty, the various employments of the mechanic and labourer are superadded, man being in the full exercise of his physical powers from 25 to 40; and after 40, the degeneration of the osseous system. If this last and influential predisposing cause of fractures does not augment the number of these injuries between 55 and 80, and even permits of a diminution of the average after that period, it is because the infirmities of age become an exemption, by withdrawing individuals from many occasional causes of fracture, such as heavy work, personal encounters, &c." (p. 4.)

We are scarcely disposed to admit, with our author, that the skeleton begins to degenerate so soon as forty; and we think he has overlooked some of the true causes why fracture becomes more frequent, up to a certain point, with advancing years. We allude to the diminishing activity of the muscular system, together with the frequently increasing bulk of the frame generally. Thus, the young and active often extricate themselves from the risks to which they are subjected, and which are frequently attended with serious results to the less agile. Moreover, there is a resiliency in the youthful frame, which is much impaired in middle life, and gradually lost as age encroaches, so that the body falls more as a dead weight; and, if we superadd to this, the fact of the frame augmenting very often in bulk and weight, so that increased momentum is given to a fall, we have altogether an important class of causes in operation, which are not noticed by M. Malgaigne.

In speaking of the obsolete opinion of Boyer and other surgeons, that a preponderance of the earthy constituents of bone renders fracture more frequent in the old than young, our author combats this notion, which was founded on hypothesis and not on experiment. He adds that, so far from this supposition being correct, it has been ascertained that, in those bones where the *animal* matter predominates, fracture is more frequent, the compact bones of adults being possessed of the greatest power of resistance to external force. He seems to have devoted considerable attention to this subject; and the views published in his 'Anatomie Chirurgicale,' are stated to have been confirmed by subsequent researches at the Hospital Bicêtre. Some skeletons, he remarks, become even more dense, and therefore resisting, with advancing years; but, more commonly, the texture of the bone is thinned by absorption, which, we think with him, is the true cause of fracture, especially of the ribs and neck of the femur, in the aged. The extreme thinness of the bones of some persons in advanced years is rare, and is regarded by M. Malgaigne as an abnormal condition, and treated of elsewhere.

As regards the influence of season on the production of fracture, our author denies that winter is more prolific than summer. He admits that falls are more frequent, but they are less severe; and the slippery state of the pavement is more than counterbalanced by the operation of other causes that are productive of severe falls, such as building, &c., which are more particularly confined to the summer season. In these remarks, reference appears to be made exclusively to a town-population. Again, fractures have been found by our author to be decidedly more rare in winter, amongst children, than in summer; which he accounts for, by supposing that they are kept at home by their parents, instead of being allowed, as in fine weather, to wander about and play.

Some useful and interesting tables follow, exhibiting the relative liability of different parts of the skeleton to fracture: these, however, we pass by, merely noticing that the right side of the body is shown to be more obnoxious to these injuries than the left; and that fractures of the extremities, especially the inferior, are more common than those of the trunk.

The following is a summary of our author's opinions on the subject of certain affections, which have been regarded as predisposing to fracture. In the first place, he divides these causes into two classes,—those which render the bones more friable, and those which inordinately augment the power of muscles acting on the bones. In speaking of scorbutic affections, M. Malgaigne states that the Bicêtre has an annual visit, in the spring, from this disease; but that he has never been able to trace a single fracture to its influence. Of gout he has been able to discover but two instances, and those not very satisfactory. He admits the influence of cancer, but regards the cases as rare, and the conditions associated with the disease, in its operation upon the osseous system, as but little understood. Where spontaneous fracture occurs in cancerous subjects, he regards this effect as due, in the majority of cases, to cancerous degeneration of the bones themselves; exceptional cases sometimes occur, however, in which there is a species of atrophy, similar to that of old age. Scrofula, in our author's opinion, is rarely influential in determining fracture; but rickets he regards as the most rife cause, and that not in childhood only. In addition to the above, many other casual affections are enumerated, such as necrosis, hydatids, osteo-sarcoma, the neighbourhood of an aneurism, &c. But there is still one predisposing cause, which our author considers has been much overlooked, and which assists in explaining why long bones are sometimes fractured by muscular action; and this is a local inflammation of the osseous tissue itself, signalised by dull pains, usually set down as rheumatism. Of this form of disease, as predisposing to fracture, M. Malgaigne cites some instances.

In the next section, which treats of the immediate or determining causes of fractures, we meet with some very good remarks; but shall be able only to cull one here and there for the benefit of our readers. The two great heads under which our author includes these causes are, "External Violence, and Muscular Action." He remarks, indeed, that a case is reported of a rib being fractured by the shock of the heart's action (therefore an *internal* cause), but very properly adds, that it was a presumed, rather than a satisfactorily proved, cause; and therefore not worthy of constituting an exception to the above classification. The subdivision adopted is that which is usually recognised as distinguishing the causes of fracture which operate from without, viz. into direct and indirect; but M. Malgaigne adds, a little further on, an opinion, in which we entirely concur, that the momentum of a fall requires (usually) the superadded influence of muscular action in fixing the bony levers, in order to the production of fracture; and this is especially apparent in indirect fractures, as proved by the difficulty of breaking a bone in this way in the dead body. As a further corroboration of this opinion, he cites the proverbial immunity of drunkards from the severer effects of falls,—a proverb, by the way, the truth of which our own hospital experience scarcely confirms.

At the close of the present section, on the causes of fracture, violent explosions of gas are mentioned; and a strange instance is cited, as having occurred at the catastrophe on the Versailles railway. It is that of a circular fracture of the skull, which was shivered by the disengagement of steam, consequent on the action of heat on the brain (*ébullition du cerveau!*) —a horrible example, indeed, as M. Malgaigne observes, of the "power of these new agents set going by the hands of man."

One curious branch of this division of our subject remains to be noticed, viz. the occurrence of fractures before birth. Apart from the clumsiness of the accoucheur or midwife, or from disproportionate development of the head, and of the pelvis through which it has to pass, there appears no reason to question the fact, that fracture may occur during uterine life. Devergie relates an instance of a woman having struck her abdomen violently against the angle of a table, when six months advanced in pregnancy; and, on being delivered at her full period, she produced a child whose clavicle had been fractured, and firmly, though not very accurately, united, as was proved by a post-mortem examination, the infant dying shortly after birth. A similar case occurred to Carus; but in this instance the fracture was compound, and of the leg. The protruding bone was necrosed, and the infant died of mortification of the limb. But the most remarkable example of fracture occurring within the womb, and related by our author, is one in which no external injury was received by the mother; but she heard, during a movement of the child, a noise like the breaking of a stick. She was delivered, six weeks afterwards, of male twins, one of which had a broken thigh. The speculators on the mode in which this fracture was produced have surmised, that it was either caused by muscular action or by the entanglement of the limbs of one fœtus with those of the other. We think, with M. Malgaigne, that it is wiser to await the occurrence of similar cases before we decide that such a lesion can only occur where there are twins; in truth, we are at a loss to comprehend what value is to be attached to the determination of the case either way, except as a matter of simple curiosity.

Want of space obliges us to pass by the greater part of the ensuing chapter, on the "Varieties of fractures;" and this we do with the less regret, as we shall have casually to allude to the subject in our notice of the second division of the volume. Under the head of "Incomplete fractures," we observe that some notice is taken of the possibility of bones being bent,—an explanation which some have preferred to the supposition that a bone may be partially or incompletely fractured. We believe in the possibility of either of these forms of injury, though they certainly are not of common occurrence. M. Jurine, of Geneva, seems to be of a contrary opinion; for he says that accidental curvature of the bones of the fore-arm is not very rare, and speaks of having seen as many as a score of cases. He has also seen an instance of curvature of the humerus, and another, in which the bones of the leg were similarly affected. His treatment is based on the simple principle of gradually unbending the curve by pressure. He places a splint on the palmar aspect of the hand and fore-arm (when the bones of the latter are the seat of the injury), and applies a roller firmly, from the fingers upwards. The experience of most authors is decidedly in favour of the rarity of this form of injury; one case, in which we judged the forearm to be thus affected, has come within our own sphere of observation; and we also remember to have seen an instance, in which the parietal bone was "dented in" by pressure, in a young child, without any apparent fracture of the bone. The absence of head symptoms added to the interest of this case. We apprehend that firm but cautious pressure with the thumbs on the salient part of the curve should be made, before applying the splint and bandage as recommended by M. Jurine. The treatment of these accidental curvatures and incomplete fractures should be conducted on the same principle; therefore the question of diagnosis between them resolves itself into a matter of pathological curiosity.

M. Malgaigne's division of "Complete fractures" is into *transverse, jagged* (*dentelées*), and *oblique;* to which he adds, separation of the epiphyses of bones. In speaking of denticulated or jagged fractures, he states that he has conducted a series of experiments on the dead body, to render this division of his subject more complete. He found, when he broke a bone with a heavy iron bar, that, in the immense majority of cases, this form of fracture was produced, and in such a way that there was most frequently neither displacement nor crepitus, in consequence of the angular points dovetailing into the corresponding depressions of the fractured surfaces. If the force employed in the production of these artificial fractures was sufficient to occasion partial displacement of the broken ends of the bone, this effect our author found due to the breaking asunder of some of these tooth-like processes, which thus became so many isolated fragments or spicula; whilst others, retaining their attachments to the main fragments, sufficed to prevent the displacement from becoming complete; and the same cause operated in rendering reduction difficult.

The foregoing experiments, and the marked uniformity in most instances of the result, induced M. Malgaigne to direct his attention especially to the subject in cases of fracture which came under his notice; and he found his conclusions entirely verified.

> "I think, therefore," says he, "that I may lay it down as a general law, that the greater number of simple fractures, produced by direct force, are denticulated fractures, presenting the same phenomena in the living as in the dead: that is to say, either unattended by any displacement, or accompanied by either partial or complete displacement; but, in the latter two alternatives, the difficulty of reduction is greatly augmented by muscular action. I shall notice further on that oblique fractures themselves very often present these denticulations, and that certain comminuted (*multiples*) fractures are only denticulated fractures, in which a large tooth has become isolated by the same shock which broke the bone." (p. 63.)

This is an interesting generalization, and one worthy to be remembered by the practitioner.

Accidental separation of the epiphyses of long bones is classed by our author amongst fractures; and, for all practical purposes, there can be no objection to the classification. He attributes the first distinct and satisfactory notice of this form of injury to Bertrandi, towards the close of the last century; and cites some authenticated instances, in which it was produced, from rough and careless treatment, in bringing children into the world. The bones which suffered were the femur, tibia and humerus. These accidents may ensue from analogous causes after birth, and up to the time of puberty. They are, however, of rare occurrence, and require the same treatment as fractures in the same situations,—a fortunate circumstance, inasmuch as we are acquainted with no satisfactory diagnostic by which to distinguish between the two forms of injury.

The section on "Comminuted fractures" (*fractures multiples*) follows, and is treated of under four distinct heads, viz. fractures with splinters; cases in which the same bone is fractured at several points; those in which the bone is crushed; and lastly, fractures which involve several bones at the same time.

It will be perceived that the French and English terms do not exactly correspond,—the former being more inclusive than the latter. Our *compound* fractures, again, are included under the more generic head of "*fractures compliqueés*" in the French treatise; this term comprising, in addition to what we mean by a compound fracture, all concomitant lesions in the neighbourhood of the seat of primary mischief. The coexistence of fracture and dislocation M. Malgaigne admits to be very rare, as, indeed, his table of the cases, already referred to, shows. Of the 2358 fractures which constitute his chief statistical resource, four only were complicated with dislocation. In the first, the neck of the humerus was fractured and its head dislocated; in the second, the same form of dislocation coexisted with fracture of the shaft of the bone; and in the other two, the double injury affected different bones, and therefore belonged, in reality, to a distinct class of cases.

The general symptoms attendant on fractures form an interesting and important section, which includes the ordinary signs by which these injuries are characterised and identified, such as the cracking sound at the time of the accident, and the subsequent pain, loss of power, swelling, abnormal mobility, &c. We shall limit our observations to the last-mentioned of these diagnostic indications, as it will afford us an opportunity of allowing our author to speak for himself, in giving directions how to elicit this important sign. After complaining of the careless manner in which this source of diagnosis has been treated of by most systematic writers, and making some general remarks upon the subject, he proceeds in the following strain:

> "The different means to be had recourse to for detecting mobility in a broken bone must be varied according to the seat and nature of the fracture. When the humerus or femur is the seat of injury, it is necessary to fix the upper fragment, and to carry the knee or elbow forwards or laterally; an angular yielding of the limb will then be perceived where the fracture exists. In fracture of the clavicle, the weight of the shoulder is sufficient to produce this angle; and, if it should not be apparent when the patient is lying down, it may be readily rendered so by drawing the shoulder downwards and inwards. In fractures of the fibula, Dupuytren recommended that the fingers of either hand should be placed along the tibia, whilst the two thumbs rested on the fibula at some distance apart from each other; pressure was then to be made by each thumb alternately, so as to force inwards the broken extremities of the fibula at the seat of fracture. A similar plan may be adopted where the tibia or one of the bones of the fore-arm is the seat of injury. But when the fracture is near to a joint, it is necessary to resort to other methods; and it is essential to ascertain by what alteration in position the fragments move most readily on each other. Thus, Desault suggested, in doubtful fractures of the cervix femoris, that the thigh should be rotated; and the arc of the circle described by the great trochanter ought, according to its extent, to indicate whether this apophysis is still supported by the uninjured neck of the femur, or whether it turns upon its own axis. M. Maisonneuve considers the following to be valuable tests in some fractures of the lower extremity of the radius or fibula. In the former, mobility is often rendered very apparent by forcibly bending the hand backwards; and, in the latter, the fragments are separated by directing the point of the foot outwards,—results which no other plan of proceeding can elicit." (p. 91.)

We are subsequently warned against certain sources of error in forming a diagnosis from this sign; such as diseased softening of bone; and the natural flexibility of the fibula (pointed out by Dupuytren), which is very apparent towards its centre, and when deprived of support. The same remark is applicable to the ribs. Again, the mobility of a neighbouring joint, when the supposed fracture is near to it, may be mistaken for the abnormal mobility of a broken bone.

Lastly, our author notices one source of error which has misled or left him in doubt, viz. a condition of integument, in which that structure has been deprived, by age or inflammation, of its natural suppleness; so that, in flexing a limb, to ascertain the presence of any abnormal mobility, the skin has been thrown into transverse folds, simulating the appearance which would be presented if the limb had really lost the continuous support of the bone or bones. We need scarcely remark, that this and the other sources of deception can scarcely mislead a surgeon who has had even a moderate share of experience; but it is important that the young practitioner should be made acquainted with them and bear them in mind. To distinguish between indistinct crepitus in the neighbourhood of an articulation, and that which is proper to the joint itself where the synovial membrane is affected, is much less easy, and may perplex or deceive even experienced practitioners. But it is quite unavailing to lay down any rules or to give any directions, upon which the inexperienced can depend in this difficulty. Personal observation is the only sure guide, and even that, as we have remarked, is sometimes insufficient. We cannot say we have ever derived much assistance from the use of the stethoscope in the detection of crepitus, though M. Lisfranc speaks highly of it. An educated hand and delicate touch are more discriminating than even the practised ear, under the circumstances we are considering.

The next section, on the "Progress and terminations" of fracture, includes several subdivisions of importance, which it behoves us to glance at as we proceed. M. Malgaigne divides the period required for the reparatory process into three stages of equal extent. The first is occupied by the preparatory steps of dispersion of the extravasated blood, subsidence of the inflammation, and the secretion of the materials necessary for reunion. During the second section of the entire period, these materials become organized, and assume increasing consistence around the seat of fracture; whilst the third is dedicated exclusively to the process of ossification. "After this," says our author, "I admit no further stage; and this remark will find its application when I discuss the theories which have been broached in reference to the progress of the formation of callus."

A passing allusion is made to an observation of Guenther, on the arrested growth of the nails during the period occupied by the reunion of a fractured bone of one of the limbs,—the doctor thinking he had made the discovery, that this condition is constant in such cases, and that the growth does not again commence until reunion is completed. M. Malgaigne has put these observations to the test, and has arrived at the conclusion, that the Saxon doctor was the dupe of his own imagination or of his patient.

The origin of callus—a much disputed point,—deserves some notice. A brief retrospective view of all the hypotheses propounded regarding this interesting subject is first taken by our author, who, however, limits his observations to two theories, which he traces severally to Galen and John Hunter. Respecting these he makes the following remarks:

> "The most remarkable point associated with these theories is, that they are not deduced from actual and particular observations, like most of the others, but that they are propounded more in the form of *à priori* assumptions, or rather, as flowing from the general notions which their authors put forth regarding the restoration of divided parts,—Galen teaching that all solution of continuity (in organized parts) is reunited through the medium of the nutritious juices proper to each part, and Hunter attributing the same function to the extravasated blood. If one thing in this history is calculated to excite one's wonder more than another, it is the contemplation of modern genius succumbing to ancient,—John Hunter bowing to Galen." (pp. 114-15.)

It is not our intention to discuss the merits of these hypotheses, or of M. Malgaigne's comments upon them. We shall confine ourselves to a brief abstract of the account given by him of the process under consideration.

The first phenomenon, as admitted at all hands, which presents itself on dissection, is the extravasation of blood, in varying quantities, around the seat of fracture. This is a necessary effect of the rupture of vessels, consequent on the injury sustained; and at the same time it appears to be made subservient to the subsequent steps which are essential to the reparation of the lesion.

The second stage consists in the outpouring of coagulable lymph, which gradually replaces the effused blood. This usually takes place in the course of twenty-four hours after the occurrence of the accident. This plastic lymph, our author considers to be produced conjointly by the medullary tissue and periosteum; "perhaps also," he adds, "by the fractured surfaces of the bone,"—an opinion in which we concur. Though this lymph is poured out into the neighbouring tissues surrounding the seat of fracture, it is more especially met with in the periosteum and interior of the bone. The succeeding step is the organization of this new deposit by the penetration of vessels into its interior, the extravasated blood disappearing *pari passu*. As to the organization of the clot itself, our author considers that, as a fact, it rests on no foundation or proof which can support it. Thus much concerning simple fractures. With respect to those which are in communication with the external air, M. Malgaigne remarks:

> "As far as my means of observation have enabled me to judge, in man, the outpouring of coagulable lymph takes place in every part where the air does not penetrate, and fills even the intervals between the fractured extremities of the bone, when they are so placed in relation to each other, as to be preserved from the contact of the air. Granulations spring only from the suppurating parts; and, instead of aiding in the reunion of the fracture, they are produced at the expense of the bone, which they excavate, or cause the absorption of, wherever they are developed. So that, if, in the most complicated cases, a process of suppuration should have denuded the extremities of the fragments, which have thus become eroded and covered by fleshy granulations, it is necessary, in order to reunion, that lymph should be specially deposited between the opposing surfaces of granulations; and it is in this lymph that the ossific deposit takes place, by which the loss of substance is compensated for. On the contrary, when an osseous surface continues isolated, without relation or possible chance of union to another surface, then no fresh bone is deposited, and the loss of substance produced in the manner above noticed is permanent." (p. 118.)

This organization of the callus is, however, but the completion of the first great stage in the reunion of a fractured bone; or, more correctly speaking, the preparatory process which necessarily precedes that of ossification, and is, therefore, in itself transitory, and but a means to an end. It is not our intention to accompany our author through his discussion of the various and contradictory opinions which have been entertained and promulgated on this subject. As the theory of Dupuytren, so complete and apparently satisfactory and conclusive in itself, has been so much in vogue, especially in France, M. Malgaigne has devoted a considerable space to analysing its correctness. We may remind our readers, that the eminent surgeon to whom we refer, divided the process of reunion of a fracture into five distinct stages; to wit,—1, the vascular distension of the soft parts, which extends over a period from the first to the eighth or tenth day; 2, the epoch in which cartilage is formed, reaching from the tenth to the twenty-fifth day; 3, that in which spongy ossification ensues, including a period from the last date to the fortieth or even to the sixtieth day, according to the age, constitution, and health of the patient; 4, in the fourth period compact ossification takes place, and this is not completed until the expiration of the fifth or sixth month; and lastly, 5, the epoch of definitive or permanent callus, which is accomplished by the eighth, tenth, or twelfth month. The main objection which M. Malgaigne considers to attach to this theory, as, indeed, to many others, is, that it cannot be proved that any transformation to cartilage ever takes place in man. He does not deny that such a stage may have been observed in the lower animals, though the identity of this temporary deposit with the true and permanent texture of the same name, may fairly be questioned; but he agrees with André Bonn in stating that he has never observed such transformation, where he has had the opportunity of examining a fractured limb at this period. He admits, however, that his observations in the human subject have been limited to the adult and aged; and suggests that, probably, the period of cartilaginification (if we may be allowed the word) may occur in the young,—or, in other words, that the plastic lymph in children assumes the appearance of cartilage before ossification ensues; whereas, in the adult, the corresponding tissue has a reddish hue, and presents more of a fibrous character; and in this the ossific deposit takes place.

Our author takes objection likewise to the fourth period of Dupuytren. He considers that the hypothesis of the conversion of spongy into compact callus is founded on too few observations and experiments to be of any value; and cites examples (some, indeed, from Dupuytren's own lectures) to support his objection. In many instances, he asserts (and we think correctly), that the spongy union is the permanent condition of the reunited bone after fracture. The conversion into compact tissue he regards as the exception to the rule; and even where such conversion ensues, he considers that Dupuytren has no grounds whatever for limiting the period to the interval between the third and sixth month.

We must not, however, pursue this interesting problem any further, but refer our readers to the original for the opinions of our author in detail. We may, nevertheless, remark, in dismissing this section of the work, that we consider its subject anything but exhausted, and as offering more fruits to the laborious and patient cultivator of pathology.

We pass by the succeeding articles on the "diagnosis, prognosis, and treatment" of fractures generally; as well as the greater part of those on the "reduction of fractures," and the relative merits of various forms of apparatus employed for retaining the injured parts in proper relation; confining our notice to a few interesting details which occur towards the close of the first division of the work. First, respecting the influence which the movement of the trunk has upon fractures of the lower limbs, especially the thigh. M. Bonnet instituted a series of experiments, in which he proved, and probably exaggerated, the ill-effects of raising the pelvis to place the bed-pan beneath it, and such-like movements. Without underrating this serious inconvenience, M. Malgaigne attaches less importance to it than M. Bonnet, but gives directions how the patient should be raised. By far the best and most effectual remedy is to avoid the cause of such mischief, by having a trap to open beneath the patient. As to the lateral and rotatory movements, there can be no reasonable apology for them, except in unusual circumstances, such as a protracted confinement, &c., when they are not likely to be as injurious, if at all so.

As opposed to the entirely immoveable forms of apparatus, we have the "swing-box," in which the limb is suspended, and which we have employed with advantage in some instances of fracture of the leg; but it is entirely inappropriate in fractures of the thigh; and we would remind those who draw inferences on such a subject as this from experiments on the dead body, that one, if not the most, important element in the consideration of the subject, is lost sight of, viz., muscular action.

The section devoted to the treatment of compound and comminuted fractures, generally, contains many valuable hints, with which, in the main, we concur. With respect to the most serious injuries to the bones of the face, we can bear testimony, from frequent observation, to the extraordinary power of reparation,—dependent, doubtless, on their high vitality,—possessed by this division of the osseous system; with the exception of the lower jaw, which seems to come into the same category, in this respect, with the long bones. As regards the removal of spicula or detached fragments of bone, where the fracture is both compound and comminuted, we regard it as sound practice to remove all such as are entirely isolated, or nearly so, and to enlarge the external wound for that purpose. Where still firmly attached, the size, form and position of such fragments, together with other casual circumstances associated with the case, must guide the surgeon as to the proper course to pursue. Paré's simple but practical recommendation should always be followed, viz. to explore with the finger, and not to trust to the questionable information obtained through the medium of probes or other instruments. On the proper and careful dressing of the wound in compound fractures, our author justly lays considerable stress: whatever apparatus be employed, this part of the limb must be left exposed, and within reach of the surgeon, that it may be daily looked to, and, if necessary, dressed: all constriction should be avoided, and warmth and moisture substituted for other dressing, where an angry and suppurative action exhibits itself. These principles leave no hesitation in the mind of the practical surgeon as to the propriety of selecting that form of apparatus which at once secures entire immobility of the limb, at the same time that the superficial injury can be readily exposed, without such disturbance of the splints, as will risk displacement of the fractured bone or bones. We therefore do not hesitate to enter our protest, in common with M. Malgaigne, against any form of apparatus which requires readjustment every time the wound is dressed; as well as against the mischievous practice of dressing the wound for once, and then inclosing it in whatever mechanical contrivance may be selected for keeping the limb at rest, and leaving the remainder to Nature.

Under the head of "accidental complications" of fractures, the subject of vascular lesions is considered. There are not many points of practice which call for more cautious discrimination, as well as promptitude of action, than those which involve the question regarding amputation in fractures. It has been our lot, and we believe every conscientious surgeon will make the same confession, occasionally to witness recovery after we have counselled amputation and it has been resisted by the patient; and to see death ensue when we had determined on attempting to save a limb, where the complicated nature of the injury had raised the question of its removal. Of all these complications, probably injury to blood-vessels is that which calls for the most calm and serious consideration in the treatment of these injuries. Frequently extravasation of blood may take place into the whole soft parts of a limb, without external bleeding, or positive means of ascertaining the source of the hemorrhage; thus endangering the immediate vitality of the limb from mechanical pressure. This is, however, only one of the causes of risk where a blood-vessel of importance is wounded. All must be reflected on, and carefully weighed; and the age, constitution, temper, and especially the previous habits, of the patient, must constitute elements in the consideration of the question, and data on which to found a judicious opinion. This interesting subject is fairly discussed by our author, and we agree with him in opinion as to the proper course to pursue as a general rule; though we must qualify that opinion by stating that there are cases which admit of no alternative but that of amputation. In simple fracture, complicated with a pulsating tumour, M. Malgaigne prefers placing a ligature on the main trunk of the wounded artery to the alternative of amputation, a practice which is justified by the successful cases recorded by Dupuytren, Delpech, and B. Cooper. Further, he considers the same practice preferable to the condemnation of a limb in cases of compound fracture, *where the lesion is not of that character which otherwise renders the sacrifice imperative,*—a qualification which must be accepted unreservedly before we can subscribe to the opinion.

Tetanus is a complicating sequence of a very serious nature; and the question of amputation is not unworthy of consideration, though we fear we must add the results of our own experience to that of M. Malgaigne, in sad confirmation of the general futility of an operation where this dire disease has been once fairly established. The only instance which we can recall to memory of decided relief, *apparently* consequent on removal of the injured part, was a case of contusion of the foot, involving mischief to the phalanges of several of the toes. In this instance the nervous affection existed in a subacute form; and the case therefore seems to confirm the statement of Larrey, derived from his own experience, that more success might be expected where amputation is resorted to in chronic than in the acute form of tetanus,—a conclusion, indeed, which our knowledge of the disease would have led us to anticipate. "*Sublatâ causâ tollitur* *effectus*" is an aphorism which is unfortunately inapplicable to the subject under consideration.

The causes and treatment of non-union of fractures have a section appropriated to them by our author. The remedial measures at our command are classed by him under the four following heads: " 1, To secure for a sufficiently protracted period entire fixedness of the limb; 2, to procure approximation of the fragments by compression; 3, to excite the vital energy of the seat of injury by local means; and 4, to act, according to the particular circumstances of the case, through the medium of the system generally." We shall not dwell at length on this subject, as we shall devote a short space, in another part of our present Number, to its consideration.* We may, however, remark, in passing, that we coincide with Mr. Liston and other surgeons of experience, who have recommended the seton in long-standing cases of non-union, in deprecating the practice of leaving the threads in for more than ten days or a fortnight; if this do not induce the desired effect, we have little faith in a more protracted trial, to say nothing of the mischief which is likely to ensue on the adoption of such practice. The results which are brought out by a statistical review of cases thus treated, are confirmatory of this opinion. Excision of the fractured ends of a broken bone we regard as a much more serious operation,—one, in fact, which, except in the fore-arm, is scarcely to be recommended: in the case of the humerus the operation is tedious and not free from risk, and by no means certain in its effects,—an observation which possesses still more force when the femur is the seat of injury. We shall conclude our notice of this subject, and, indeed, of the first division of our author's work, by a quotation of his own opinions on the treatment of false joints, resulting from ununited fracture; premising that these opinions are founded on the assumption that it is on the soft parts we have to rely for the production of callus. We shall not wait to discuss whether his data are correct; in practice his conclusions, we think, are on the whole sound.

> "It is my opinion that we ought to act upon the soft parts especially; and, in following the example of Nature, we must, above all, strive to excite inflammation of an adhesive kind. Stimulating lotions are too insignificant; blisters are more active; but sinapisms, which produce an irritation at once more prompt, intense, and deep, without a suppurative action, which is undesirable, appear to me to merit preference. Rubbing together of the fractured ends of the bone acts only by irritating the soft parts; and the same effect is produced by sudden and violent movements of a fracture, with a view to burst asunder the fibrous bands which connect the fragments together, as well as by direct and transient compression, by which the surrounding muscles are contused. Lastly, acupuncture is a resource without risk, the efficacy of which is enhanced by the aid of electricity, of which the needles may be made the conductors.
>
> "Thus, then, the primary object should be to excite adhesive inflammation; and, when all the means at our command of producing it have failed, then, and not till then, we must endeavour to promote suppuration. Here, however, the first question which presents itself is, whether we ought, as some partisans of the method by seton have recommended, to persevere in the employment of this irritating agent for months together, or to limit its use, as advised by Liston, to eight or ten days? To attempt to establish as a general rule such long persistence in the

---

* In our review of the Transactions of the Medical and Chirurgical Society.

use of the seton, is an absurdity; for what is more likely to delay and interfere with the consolidation of a compound fracture, than protracted suppuration? But, on the other hand, the limit fixed by Liston is likewise too arbitrary: the true rule is, to persevere until vascular fulness (engorgement) of the soft parts is established. Eight days will suffice to produce this effect in some instances; whereas, in others, fifteen or twenty days, or even a longer time, are necessary. The study of facts, as presented to us in practice, has already led us to the same conclusion; and we have seen the seton miscarry both from too early removal, and from its presence being tolerated too long." (p. 321-2.)

It is not without premeditation that we have devoted so much space to the first division of M. Malgaigne's work; for the general principles which should guide the surgeon in the treatment of disease or injury, seem naturally to afford a fairer scope for judging of an author's merits, and of the degree of reliance we may place on his recommendations in the treatment of individual cases. We have, therefore, the smaller space to devote to the second great section of the work, which comprises the nature and treatment of the fractures to which the different bones are liable. We cannot, however, in justice to our author, entirely pass by, without notice, this essential part of his volume; but we must content ourselves with the selection of two or three chapters, which will afford us material for illustrating the practice of M. Malgaigne in the treatment of fractures. Those on injuries of the sternum, clavicle, and fore-arm will suffice for this purpose.

The rarity of fractures of the *Sternum* has induced us to select the chapter which treats of them, to observe if our author can throw any fresh light on their causes, nature, and treatment. He states that but one instance has presented itself in the course of eleven years at the Hôtel-Dieu; which, considering the vast number of injuries admitted into this institution, appears remarkable. These fractures are usually transverse, and not by any means necessarily attended with deformity. M. Malgaigne refers to but one case of longitudinal fracture of this bone, recorded by Barrau, and which was cured, though it is not stated whether any deformity remained. Our author relates another case of singular comminuted fracture which terminated fatally, the nature of the injury being masked during life by the amount of consequent swelling and the complicated nature of the injury. Direct or indirect causes may produce this injury: our author adds muscular action as a third agent in the production of fracture of the sternum: we must say we think this very questionable, save in diseased, and consequently friable, bones. Two cases have come under our own notice and care, illustrative of the more ordinary causes. In one, the subject of the injury fell from a considerable height, and fractured several ribs, as well as the sternum transversely. In the other case, the fracture was produced by the patient falling a short distance with a sack of flour on his back, by which the trunk was bowed suddenly and violently forwards. In the former of these, the age of the patient, and the complicated nature of the injury (involving internal lesion), caused death; in the latter, the patient recovered without a bad symptom, though the fracture allowed of the distinct movement of one fragment on the other; there was no displacement productive of deformity in either case. M. Malgaigne narrates several cases, recorded by different authors, of transverse fractures produced by falls on the back. Chaussier, he adds, has seen two instances in which fracture of the sternum occurred during labour, the patients bending themselves forcibly backwards during the labour pains, and resting on the arms and heels. A similar effect followed a similar act (as regards position) in a mountebank, who was exhibiting the feat of curving the body backwards, whilst he raised a heavy weight with his teeth and arms. These, however, cannot fairly be placed in the category of fractures by muscular action, though, indirectly, they were thus produced. The only genuine case of this last-mentioned form of fracture, noticed by M. Malgaigne, is one recorded in the 'Gazette des Hôpitaux' for 1830. The subject of it was the victim of scirrhus of the stomach, which occasioned frequent and violent fits of vomiting. After death the sternum was found to be fractured in its upper third; the presumed agent was the diaphragm; and the reporter states, that though "the bone appeared to be diseased an inch below the lesion, it was healthy at the seat of fracture itself." (?) The prognosis in cases of fracture of the sternum is usually favorable, save where this injury is accompanied by other complications, or the bone is driven in, so as to press upon or mutilate the viscera beneath it. Where the latter occurrence is met with, it may be imperatively necessary to raise the depressed fragment, for which M. Malgaigne considers it justifiable to make an incision through the integuments, and introduce an elevator; but, he adds, "two circumstances ought to put the surgeon much on his guard, not only with respect to operations of this kind, but even in pressing too far the attempts at reduction by ordinary methods; the first is the danger of suppuration, and the second, the difficulty of retaining the fragments in their proper relation when reduced." (p. 459.) Fortunately these serious cases are of extreme rarity; and certainly we ought not to subject our patients to so serious an operation as that first noticed, for the replacement of the depressed fragment, unless other accessory symptoms render it imperative. Petit and Boyer recommended that the sternum should be trepanned where blood is extravasated beneath it; but, as our author observes, this practice scarcely requires a serious refutation; though he admits the propriety of such an operation, where pus in any quantity has collected behind the bone, and the latter is carious.

Fractures of the *Clavicle* are as frequent, as those of its neighbour, the sternum, are rare. The statistical materials at the command of M. Malgaigne show that not more than one fourth of the cases which came under his observation, during eleven years, at the Hôtel Dieu, occurred in females; at least this proportion continued valid between the ages of fifteen and sixty-five, after which period the preponderance was in favour of the female sex. The classification of fractures of the clavicle, adopted by our author, is that which is usually employed, viz. those which affect the shaft of the bone, or either extremity. We turn to the treatment of these fractures to notice whether there is anything particular in the methods he recommends. When the body of the clavicle is broken, M. Malgaigne considers that the following indications should be fulfilled, and in the following ways: First, to elevate the external fragment; this he thinks is best accomplished by an ordinary sling, including the fore-arm and the elbow. Secondly, to carry backwards the external fragment. For this purpose various forms of apparatus have been constructed and recommended, and these are all noticed by our author without particular preference being given to any. A most important indication it certainly is, and may be fulfilled by the employment of the ring pads and straps attached to them, which buckle between the shoulders. If this apparatus cannot be obtained, a broad bandage carefully and properly applied answers the purpose, in our experience, remarkably well; it should also be made subservient to keeping the arm of the affected side fixed to the trunk. The third indication is to carry the external fragment outwards; a pad in the axilla and the approximation of the elbow to the side, are the means to be resorted to. Our author speaks of various forms of pads for this purpose, but does not give the preference to any particular one; we have never seen the air-cushion employed, as suggested by M. A. Ricord, but should be disposed to think it applicable to some cases, where the patients are moderately tractable. The fourth and fifth requirements are to depress the sternal fragment, and to keep the parts at rest. Of the former of these indications we do not recognise the importance, save in exceptional cases, and then we have little faith in the contrivances proposed to accomplish it; we should place more reliance on relaxing the sterno-mastoid muscle, where such deformity existed, after the adoption and failure of the measures recommended to fulfil the other requirements.

As to rest, that is of course imperative; and we do not consider with M. Malgaigne, that any especial apparatus is essential for this purpose; but we do know, from experience, that constant observation even of the most tractable patients is requisite, in order to the readjustment of whatever mechanical arrangements are resorted to in the treatment of these fractures.

The rarity of fractures of the sternal extremity of the clavicle, and the comparative frequency of those of its acromial end, are admitted by M. Malgaigne. Of fractures of both clavicles together, he says he has seen but one example, and been able to collect but four more in the records of surgery. We can add a fifth, which occurred under our own observation a few years since. The subject of the injury was standing against a wall on the approach of a waggon; the space was so narrow that the wheel caught his shoulder, and he was thus rolled round between the vehicle and the wall. Both of the clavicles suffered comminuted fracture, and one was broken into many fragments. The deformity was naturally considerable from the loss of support to the scapulæ; but, the patient being particularly docile, the cure was by no means protracted, and in every respect satisfactory. It has appeared to us, in the treatment of comminuted fractures of the clavicle, that its power of reparation exceeds that of most of the long bones, small spicula becoming more readily united to the main fragments during the process of consolidation: compound fracture, and exfoliation after comminuted fracture, are certainly very rare.

Of the fractures to which the *Fore-arm* is subject, our author states, as the result of his experience, that those of the radius are the most common, and those of the ulna, alone, more rare than such as involve both bones: we believe that most surgeons would agree with him in this statement; and further, that a large proportion of all of these accidents occur in the male sex. After the age of forty-five, however, the disparity disappears in cases in which both bones are involved in the injury; and M. Malgaigne's tables further show a remarkable preponderance on the part of the female sex, where the radius alone is broken, the relative numbers being twenty in men and forty-one in women. We do not stop to speculate on this curious phenomenon.

Fracture of both bones is usually the consequence of direct injury, though a fall on the hand may, and sometimes does, produce this lesion. Our author has met with one instance in which the double fracture in question was caused by muscular action: a robust man, of thirty-eight, was in the act of raising a spade loaded with earth, when he heard two distinct snaps in the fore-arm, and both bones were found to be broken, though not opposite the same spot. This, we apprehend, is a unique case.

We turn to that subdivision of the present chapter which treats of fracture of the lower extremity of the radius, to notice what plan of treatment our author recommends; but we may remark, in passing, that he attributes the usual tumefaction observed on the anterior aspect of the radius, when broken low down, to extravasation of blood in the first instance, and to the product or inflammatory action which subsequently ensues in the cellular tissue, and in the sheaths of the tendons around the wrist. Sir A. Cooper was disposed to ascribe it rather to the displacement of the fractured ends of the bone; but we think there is much truth in M. Malgaigne's suggestion. Our author further speaks of the extreme facility of detecting these fractures close to the wrist; we must acknowledge we have been occasionally a little puzzled when the cases have not come under our observation at an early period, for the consequent swelling is generally very considerable.

But respecting the treatment; the indications to be here fulfilled, are enumerated under three heads, viz., "to preserve the interosseous space, to correct the displacement of the lower fragment backwards (or forwards where such is the case), and to prevent the abduction of the hand, by keeping the styloid process of the radius on its proper level." The first of these requirements M. Malgaigne admits is rather imaginary than real, inasmuch as the interosseous space can scarcely be said to exist so close to the wrist, and the displacement inwards is, under any circumstances, so trifling as to be undeserving of any especial attention. To accomplish the second object, our author employs the following plan; we quote his words—

> "I prepare two pads of three fingers' length, and in breadth equivalent to that of the fore-arm; I dispose one across the anterior fragment, beyond which it must not extend, and the other on the posterior fragment, taking care that it does not descend so far as to interfere with the usual flexure of the hand backwards. I then apply a splint on either surface of the fore-arm, which presses firmly on its pads, but without reaching below the wrist, the front one alone being allowed to extend a little way below the corresponding pad, which latter keeps it sufficiently removed from the wrist to prevent its doing any mischief; and this trifling prolongation is essential to ensure the pressure of the back splint. The whole is kept in position by a roller, which must extend all along the splints, and this may be stiffened with albumen if necessary, or a few strips of adhesive plaster may be substituted. Lastly, the arm is to be supported in a sling in an intermediate position,* that is, resting on its ulnar border." (pp. 613-14.)

The third indication is the most important, as well as generally the most difficult to achieve. Mr. Cline first proposed a method which has been largely and successfully adopted, viz., by the arrangement of the splints so as to allow the hand to drop towards the ulna, by which means the tendency to abduction is counteracted, and the upper extremity of the lower fragment is tilted outwards, and kept separate from the ulna.

---

* Between pronation and supination.

Dupuytren employed a curved steel splint, which he applied on the ulnar side of the fore-arm, so as to act by its elasticity on the hand and lower extremity of the radius, and thus produce a state of forced adduction by which the tendency to deformity is corrected; but this and similar forms of apparatus must be irksome, if not distressing, to the patient. Our author prefers the employment of two lateral splints, acting in the same way and on the same principle as those placed anteriorly and posteriorly; that on the radial side pressing the inferior fragment (through the medium of a pad) inwards, the ulnar splint at the same time pressing this bone outwards, whilst the hand is permitted to drop according to the practice suggested by Cline, and adopted by Sir A. Cooper. M. Malgaigne examines the seat of fracture and reapplies his apparatus about the eighteenth or twentieth day, and then leaves it till the thirtieth day, after which time the limb is set at liberty. This plan of treatment he has found both successful and unattended by any drawback during the progress of the cure, or by subsequent deformity.

We do not find that any notice is taken of the simple but ingenious method recommended by Professor Fenger of Copenhagen, who keeps his patients recumbent, and with the injured member resting on a plane which is inclined upwards from the elbow to the wrist, whilst the hand is allowed to drop over the most elevated part of this inclined plane, the arm being only so far confined as to prevent its being shifted from its position,—a precaution which is unnecessary if the patient will keep entirely at rest. The Professor appears to have been peculiarly happy in the treatment of these troublesome fractures of the radius close to the wrist, by pursuing this simple method.

We trust it is unnecessary to pursue our analysis of this the second part of our author's work further, to satisfy our readers of its systematic and eminently practical features. We have but few words to say in conclusion. M. Malgaigne has availed himself largely and judiciously of the labours of others in the accomplishment of his task, at the same time that he has brought the results of his own experience and observations to bear on every part of his subject. We are fain to place reliance on what he states, and to have confidence in his inferences; for he is eminently cautious in weighing and analysing the opinions and statements of others; and frequently exposes the illogical conclusions drawn from false or insufficient data. The accompanying Atlas contains a number of illustrative lithographs, which are very nicely executed, and are calculated to enhance greatly the value of the letter-press.

In fine, we feel that we can with pleasure and satisfaction recommend the volume before us as a safe guide to the surgical student and young practitioner; at the same time that it merits to be classed with those systematic and practical works, which enable us to form a just estimate of the existing state of those branches of operative medicine which they profess to discuss.

Art. IX.

1. *Lectures on the Diseases of Infancy and Childhood.* By Charles West, m.d., Senior Physician to the Royal Infirmary for Children, &c. —*London.* 8vo, pp. 488.
2. *A Practical Treatise on the Diseases of Children.* By J. Forsyth Meigs, m.d., Lecturer on the Diseases of Children in the Philadelphia Medical Association, &c.—*Philadelphia.* Small 8vo, pp. 575.
3. *Essays on the Theory of Convulsive Diseases.* Essay I. *On the Convulsive Affections of Infants, and especially Laryngismus.* By Marshall Hall, m.d., f.r.s., &c.—*London.* 8vo, pp. 38.
4. *Monograph; On the Cerebral Diseases of Children.* By Walter C. Dendy, Surgeon to the Royal Infirmary for Children, &c.—*London.* 8vo, pp. 42.
5. *Reports of the General and Medical Committees of the Royal Infirmary for Children, Waterloo Bridge Road.—London,* 1847.
6. *Bibliothèque du Médecin Praticien.—Maladie des Enfants de la Naissance à la Puberté (Médecine et Chirurgie).* Sous la Direction du Docteur Fabre.—*Paris.* 2 vols. 8vo, pp. 684, 674.

We believe that, from those who are the best able to give judgment upon the matter, we should derive the confession, that in this country—so far as the *general body* of practitioners of all grades is concerned,—there exists not a class of diseases, relative to whose pathology and treatment less exact information is possessed by it, than that which includes those more particularly affecting infants and children. But that the practice of an every-day routinism of therapeutic interference is sufficiently and widely enough exerted, there can be no doubt whatever; and it is remarkable to observe the often apparent *nonchalance* with which the treatment of the most serious affections is undertaken, and the impression which appears to exist as to the facility and safety with which, as it is termed, children may be "doctored." But not only—as a general rule—is there a most sad and grievous want existing in the ranks of the profession, as to clear and precise views of the nature and pathology of the disorders of early life, and consequently often a recklessness as to their management, but, as a consequence of this assumed facile acquirement of their knowledge and treatment, the latter is made to constitute one of the more lucrative branches of "counter-practice"—a practice carried on by men who cannot properly be supposed to possess an *iota* of the necessary attainment. And not only do medical practitioners and druggists treat lightly these peculiar and frequently eventful affections, but nurses, old women, and "skillie wives" enter the arena of medical warfare boldly on their behalf, and often claim a sort of right, by hereditary descent, to undertake their management. Of course the power thus vested in the hands of so many, to treat disorders of a peculiar character, proceeds in part, at least, from a like belief in such facility of their comprehension on the part of the parent and the public; and we are not stating an untruth, we believe, when we say, that there exists many a father who considers a particular medical man as "all very well for the children," though he refuses to trust his own case in his hands. Frequently, too, we have heard mothers assert that they send for Mr. So-and-so, if the servants and children are ill; but if they themselves or their husbands are indisposed, they employ another practitioner.

Almost universally as this capability is imagined to exist, of "doctoring" children, yet, as in everything else, we now and then hear of some particular individual who is famous in this department or *specialité*. This is not so frequently the case as respects the legitimate, as it is with regard to the illegitimate practitioner. We constantly have patients brought to us, who, we are informed, were at first taken to a particular counter-prescriber, because he is said to be "so famous for children." And what, alas! is, or can be, the result of all such procedure? Can the insidious approach of pneumonia or pericarditis be diagnosed by simply looking at the child in its bed or cradle, or can such diseases be treated by an aperient powder or syrup of squills? Can auscultatory signs be discovered, when practitioner and patient are carrying on a *consultation* on opposite sides of a counter? Are the premonitory symptoms of tubercular meningitis, or the secondary affections of dentition and diarrhœa, so easy to be seized that all who run may read them? Is entero-colitis to be treated with syrup of buckthorn, or pulmonary tuberculosis with a "cooling mixture"?

That the study and management of children's diseases are quite the reverse of an almost intuitive or easy acquirement, Dr. West rightly impresses on the student at the commencement of his Lectures; and offers as reasons for adding another to the already numerous courses they are called upon to attend, the frequency, fatality, and the many peculiarities of these diseases. Speaking of the periods of the first and second dentition, he says:

> "Disease is then frequent and serious beyond what it is at other times; and every ailment then warrants a double measure of anxiety.
>
> "Your old means of investigating disease will here to a great degree fail you; and you will feel almost as if you had to learn your alphabet again, or as if, entering a new country, whose inhabitants you expected to find speaking the same language and having the same manners as the people in the land you had lately left, you were to hear around you everywhere the sounds of a foreign tongue, and to observe manners and customs such as you had never seen before.
>
> "You cannot question your patient; or, if old enough to speak, still, through fear or from comprehending you but imperfectly, he will probably give you an incorrect reply. You try to gather information from the expression of his countenance, but the child is fretful and will not bear to be looked at; you endeavour to feel his pulse, he struggles in alarm; you try to auscultate his chest, and he breaks out into a violent fit of crying.
>
> "There is, moreover, a certain tact necessary for successfully investigating the diseases of children." (p. 2.)

Great as is the error, then, of not considering these diseases as necessitating special study, and, as a consequence, often dire the result, yet there exists quite an opposite view of them in the minds of some; but which, nevertheless, from peculiar circumstances, is causative of like effects. It is freely admitted by those persons, that the affections of childhood do really require particular study and attention; but, in attempting to bestow such, they meet with difficulties which dishearten them and make them abandon the duty in despair. As Dr. West remarks,—

> "Some practitioners never surmount these difficulties, and the diseases are consequently a sealed book to them. After a time they grow satisfied with their ignorance, and will then with the greatest gravity assure you that the attempt to understand these affections is useless. They have fallen into this unfortunate error from not taking the pains to start aright." (p. 3.)

That such persons will leave the sick-room little wiser than when they entered it, and may remain convinced of the impossibility of making out children's diseases, is easy to be understood. It would be a serious error, however, to spread abroad the doctrine, that none, save those especially trained by their professional studies, can be conscious of any of the signs and evidences of infantile disorder. On the contrary, from the peculiar relationship of the mother to her child,—ever watchful, ever observant of the physiognomy, habitude, state and condition of her offspring,—we must be careful, as Dr. West remarks, not to underrate the value of the information she can communicate, or even the opinion she may form. The peculiarities of facial expression, of the coloration of the face and integuments, of the expression of the eyes, of the gesture and attitude, and of the cry, are all so many invaluable aids in the diagnosis of infantile maladies. And of these the nurse and the mother can give us most useful information, which often could be gleaned but very partially by ourselves. We believe M. Jadelot and his commentator, M. Eusèbe de Salle, to have declared, that the form and face of children at the breast present no traits sufficiently permanent to allow of our drawing from them available deductions; though, according to M. Jadelot, from the epoch of the first dentition up to puberty, we may seek with advantage some principles of physiognomic semeiology. But we entirely differ from the first assertion, and we consider that M. Bouchut, in his 'Manuel Pratique des Maladies des Nouveaux Nés,' has shown its incorrectness. It may be true that the early aptitude of judging of these traits will very much depend upon a peculiar ability so to do, with which some persons have been richly endowed by Nature; but habit and experience will at length carry others quite as far upon the right path, as trodden by their more fortunate brethren.

Passing by without further comment those reasons which so often tend to the surrender of the treatment of the sick child into the hands of pretenders, we shall inquire into some of those which lie at the root of the other great evil,—the general insufficiency of right information concerning the disorders in question amongst the ranks of the profession. To the junior practitioner we would say, in the words of Dr. West,—"Children will form at least a third of all your patients; and so serious are their diseases, that one child in five dies within a year after birth, and one in three before the completion of the fifth year:" and in those of the Report of the Medical Committee of the Infirmary,—that is "not, however, solely on the high rate of mortality in infancy, that we desire to insist, nor is its diminution our only object; there are many diseases incidental to childhood, which, though they do not directly tend to shorten life, yet render that life one of suffering," and which, if uncured, will conduct through a sickly infancy to a feeble manhood. Under these circumstances, the inquiry we propose is not without moment.

If the facts which we shall state be true, and we believe them to be, there can be no doubt but that the grand cause of the insufficiency we have affirmed to exist, lies in the want of proper teaching during the pupilage of the medical practitioner, and in the non-existence of some of the more necessary things by which alone that teaching could be properly effected. The great capital of this empire stands, in comparison with the continent, nearly alone in the want of an hospital for sick children, or of any special provision—with one partial exception only, so far as we are aware—for the regular admission of young patients into the general hospitals for adults. The exception to be made is in relation to the great establishment of Guy's; in which some beds are so appropriated, and are under the supervision, we believe, of Dr. Golding Bird. This is sufficient to render the future medical man's education in a great and essential point incomplete; inasmuch as that education should embrace, as one of its more important elements, a special drilling, if we may so call it, in the pathology and treatment of children's diseases. To carry out satisfactorily this branch of education, to be able to effect it by the assistance of proper teachers, it is necessary that there should be schools in which these teachers may perfectly, though gradually, educate themselves; and what can these schools be, but such hospitals as we have mentioned? It is not alone sufficient that we have here and there an asylum for orphans,—for blind, deaf, and dumb children,—and the like; these for their own purpose are all well enough, but for ours apparently next door to useless. They are not hospitals for *sick* children; some do not receive children at an early age; and none, so far as we are aware, are made available in the least possible way for the instruction of the profession, so far, at least, as the *general* maladies of children are concerned. We believe still, that they might be rendered subservient to this purpose more effectually than they are; but yet they would be incapable of effecting most of what we desire to see accomplished. *We* need an Hôpital des Enfans Malades in the metropolis, near the schools of medicine; we need men of admitted ability, professional, scientific, and literary, able to stand side by side with the officers of the hospitals for adults, prosecuting the study of a special pathology and treatment in its wards, and hence able and willing to impart the results of their experience to the rising generation. We are sufficiently aware that objections have been raised by good authorities against the presumed value of the continental hospitals, so far, at least, as they relate to the benefit of the little patients. Their high mortality is undoubted; and the tendency of many of the diseases occurring in and received into them, to run a complicated course, is frankly to be owned, as also that it is almost alone in the atmosphere of sick and foundling hospitals, that we find such diseases as *muguet, infantile erysipelas, sclerema, infantile acute peritonitis,* &c., very frequent and running a very fatal course. But without doubt some of the chief causes of inconvenience, &c., mentioned by M. Barrier, and commented on by his reviewer in one of our predecessors (Traité Pratique des Maladies de l'Enfance, reviewed in Brit. and For. Med. Rev., Vol. XV), are capable of being remedied, or of being entirely prevented, if we were to establish a child's hospital among ourselves;—such, for instance, as the allowing of an extreme cold being felt in winter, and the breathing of a close, ill-ventilated atmosphere in the summer season; and, if it should be found that the stinging and worry of gnats and flies infesting the wards, "keep the children in a state of constant irritation, and operate very unfavorably upon the patients," we believe that, by the aid of gauze or net thrown lightly over the face and limbs, and of fly-poison paper placed (as it might be) beyond the reach of the little invalids, these evils might soon be rendered nugatory or tantamount to nothing. One great evil, it seems, at the Hôpital des Enfans Malades, is, or was, a deficiency of nurses and attendants; this is one certainly not *necessarily* attendant upon, or naturally involved in, the administration of hospitals for children. It is simply a question of forethought and money. The importation of sweetmeats and pastry by the parents and friends could be also just as much under the control of the inspecting nurse or porter, as is that of gin and whiskey by the visitors to our hospitals for adult patients; and, with regard to the supposed impossibility of varying the diet, so as to suit the caprices and likings of the inmates, we see no greater difficulty than may be supposed to exist relative to the same endeavours at the children's own homes. Of the propriety of having a distinct ward for the eruptive fevers—unless the hospital was very large and built in a peculiar manner, so that all direct communication could be cut off between the fever ward and the others—we have great doubts. When all those labouring under such affections are concentrated together in a limited space, such an intense focus, from which zymotic poisoning may proceed, will be generated, that we believe more harm would be done, than by scattering the cases of such diseases generally through the hospital. To assert, as some have done, that the zymotic particles only act within a few feet of their source, is contrary to our experience and the general facts connected with the epidemic visitations of these maladies. The more intense the focus of poison, the more extensive will be its spread; but, by the diffusion of it through many wards, its dilution will be effected, and, in our opinion, less harm from infection will result. Of course there is a limit to the impression of zymosis, in relation to the distance of the source of the zymotic material; but not, in our opinion, within the range of an hospital, where fever patients are concentrated together in a single ward. And further, we are sure that the concentration of such patients would tend to render the exanthems, under which they labour, of a more severe or typhoid type; hence the fatality in such a ward would be greatly increased, and a more malignant disorder would be likely to be impressed upon the other patients. We do not deny that, upon our principle, children admitted for other affections might be frequently attacked by exanthematous disorders; or that, when so attacked, the chance of the favorable progression of the malady is less than when the eruptive fever is the primary malady. Nor would we maintain that an hospital is exactly the place we would choose for the treatment of these diseases, *if* we could arrange everything as we desired at the houses of the sick poor. But we know, that, at present, we can effect no such thing; and we think, moreover, we never shall be able to do so. We therefore maintain, that, *if* these infectious diseases are to be admitted into a general hospital, less harm would result from the adoption of our own method, than from putting in force the other; unless, perhaps, as we before remarked, such peculiarities in the building are enforced, which will dissever such fever wards from the more general ones of the hospital. It is but fair that we should recommend our readers to re-peruse the Report of Mr. Farr, on the Causes of the High Mortality in Town Districts, in the Registrar-General's Fifth Annual Report; in which some views on the value of hospitals generally are advanced, contrary to those we ourselves hold. We entirely concur, however, in the remark of Mr. Grainger, in his late admirable Report concerning the Tooting catastrophe (see Times, Jan. 13, 1849), in which he states, that—

> "It is one of the best ascertained facts in the construction of hospitals, that wards, containing a large number of patients in each, as seen so often in continental cities, exert a most prejudicial influence over the patients lodged within them, and never can be rendered so healthy as smaller wards. The same principle strictly applies to the case of apartments designed for children, especially those of a tender age."

We do not doubt either the value which would accrue, *could* we form hospitals on the plan recommended by Dr. Simpson, in his Report of the Maternity Charity of Edinburgh. He advises that our sick institutions should be changed from "crowded palaces, with a layer of sick in each flat, into villages or cottages, with one or, at most, two patients in each room." Such a village is also to be built of iron, instead of brick or stone, so that it could be taken down or rebuilt every few years. But how such a *village* could effect in many ways, that which Guy's, Bartholomew's, the North London, and the Westminster Lying-in Hospitals, do for such a metropolis as our own, we cannot discover.

Admitting, however, that there may be unfavorable circumstances, arising from the assembling of children in the wards of hospitals, yet, with all its attendant evils, we believe that it would be accompanied with so many positive advantages to the public and the profession, as to make us earnestly desire to see institutions for sick children in as full vigour here, as they are known to be abroad. That some few earnest and laborious men may reap a rich harvest in performing the arduous office of *visiting* the young patients of a dispensary at their own homes, over a wide district, regardless of time, trouble, and weather, is sufficiently proved by the admirable work of Dr. West, at the head of this article. That many such *will* be found, however, we do not believe will be the case; that they *have* been, we deny for certain. But, beyond the mere education—if we may so term it—of such a single man, and that which he is able to effect in others through his most valuable production, we have to look at that of the *many*,—at that, too, which is of a kind that no book, however perfect, save of Nature herself, can ever inculcate. It is from this book that we must impress many minds at the same time, and in the most convenient method; and how is this to be done as respects the teaching of the science and art of medicine, save with that open book of Nature—an hospital? An hospital with its formal school,—for without *form* no teaching can be effected,—its lecture theatre, its post-mortem examinations, its daily clinical visits, and its operations, &c. &c. Whilst, to the *ripening* of such a man as Dr. West, an hospital, with such collaterals, may not be necessary, to such as may be his pupils, for their mere *budding*, we conceive that they are essentially so.

It might be thought, that, deprived as we are in this metropolis of thus *effectually* teaching the nature, &c., of children's diseases, yet that some kind of instruction—say, by mere lectures—would notwithstanding be afforded to the student and junior practitioner. But no; until a few months back, nothing of the kind. It is true that, for season after season, the school prospectuses have wound up their announcement of the lectures on obstetrics with a promise of some also on the "diseases of women and children;" and these, too, often promised, be it remembered, in a three months' course of the former. Now we believe that, except as relates to the more immediate treatment of the child after the separation of it from the placenta, or when the meconium does not pass quickly, and analogous circumstances, the lectures on the diseases of children have not even consisted in the *vox*, but have sunk gently down into the *nihil;* and we can imagine the smiles of our friends, MM. Trousseau, Rilliet, Barthez, Bouchut, &c., as well as of Dr. West, when they thought of how we managed this important branch of education for general practice. But it seems to have been too serious a matter with our countryman thus to let matters stand; for he saw that—

> "Something more is essential to the successful treatment of children's diseases, than to watch their advances carefully, and to adapt the strength and doses of your remedies to the tender years of your patients. It is not mere hyperbole to say, that you have to study a new semeiology, to learn a new pathology and new therapeutics—matters of such importance cannot properly be examined at the end of a course of lectures on midwifery. I have therefore preferred making them the subjects of separate consideration." (p. 2.)

As to what further instruction about children's diseases has usually been obtained from the lectures on general medicine,—with the exception of those on the exanthemata,—we leave those to judge who have attended them; we believe, however, that they will agree with us in thinking it has been just so much "leather and prunella;" and, considering how much has therein to be taught, we do not find room for the admission of anything further.

It may be asked of us, if we really intend that students should be obliged to attend a special course of instruction upon the subject in question. If so, we answer, most undoubtedly; and consider that Dr. West did great service in offering to the students of the Middlesex Hospital a voluntary course upon it. Have they not, it may be replied, enough to do already? Quite, would we answer; but we would add, chain them down less to the minutiæ of chemistry and botany, and the least profitable portions of anatomy, and let them go instead to the bedsides of sick children. For the mass of them this will be more available than the analysis of an ore, or the diagnosis of a natural family, or than the knowledge of the distribution of a small artery or nerve in an unimportant region.

It may be urged, too, against us, that so far, at least, as the "teachers" are concerned, we are tending to make another subordinate branch of medicine, creating a new *specialité*, and furthering the increase of what Dr. Copland might call a fussy practice. But, be that as it may, we cannot shut our eyes to the fact, that there are departments of our art which are much benefited by a special attention being paid to them (Dr. West's exertions prove it), and a constant practice so devoted; and it is from men who thus attend and practise, that the necessary instruction is to be sought for by others.

It would be unjust and untruthful to permit our animadversions to remain on record, without acknowledging how much we are indebted to the labours of some amongst us, relative to the pathology and treatment of the diseases under consideration: it will be sufficient to indicate Hennis Green, to show that we are not unmindful of them. But, whatever service they have effected in this department of medicine, we may affirm, that papers, however valuable, here and there scattered through the 'Medico-Chirurgical Transactions,' the 'Dublin Journal,' and other periodicals, are not, and cannot be, the source of that education which is necessary for the student in an important branch of medicine. We believe, then, that in what we have said we have not gone beyond the truth, and we are sure that other minds have thought as we think; in proof of this we find Dr. West prompted to deliver a course of lectures at the Middlesex Hospital, and publish them in the 'Gazette,' and Dr. Willshire, his colleague at the Infirmary, to publish a series in the 'Medical Times.'

We remember noticing, during the publication of Dr. West's lectures in the 'Medical Gazette,' that immediately following one of them (Lect. X, p. 445) was an extract from the Essay of Bishop Wilberforce, on the 'Discovery of Truth in Science,' the spirit of which seemed to us to be a re-echo of the voice of that which vivified the preceding pages of the volume, bearing the results of the exertions of Dr. Charles West:

> "For the discovery of truth," says the Bishop, "it is needed that the facts of nature should be questioned by man's intelligence. For this questioning the first of all conditions is, that he should have these facts clear, defined, separated from others, ascertained in themselves. That he should so have studied them as to know their true relations, to see through seeming resemblances, to catch the scattered hints which declare, in the midst of apparent dissimilarity, real connexion; to see the value of a fact which, having been arbitrarily thrust from its true place, has seemed hitherto a perplexing superfluity; that he should thus have, plain and clear before him, the elements of which the insight of his highest reason is to suggest to him the law."

Knowing that Dr. West had held the office of physician to the Royal Infirmary for Children for several years, that he had been devoting in an arduous manner his best exertions to the fulfilment of his duties, prosecuting the investigation of the pathology and treatment of a special class of diseases on an extensive scale, and with a determination not directed to the like object by any other physician, and gratified and instructed, as we ourselves had often been, by the valuable Reports and Papers occasionally communicated by him to the 'British and Foreign Medical Review' and 'Medical Gazette,' we were satisfied that he had been able, so far as it was possible, to procure his "facts clear, defined, separated from others, ascertained in themselves." As a consequence we looked forward to possess, when some time ago he promised to bring the results of his labours systematically before the public, such a work as we believed no other Englishman could have produced. Those who, like ourselves, have now become familiar with it, must freely coincide in our judgment, that most worthily has he exemplified Bishop Wilberforce's axioms, that he has well studied the signs and symptoms of a highly important class of maladies in relation to their morbid anatomy; that he has been able to seize those facts which, under a seeming resemblance to an obedience to certain principles of pathology, were, on the contrary, but the exponents of others, and has associated them accordingly; that he has connected many among which apparent dissimilarity existed; and that he has made plain and clear before him the elements out of which he has constructed the edifice, the design and execution of which we have yet more particularly to notice. Thus much he has accomplished, in so far as it is at present possible, in this department of medicine; and if sometimes the torch, when held up, only shows the darkness around, this arises from the source of lumination in our science not as yet being permitted by the Highest Wisdom to be greater.

To those who may not happen to be so well aware as we are, of the advantageous position long held by Dr. West, it may be satisfactory that we quote the following from his preface:

> "These observations were made in the large field presented by the Child's Infirmary, which was first thrown open to me in the year 1839 by the kindness of my friend Dr. Willis, then physician to that institution, to which office I succeeded on his resignation in 1842. Very nearly 14,000 children have thus been brought under my notice during the past nine years; and I have kept accurate notes of the diseases of 600, as well as of the results of 180 dissections of cases in which those diseases terminated fatally." (p. v.)

It may appear to some of our readers that, in thus indicating the valuable fountain from which Dr. West has filled his urn, we somewhat belie our previous statements as to the want of such a source available to the necessities of the student and practitioner. But it is not so; the so-called Infirmary for Children in the Waterloo Bridge Road, is as yet but a mere dispensary; no patients are received into the house, so that all direct *clinical* observations have to be prosecuted by the medical officers at the patients' own homes,—through a dense district, however, containing 70,000 inhabitants.

> "The first institution established for the diseases of childhood, was founded in London, in 1769, by Dr. G. Armstrong, but it did not obtain sufficiently firm hold on the public mind to be continued after his death. In Paris, however, the importance of the subject was soon after acknowledged by the Government; an hospital containing 600 beds, has existed there for nearly fifty years, and there are now similar institutions in almost every capital city on the Continent, whilst in London, so long and justly distinguished for its numerous and valuable charities, the want of an institution for the treatment of the diseases of children was neglected till 1816, when this dispensary was founded by the late Dr. Davis. His scheme included the establishment of an hospital for the treatment of those diseases of childhood which could not be properly attended to at the dwellings of the poor, but from the death of the founder, and of others who took an active share with him in the support of the institution, this philanthropic and important part of the original intention has never been realized.
>
> "Each fact impresses more and more strongly upon the mind of every reflecting person how much is yet to be done, and must also satisfy every intelligent inquirer that, in a great metropolis like London, there ought to be an institution for the treatment and study of the diseases of children in every stage—diseases unknown or rare in adult age, and causing a mortality, the possibility of lessening which cannot fail to secure ample support to this charity. The funds at present at the disposal of the Committee, are scarcely sufficient to defray the expenses of the dispensary." (Report, &c., of Royal Infirmary.)

Valuable as such a *dispensary* may be, as we have said, to such an earnest and hard-working man as Dr. West, it yet cannot fail to be seen how utterly inadequate it is to the great purposes we have already sufficiently dwelt upon. Assiduously and quietly for several years has Dr. West been employing such advantages as were at his command towards results which we believe will be of lasting credit to him, and we praise him the more because he has established for himself a reputation so unobtrusively. We have no doubt but that Dr. West is familiar with the lines in the Torquato Tasso—

"Es bildet ein Talent sich in der Stille
Sich ein Charakter in dem Strom der Welt."

We are willing to bestow upon him the praise of having ably exemplified the truthfulness of the first of them.

In treating of the diseases of children, both Dr. West and Dr. Meigs, junior (upon whose work we shall make some general observations presently), follow the simple, and, in our opinion, most *practical* method of arranging them according to the *systems* which they affect. We do not say this is the most *scientific;* but we think, for the student, it is by far the more *useful* classification. While, however, the latter gentleman plunges at once, without a word of introduction, *in medias res,* and commences with a definition of *coryza,* the former introduces his subject with an admirable lecture on the peculiarities, &c. of infantile affections, and the better methods of examining sick children.

Dr. West then proceeds with the consideration of the diseases of the nervous system; for, "besides the general interest and importance of these affections, at whatever age they may occur, *their extreme frequency in early life,* gives them an additional claim on our notice . . . . though some persons, not being able to account for the fact, have affected to doubt its reality."

Dr. Meigs postpones their consideration until after that of the affections of the respiratory and digestive system, apparently because it is a *wrong,* though "a very common opinion, both in and out of the medical profession, that this class of diseases occasions a much larger number of deaths in childhood than any other."

Dr. West, however, agrees with Dr. Meigs in attributing to these affections a lower rate of mortality than to the disorders of the respiratory organs, which are well known to be the most fatal of all; and Mr. Dendy uses terms very negligently and wrongly, and states not the truth, when he says, "we may term disease of the encephalon the great destroyer of the infantile population." (p. 18.) "It appears," says Dr. West, "that a third of all deaths, under five years of age, are due to the diseases of the respiratory organs, while not above one child in four dies under that age from diseases of the nervous system; and not above one in six from those of the digestive system."[*]

Notwithstanding the greater fatality of the respiratory affections, we agree with Dr. West in thinking, that there exist circumstances which render diseases of the cerebro-spinal axis necessary to be first understood by the student; though we may admit that difficulties often attend their study, and that the truth is sometimes veiled in so much obscurity, that we may altogether fail to perceive it. But without some previous knowledge of these affections, the student will be at a loss to understand many of those sympathetic and secondary phenomena, which in children so constantly arise

---

[*] Table, showing the proportion per cent. of Deaths from different causes in Childhood, in the Metropolis, as compared with subsequent life. (Deduced from the Fifth Report of the Registrar-General.

	Under 1 year.	Between 1 and 3.	Between 3 and 5.	Under 5.	5 to 10.	10 to 15.	At all ages above 15.
From diseases of the nervous system .	32·5	19·5	18·3	25·4	15·8	9·7	9·1
Ditto ditto respiratory system .	28·2	41·	37·2	34·2	30·9	34·1	31·5
Ditto ditto digestive system . .	17·5	13·5	5·2	14·2	6·5	8 3	5·2

in the course of other or coincident diseases, so that he will be frequently confounding cause and effect; and therefore it is, as Mr. Dendy remarks,—

> "The pathology of the infantile encephalon, which is the subject of our most especial interest, replete with sympathies so delicate, sensitive, and impressible, and so obscure in its indications, notwithstanding all which Paisley, Whytt, Quin, Goelis, Cheyne, Braschet, and others have written. It is the seat also of that mind which is to influence the thoughts, passions, and actions of after life, while thus combined with matter, until at its appointed season it will
>
> 'Burst a seraph on the blaze of day.'" (p. 4.)

In his consideration of the *Diseases of the brain and nervous system*, Dr. West first treats of *Convulsions*, then passes through the various forms of *Cerebral congestion* and *Hæmorrhage*, to the *Inflammatory affections*, including the different varieties of *Hydrocephalus and Phlebitis of the sinuses of the dura mater. Hypertrophy* and *Atrophy of the brain* next follow, after which come *Hydrocephaloid disease, Tubercle, Hydatids, and Cancer*. Lecture XI is occupied with the investigation of the *Diseases of the spinal cord*, including *Trismus*; and the next one, terminating this division of the course, commences with the interesting subject of *Night terrors*, and terminates with that of *Facial hemiplegia in newborn infants. Epilepsy, Chorea*, and *Paralysis* connect these subjects.

The great frequency of *convulsions*, and their constant association with nearly all forms of maladies in the child, have always rendered them a subject replete with interest to the physiologist and pathologist; and the modern writers of France and Germany, and particularly Dr. Marshall Hall, among ourselves, have taken great pains to investigate their nature, causes, and treatment. But the difficulty of such investigation, the presumed necessity of making many kinds, or varieties, or special forms of the disorder, the peculiar and changing views, held according to the amount of our knowledge possessed at a given time, of the physiology of the nervous centres, and of their connexion and sympathy with the derangements of other systems and organs, and though last, not least, the lax and indiscriminate use of terms, have together produced such an undigested mixture of thought and observation, that, in endeavouring to try its value by our own experience, and reduce the *theory* of convulsive action and its causes to some definite form, we meet with obstacles of no mean amount and obstinacy.

After all that has been written upon the subject, we confess, that were it not for the light held up to us by Dr. Hall, we should leave it untouched; but guided by it, we are tempted to offer a few remarks, in which will be interwoven the results of his own teachings, those of other modern writers, and those which have been gleaned from the observations we ourselves have made and the experience we have had.

1. The first proposition we may lay down is that of Flourens, Magendie, and Hall, that no disease of the mere cerebrum, or disorder limited in its effects to that organ, or to the purely cerebral nerves, can induce convulsion; for no irritation confined to these parts can bring about muscular contraction. If disease or disorder, or so-called irritation, commence in these parts, it must afterwards be propagated to, or its results there must affect, the membranes, or the incident or motor nerves, or the medulla oblongata, before convulsions can arise.

Proposition 2. Convulsions, either partial or general, may occur, and during the fit the child may die: yet after death no *visible* alterations, or organic lesions, or anatomical changes, can be discovered in the nervous centres, with which such convulsions can be rightly and satisfactorily associated as an effect; nor has the child been known to have laboured under any affection of other systems or organs, by which the centres may have been more immediately or secondarily affected, or with which they may have *sympathised;* nor have the incident or excitor nerves been known to have been exposed to such irritation, as might have produced the convulsions in the way of reflex actions. It is here then supposed that certain alterations, of a dynamic character, in the nervous matter of the centres, primarily arising there, are the cause of such convulsions, which are therefore of *centric* origin; a cause which is certainly not anatomically definable, but only assumable in its existence.*

Proposition 3. Convulsions, either general or partial, may occur, and the child may die in the fit; but after death no morbid lesional changes are found to exist in the nervous centres, to which we can ascribe the disorder as an effect. We refuse to acknowledge that the cause of the convulsions had its origin in the nervous matter of the centres, or primitively affected it, because we know the child to have been exposed to irritants of the incident or excitor nerves; and we believe that these convulsions were caused by reflex spinal actions, and that the latter were the result of the irritation so produced. We believe, moreover, that the child laboured under no affection of any organ which could alter the proper relations of the vascular system of the brain with the nervous matter, or at least produce such an alteration which proves itself by an abiding and evident change to be witnessed after death. These convulsions are of *excentric* origin; and the only cause of them, down to which we can generalize, is the peculiar irritation to which we believe certain incident or excitor nerves have been subjected.

Proposition 4. A child dies after having been convulsed. After death, more or less evident and important lesional changes and anatomical alterations are found in or about the nervous centres. These we believe sufficient to account for such convulsions, and we trace them to such morbid changes, &c., as their effect. We also affirm them to be caused by disease of the centres, because before their appearance there existed a greater or less amount of so-called "cerebral symptoms." We believe these cerebral symptoms, and consequently the disease of the centres, to have been of a *primary* character, that is to say, to have had their commencement either in the nervous or vascular apparatus contained in the cranium or spinal canal, and not to have followed as a secondary disorder upon a more or less acute affection of any other or more distant organ. These convulsions are of *centric* origin.

Proposition 5. A child dies in convulsions. After death we may, or we may not, perceive such material changes in the centres as we may affirm to have been their immediate cause. If they exist, so far it is satisfactory; if not, we yet believe that *temporary* alterations were produced, either in the relations of the vascular system of the brain, &c. to its nervous matter, or else in the *sympathetic* relationships between the centres and other systems, organs, or structures of the body, by the previous existence of important disease in organs, &c., other than these centres. This we believe, because we know the child to have exhibited, for a greater or shorter space of time, other than "cerebral symptoms;" to have distinctly betrayed the existence of more or less acute disorder of distant parts. We therefore believe that such convulsions, or the disorder in the centres, were not of *primary* character, not the result of disease having its primal origin in the centres themselves; but that they were of *secondary* character, a well-marked and evident disorder of a distant part having at length drawn the brain—not at first disposed in itself to enter—into the general whirlpool of mischief. Such convulsions are yet of *centric* origin.

---

* Vide Bennet on Hydrocephalus, page 143.

We believe that the above arrangement can be made to include all forms of convulsions, spoken of by writers as occurring in children. But we must admit that it is very doubtful how far we may be permitted, as knowledge advances, to preserve all its various divisions. For example, it may be asked if we are *now* justified in making such a class of convulsive actions as we should include in the characters of proposition 2,—a class having only *assumable* endowments. Are we warranted in believing that, in infants and young children, such purely and essentially *neurose* disorder can arise, or that the nervous matter of the centres can assume in itself any such diseased condition?—a condition independent of alterations or changes in its vascular system. It has been affirmed by some later continental writers that, before the period of the first dentition, *essential* convulsions, such primitive neurose disorder as we have admitted, are things next to an impossibility; and Mauthner asserts that, even later in life, it forms the exception to a great rule, when we are called upon to believe in their existence. According to Verson,* "before an organ or system—causes of disease being present—can exhibit the development of a pure and special form of malady, proper to and proceeding from itself, it must be endowed with all those qualities which make it capable of the conflict; it must possess a completely developed organization, and through the latter be in a condition to react against those influences which are affecting it; do we find this to be the case with the *sensitive* sphere of life in the infant? By no means. In it predominates the *vegetative*, and that portion of the nervous system alone which is necessary for the carrying on of its functions. The ganglionic system is, therefore, developed relatively to such necessities."

The comparatively more perfect development of the spinal cord in children, with which the ganglionic system is in such intimate connexion, in relation to that of the brain, is one of the main reasons advanced by some later writers to account, partly at least for the very great frequency of convulsions during infantile life. Dr. West remarks:

> "The grand reason of their frequency is no doubt to be found in *the predominance of the spinal over the cerebral system in early life.* In the adult, the controlling power of the brain checks the display of those reflex movements, which become at once evident if disease heighten the excitability of the spinal cord, or cut off the influence of the brain from the paralysed limb, or even if sleep suspend that influence for a season." (p. 17.)

Mr. F. Barlow has lately drawn particular attention (Medical Gazette, 1848) to the influence of the *relative* state of the brain and spinal cord in permitting spasmodic movements, and has shown how clearly Hunter foreshadowed the more definite reasons which, from the investigations of Marshall Hall and others, we are able to give for it at the present day.

---

* Der Arzt am Krankenbette der Kinder, vol. ii, p. 267, quoted by Mauthner, Krankheiten des Gehirns, &c., p. 357. Vienna, 1844.

"Women, children, and sick men," says the illustrious Hunter, "are subject to fits for the reason just mentioned (the muscles being less at the command of the will). Perhaps, also, diseases of the uterus, for the same reason, are the cause of spasmodic complaints, as being very little at the command of the will, but of other circumstances; and from this disposition it draws in other parts by consent, and so brings on general spasm." Dr. West says, relatively to what we have been speaking of—

> "In proportion as the brain increases in size, and its structure acquires perfection, and its higher functions become displayed, convulsions grow less and less frequent, until from the tenth to the fifteenth year they cause less than 3 per cent., and above fifteen less than 1 per cent., of the deaths from diseases of the nervous system." (p. 17.)

Bouchut regards the *essential* convulsions, which we should place under Proposition 2, as well as others not dependent upon material lesions of the nervous centres, "as being developed as a consequence of the most various causes, and very often, in the course of certain morbid conditions, always the same, whence we are inclined to believe in the existence of a peculiar sympathetic connexion between the brain and the organ diseased." He affirms that we willingly admit of this connexion in the adult, and therefore sees no reason why we should reject it in the child. "It manifests itself in the former by *delirium*—that which signifies the dissociation of ideas; and in the latter, by the perversion of the only and perfect cerebral function which exists, that is to say, by disturbance of the muscular movements." The convulsive state in the child, therefore, according to Bouchut, is to be regarded as the only possible *delirium* which can occur to it. Dissociation of ideas cannot ensue, because their association has not as yet been accomplished; and therefore we may view the *convulsions* which terminate infantile pneumonia as analogous to the *delirium* which may accompany it in the adult. Dr. West writes as follows:

> "In early life the superintendence of the motor power is the chief function of the brain, which has not yet attained to its highest office as the organ of the intellect. Hence the convulsions which you may observe to come on in infancy, in the course of some acute diseases, such as inflammation of the lungs, do not import that any new malady has invaded the brain, but simply that the disease is so serious as to disturb the due performance of all the functions of the organism, and of those of the brain in common with the rest." (p. 16.)

These remarks of Dr. West, and the views advanced by Bouchut, bear especial reference also to many of those convulsions which we would place under Proposition 5.

It may be asked—are not such assumed purely neurose convulsions, primarily arising from the centres, as we suppose may be placed under Proposition 2, alone non-reducible to the class of reflex actions, or to Propositions 3 and 5, simply from our defective knowledge and observation? It may be so. In old times, it was a very favorite theory to regard numerous forms of convulsive action as having simply *neurose* causes in the centres, and as constituting an *essential malady*, or a *special* disease. We have reduced many of these to more definite and easily demonstrable causes; but as yet we can neither go the length of Braschet, who affirms that in all convulsive maladies denominated idiopathic, or whose *point de départ* is believed to be in the encephalon, and where autopsies have been performed, the cause of the convulsions has *always* been discovered in this organ; nor of those who maintain that such convulsions are *always* sympathetic with, or *symptomatic* of, some other disorder. At any rate, *we* have not been always able to trace them to *material* lesional causes, to more primary disorder of other organs, or to the irritation of incident or excitor nerves.

The forms of convulsion to be placed under Proposition 3 are illustrated by the more frequent varieties of laryngismus, and the numerous fits of partial or general character occurring during the travail of dentition, from the presence of worms, indigestion, and other sources of irritation, which may act upon "any part of the class of the incident nerves of the spinal system, whether situated in the mucous or serous membranes, or other tissues or organs." (Marshall Hall.) The convulsions of Proposition 4 are indicated by those following inflammation of, and deposits in or upon, the nervous centres, or after effusions into the ventricles, &c., softening, and the like. Such are the convulsions of simple acute, and tubercular meningitis, of hydrocephalus generally, of tubercle in the brain, &c., certain forms of epilepsy, laryngismus, &c. &c. Proposition 5 will include those convulsive actions which arise in the course of other maladies, as secondary or late phenomena. Such are the convulsions of pneumonia, of exhausting diarrhœa, of hooping-cough, &c. &c. In most of these cases it is presumed that the brain secondarily suffers from a change produced in its vascular system, brought about by the primary disease, whether that change be mal-oxygenized blood—blood deprived of its serous portion, or greatly diminished in quantity—or intense though temporary congestion of the vessels of the nervous centres and their coverings. We would recommend to our readers the observations of Mauthner relative to the effect of almost the slightest disturbance of the vascular system in producing convulsions, as also his views as to the fact of their representing, during the first years of life, the *fever* which in after years is so liable to be set up on the invasion of some local disturbance.

The nature and causes of *laryngismus* have been so concisely and tersely treated of by Dr. Hall, that it is very difficult to analyse or compress his remarks upon the subject. He well states that mere laryngismus in itself is far from being a peculiar affection of infants, inasmuch as it is a common contingent of a whole class of affections of the spinal system. But it must be remembered that in the infant we often see it existing alone, the division of the nervous *system*, of whose disturbance it is the exponent, being apparently the only portion affected; whereas, in later life, it is *mostly* combined with more general convulsions.* If these occur in the child, they are rather the effect of the laryngismus, from the general congestion which it produces in the nervous centres. Dr. Hall maintains that in the greater number of cases, this disorder is a *reflex action*, having its more frequent causes either in "the irritation of the trifacial nerve in the gums and alveolar processes in dentition;" or in "the irritation of the pneumogastric nerve by indigestible matters in the stomach;" in "the irritation of spinal nerves from morbid contents of the intestines;" or in "the influence of the external atmosphere, under certain circumstances, on the incident branches of the pneumogastric nerve of the larynx."

Whilst these are the more common causes of the disorder, Dr. Hall admits, along with Dr. West and Mr. Dendy, that it *may* have a *centric* origin. After speaking of the various sources of irritation which will give rise to this affection, Dr. West alludes to a case in which it appeared, and in which no cause could be assigned during life, and no traces of disease were discoverable after death.

---

* Dr. Budd related, at the Medico-Chirurgical Society, three interesting examples of apparently pure laryngismus, occurring in adults convalescing from fever.

Mr. Dendy thinks, "that gastric irritation, especially when aided by malaria, is more influential than mere dentition" in causing it. Of the influence of gastric disorder in producing it there can be no doubt, as is well shown by Dr. West; but we confess we do not exactly know what interpretation Mr. Dendy would wish involved in the word "*malaria*," or how *malaria*, such as we mean, can *directly* produce it. Dr. Meigs does not consider laryngismus a common disease in Philadelphia.

We are glad to find that our own view of the nature of the so-called "acute hydrocephalus" is maintained by Dr. West, viz., that it is a scrofulous affection, having, as one of its most important lesional elements, the deposit of tubercular granulations. This view is the one adopted by many of the continental writers, and was first promulgated by M. Pappavoine in 1830. Dr. West gives a valuable analysis of thirty post-mortem examinations of "acute hydrocephalus," as relates to the presence of fluid in the ventricles, the state of the lining membrane, and the condition of the cerebral substance, the mutual relations of which have not been fully investigated. This valuable analysis, in itself most condensed, is however, too long for us to extract, but we refer to it with great pleasure (pp. 53, 54).

Dr. West and Dr. Meigs enter at some length upon the *prognosis* of this very fatal disorder, and the former agrees with M. Guersant in considering it is almost always fatal when fully developed, having seen but one instance of recovery from the second stage of the malady. This great fatality has been observed under almost every variety of condition, symptoms, and treatment. Dr. Meigs states that the ten cases he has seen all proved fatal. But this is a very limited experience of the malady. Mr. Dendy makes no direct allusion to the proportionate fatality, and seems to us to mix up, in "admired disorder," acute inflammation of the brain or its membranes, fortunately of very rare occurrence in childhood, as Dr. West remarks, except as the result of fracture of the skull, or of injury to the head or neck, along with tubercular meningitis. It is true that Mr. Dendy does thus write—

"Is inflammation the cause of hydrocephalus? The question is puerile."

Also that the opinion of the senior physician to the infirmary of which he is surgeon, is that—"A most important step towards a knowledge of the true pathology of this disease, was the discovery that the fluid poured out into the ventricles is not a mere dropsical effusion, but that it is the result of previous inflammatory action."

We are willing likewise to add, that Mr. Dendy implies his belief "that, in the great majority of encephalic effusions, the prevailing, if not the essential, predisposing cause is strumous or tubercular tendency;" as also that he does especially allude to *tubercular meningitis*: but must still add something more, which is, that we could not see our way at all clearly through his chapter on *encephalitis*. We seemed to lose ourselves too in a great jumble, when going through his forty-two pages which treat of all sorts of things,—from abortions of dead hydrocephalic fœtuses, through herniæ cerebri, cretinism, stridulous convulsions, &c. &c., up to *paresis* and its benefit by galvanism; we were like Cassio, remembering a mass of things, but nothing distinctly. Had Mr. Dendy *attempted* much less, he would have *perfected* much more. Of one thing further, also, we are sure; great as may be Mr. Dendy's own experiential qualifications for undertaking a 'monograph' of forty-two pages on so many of the cerebral diseases of children, yet we should require him to be well acquainted with the investigations of the *later* continental writers upon the subject, before he undertook one still more extensive; and that he is not so, is too evident.

Apropos, however, to the proportionate fatality in tubercular meningitis, with all respect for Dr. West's great experience, and after carefully weighing his very useful cautions to his students, as to their not being deceived by delusive appearances of improvement, we are inclined to extend the less than one per cent. of Guersant, after the second stage has arrived, to about five; and we think that the latter part of the statement of M. Guersant, that many of those who have seemed to have recovered, have either sunk afterwards of the same disease in its acute form, or *have died of phthisis*, does not bear upon the point in question.

The observations of Dr. West as to the value of depletion in the treatment of this disorder, are those of a sound discriminating judgment, derived from great experience, and well deserve to be remembered by such as consider this affection to be one involving alone an inflammatory action, and to be treated accordingly.

> "With reference to depletion, you must not forget that the disease in which you are about to employ it, although of inflammatory nature, is inflammation in a scrofulous subject, and is in many cases grafted on previous organic disease; such as those tubercular deposits in the membranes of the brain, which I have already described to you. You cannot therefore hope to cut short the affection by a large bleeding, but your object must be to take blood enough to relieve the congested brain, and no more than is necessary for that purpose." (p. 69.)

It is quite in accordance with our experience, as with Dr. West's, that ordinarily *venesection* is not demanded, but that *local* bleeding will effect all that is needful; and we differ entirely with Dr. Meigs, that the converse is proper.

We are disposed to attach rather more value to the action of blisters than we think Dr. West is; and, in our experience, severe counter-irritation and active purgation have done more *temporary* service—as therapeutic service, alas! is here only this in most cases—than have any other remedial measures. Dr. West and ourselves are again unfortunately at variance with Dr. Meigs, who thinks a strong derivative action upon the intestinal mucous membrane can only be injurious, by increasing the febrile reaction and nervous disorder already existing.

Dr. West gives a very interesting case of *phlebitis of the sinuses of the dura mater,* a disorder which, though not confined to children, occurs among them more frequently than in adults. Dr. Meigs leaves this affection unnoticed, also the subjects of *atrophy* and *hypertrophy* of the brain, as well as those of *tubercle, hydatids,* and *cancer* of it, all more or less noticed by his far more experienced labourer in the same field; notwithstanding that the former says, in his preface, that he hopes he has been able to select from the labours of others whatever is most important to be known in the present state of medical science. We differ from him in thinking that he has always done so.

There is one addition we would make to Dr. West's therapeutics in the treatment of *chronic* hydrocephalus; and that is, the recommendation of the daily external use of the comp. iodine ointment, along with the employment of the iodide of potassium internally. We believe that we have seen much benefit from these agents. Our author inclines to the mercurial method of Goelis as being that most likely to afford benefit.

We referred to Dr. Meigs with the expectation of almost surely finding some observations with regard to that peculiar disorder called *trismus nascentium*, because it appears to be by no means an unfrequent malady in America, Dr. Sims having lately stated that no less than fifty Negro children in one plantation have died from the disease within the last ten years. Although it may happen that, near Philadelphia, the children, Negro or otherwise, are better taken care of than in the more southern and slave states,—and hence would arise, according to Dr. Sims, their immunity,—yet, from the attention which the latter gentleman has so recently paid to the nature of this affection, and the peculiarity of his views, we think Dr. Meigs might have provided his countrymen with that information which it appears they would infallibly require should they step from Pennsylvania into Carolina, or Georgia, or Florida. Dr. West, with his usual ability, touches upon the subject. He mentions that three cases occurred under his notice in the Dublin Lying-In Hospital, but that no instance has come before him at the Infirmary for Children. He believes that few infantile maladies "run so fearfully a rapid course as this; its fatal termination almost always taking place within thirty-six, often within twenty-four, hours from the appearance of the first symptoms;" but Dr. Sims, whose experience is more considerable by far, says the disease may present different degrees of severity, some cases terminating fatally in a few hours, and others continuing for several weeks. Dr. West perhaps can only mean to refer to the more decided cases; whilst the former includes such slight forms, that, except for one symptom, much room for doubt exists as to the nature of the cases. This symptom is inability to suck, upon which also great stress is laid by Dr. West. On referring to Mauthner, we find that he and Verson state a peculiar contraction of the mouth to a point, as always to be observed at the onset of the malady, and that the difficulty of swallowing is so great, that the child at every trial made to give it food, rejects it through the nose. Dr. West remarks that the power of sucking is early lost, but for some time the child continues able to swallow. For further information upon this disorder, we must refer our readers to the 'American Journal of Med. Sciences,' 1846-48, or to p. 276 of our present volume. In the latter they will find an account of Dr. Sims's more matured views, which we may briefly state to be the following: The disease is caused by displacement and pressure of the occipital or the parietal bones upon the nervous centre; that if the proper position of the infant be attended to, Nature will correct the evil, but that it may require the interference of the surgeon to liberate the confined, depressed, or misplaced bone—this may be effected by elevating the depressed occiput with an instrument something like an awl; that the disease depends more upon the improper management of children, than on climate or anything else. These views of Dr. Sims, we would remark, cannot explain the *epidemic* which occurred in the Lying-in Hospital at Stockholm in 1834, nor the frequency of the affection sixty years ago in that at Dublin.

We pass with regret from this interesting class of affections, so ably treated by Dr. West, to the *disorders affecting the respiratory organs.*

Lecture XIII introduces the subject; and we have discussed in it, and the lectures which follow, *Imperfect expansion of the lungs, Collapse of them,* and *Induration of the cellular tissue.* Through *Catarrh* we proceed to the various forms of *Pneumonia,* to *Œdema, Gangrene,* and *Pleurisy.* Then follow *Croup, Spasm of the glottis,* and *Hooping-cough.* The subject of *Pulmonary Phthisis* concludes this, the second division.

All who have attended to infantile pathology, must know that Dr. West's Report on *Pneumonia* in the pages of one of our predecessors,* is one of the most valuable monographs we possess upon the subject; and this disease, with bronchitis, atelektasis, collapse of the lung, &c., are discussed in his Lectures with the powers of a master. So valuable is *all* that he tells us, that it is difficult for us either to comment upon or to select from it. We quote the following, however:

> "The exaggerated estimate of the frequency of lobular pneumonia, and the peculiar character of the field presented at the Hospital for Children at Paris, in which the most diligent and most successful students of children's diseases laboured, led to an underrating of the frequency and importance of *lobar* pneumonia, such as is met with in the adult, and hence you will find but little said concerning it in many most valuable works of our continental neighbours; lobar pneumonia, however, is often met with, both as an idiopathic and a secondary affection, giving rise to the same morbid appearances as in the adult, and requiring a very similar treatment." (p. 175.)

Dr. Meigs believes that the *lobular* form is much more frequent than the lobar, since of 51 cases of pneumonia which came under his observation, 38 were of the former, and only 13 of the latter form.

Our readers may be aware that two young French physicians, MM. Legendre and Bailly, have written an elaborate memoir (Archives Générales de Médecine, 1844) to prove that the so-called *lobular pneumonia* of young children is quite a different disorder to that which it has usually been supposed to be.

> "We are convinced," say they, "after attentive and minute anatomical examination, of the absence of all inflammatory results in the indurations found in the lungs of a considerable number of children who die from pulmonary catarrh, and often after other disorders—indurations which have hitherto been considered as the disseminated nodules of lobular pneumonia. Having found a certain analogy between these alterations and the state of the lungs of an infant who has never respired, we have been led to inquire if the pulmonary tissue of an infant, placed under certain conditions, might assume the anatomical appearances that it presents before the establishment of respiration, and we believe that we have arrived, after a sufficiently sound process of reasoning, at the verification of such a phenomenon. We may from this adduce, that if the modification undergone by the lung during life is nothing more than the return of this organ to its fœtal condition, we should observe after death, if not an identity, at least a very close analogy, in their anatomical characters, and in fact, such is seen." (Bib. du Méd. Praticien, p. 168.)

After having described in a very complete manner that which is to be understood as the *fœtal* condition of the pulmonary tissue, MM. Legendre and Bailly then establish two varieties of it, which may occur in the living child; to the first they give the name of *état fœtal simple,* occurring in children of from fifteen months to five years old as a complication of most maladies, but particularly of bronchitis; chiefly seen at the periphery of the organ at the thinnest parts, and forming that affection which has been described as *marginal pneumonia.* Upon the second variety they bestow the name of *état fœtal congestionnel,* the only form met with in new-born children; and affirm that it is to examples of this alteration that the greater number of facts furnished by authors, as illustrative of cases exemplifying simple or double lobular pneumonia, are to be referred. This fœtal state of congestion, united to catarrhal inflammation of the pulmonary vesicules, invading rather extensive portions of the posterior margin of the lungs of children of from two to five years of age, constitutes the alterations described under the name of *pneumonie lobulaire généralisée.* Upon this subject Dr. West thus writes:

---

* British and Foreign Medical Review, Vol. XV.

> "In the course of his investigations on the structure of the lung, Sir E. Home ascertained that during the momentary distension of the air-cells in respiration, an interruption is produced between the arterial and venous circulation in the lungs, the blood being carried no further than the small arterial branches surrounding the air-cells. Now MM. Bailly and Legendre conceive that, on the other hand, a distended or congested state of the pulmonary capillaries may compress the air-cells, and reduce them to the same collapsed condition as sometimes takes place from a mere want of respiratory power. This theory, too, I believe to have in it a large measure of correctness; and it is probable that the occurrence of this condition is due to a congested state of the vessels in many cases in which the accumulation of secretions in the air-tubes is not so considerable as to render them impervious to air, and in which long-continued illness has not exhausted the strength, and thus impaired the respiratory power. It is thus that in some cases of hooping-cough, where yet, perhaps, the quantity of fluid in the bronchi is not very considerable, we find after death a generally congested state of the lungs, and in the midst of the congested tissue patches, more or less extensive, of a dark colour and solid texture, impermeable to air, but still admitting it, though perhaps not freely, on forcible inflation of the bronchi." (p. 158.)

MM. Legendre and Bailly maintain, then, that the so-called lobular pneumonia is "produced in reality by an occlusion of the pulmonary vesicules;" that the so-considered carnified or hepatized portions may be brought back, *à l'état physiologique,* by insufflation; and that this, therefore, necessarily excludes all idea of inflammation and its veritable lesions; whilst lung, plainly hepatized, permits not of the entrance of air by such a measure. Bouchut and others assert that hepatized lung produced by inflammation, up to the second or third degree of intensity of hepatization, will permit of the gradual entrance of air, and become crepitant and floating after insufflation. We agree with M. Fabre that at present it is very difficult, without having before us still more numerous observations minutely recorded, to pronounce upon the value of the details into which the continental writers on the subject of pneumonia have entered. We shall perhaps afterwards discover that Legendre and Bailly have rather darkened than thrown light upon the subject in question. In giving the name of *état fœtal* to that which others have denominated *carnification,* or even *lobular pneumonia,* they have endeavoured to distinguish two distinct anatomical conditions, which, whether rightly or wrongly, have been spoken of hitherto under the same appellation; and in this endeavour they have done only what is warrantable. But when they call that pulmonary catarrh which others speak of as generalized lobular pneumonia, or pseudo-lobular pneumonia, they must previously have compared in their own minds two things which are not comparable, viz. the anatomical condition, and the nature or etiology of a disorder. There is *à priori* no reason why a true lobular pneumonia may not be catarrhal, that is to say, developed under those conditions which are usually understood to constitute the catarrhal state. As relates also to their anatomical details, and the deductions these writers draw from them, we may remark, that the admitted *structure* upon which they have based them is not so universally held to be true as they suppose; those anatomists who have specially occupied themselves with this subject, by no means recognising that *structure* described by Reisseisen as belonging to the lung.

We recommend to our readers the exposition by Dr. West of the connexion between that peculiar disease, extremely common in the foundling hospitals of the Continent, named *sclerema*, or *induration of the cellular tissue*, and that of imperfect expansion of the lungs, and of the consequently incomplete performance of the respiratory functions. In the 'Bibliothèque du Méd. Praticien' is a long article devoted to the same subject, which is full of interest, though no notice is taken of the important investigations of MM. Troccon, Bailly, and Legendre in connexion with it, as a result of the condition of the lungs just spoken of. The lectures by Dr. West on *phthisis* are replete with instruction.

Dr. West devotes a lecture—and a very able one, too, it is—to *diseases of the heart;* but Dr. Meigs lets them slip altogether,—why or wherefore we cannot divine, unless it be that, like many others, he may imagine that children are never the subjects of them. A more serious fallacy never existed; and we believe that when a new edition of Dr. West's Lectures are required—which they infallibly will be—he will have obtained material amply sufficient for a second lecture. Every month's experience gives us additional reason to believe, that even the most acute observers amongst us are not yet sufficiently alive to their frequency, not as relates to children at the breast, but from two years of age and upwards. We refer, of course, to inflammatory affections of the endocardial and exocardial membranes. Practitioners are constantly overlooking these disorders, through referring the dyspnœa, which is so frequent an attendant upon them, purely to disease in the respiratory organs, with which, it is true, the cardiac affections are frequently combined. Thus the central organ of the circulation escapes from their scrutiny. Since our attention has been directed to the frequent liability of children to these diseases, we throw considerable discredit on ourselves for the many past errors of which we believe we have been guilty. M. Fabre quotes a case of "*scirrhus of the heart*" in a child three days old.

From the diseases of the heart Dr. West passes to those of the *Organs of digestion and assimilation*, which include the various affections of the *mouth*, *stomach*, and *intestines*, *jaundice*, *peritonitis*, and *tabes mesenterica.* The subject of *intestinal worms* concludes the third division of the lectures.

Dr. West and Dr. Meigs both enter at full length into the evils resulting from the improper dietaries to which children are subjected; and the former pointedly exhibits both the unphysiological principles which recommend, and bad practical results which are found to follow, the use of a vegetable aliment for children only partially or not at all suckled. Speaking of the *atrophy of young children*, he thus tersely remarks:

"Whether all food is withheld from an infant, or whether it is supplied with food which it cannot assimilate, or whether disease prevents it from digesting food on which a healthy infant would thrive, the main result is the same, and the child dies of inanition. Various accidents may abridge the infant's life, or make it sink in one case under circumstances somewhat different from those which precede its death in another. Sometimes the vital powers grow so feeble, that the inspiratory efforts no longer suffice to fill the lungs with air; sometimes the irritable stomach rejects all food; while at other times diarrhœa comes on, which no medicine can check. But in these symptoms there is nothing characteristic of one special cause; they may occur alike in the infant who, though healthy when born, was early deprived of its mother's milk; or in the child who is the subject of general tuberculous disease, or whose strength has been exhausted, and its digestive powers impaired by dysentery. The symptoms, then, that accompany the atrophy of new-born children must be expected to vary much in different cases." (p. 339.)

It was this primary defective assimilative power and its results, to which the older writers gave the names of *infantile cachexia, infantile marasmus, dwindling,* &c., and which they regarded as a special disease. Although it is true that it is a condition that may be induced by various causes,—as Dr. West insists, yet we think that we have not left them far behind in attaining to a much more precise notion of it, except in words perhaps: we believe that they knew quite as much about it as we do; they only pretended to know more, and that was their error—an error shown in the attempt they often made to connect it with mesenteric disorder. If we are not able to associate a group of signs and symptoms with special and proper anatomical lesions, in the relation of cause and effect, it is still permissible in medicine to give to that group the name of, and to regard it as a *disease;* in fact we hold that Dr. West himself does so, when he devotes some pages to the consideration of "simple diarrhœa."

Dr. Meigs gives a very amusing account of the indiscriminate diet allowed to children in America; and if it be such as he states, certainly we are not surprised at the Americans being such "a pale, thin, dyspeptic, and anxious-looking race of people compared with Europeans." Upon this and collateral subjects it is evident that Dr. Meigs writes more *con amore* than upon anything else; and as his remarks then come more from himself than from other persons, they are the more worthy of recommendation to our readers, if they wish to know really what Dr. Meigs is capable of. At the present moment the following extract from Dr. West is full of interest, and well worthy of the notice of Sanitary Boards:

"In almost every country and climate, and under circumstances, in many respects, very different, dysentery has been known to occur, but in each instance it has been possible to connect the prevalence of the disease with some source or other of malaria. Although, while I was physician to the Finsbury Dispensary, a large amount of disease among children, as well as among adults, came under my notice, yet my acquaintance with those severer forms of infantile diarrhœa, which approach to the characters of dysentery, and which give rise to similar lesions, has been derived almost exclusively from observations made in Lambeth and the adjoining parishes. The children in both districts are alike subjected to the evils of improper and insufficient food, and of close and ill-ventilated dwellings, but in the latter there are superadded certain very important influences of a local character. A considerable portion of the district, on the Surrey side of the Thames, lies below high-water mark, and the kitchens and cellars of some of the houses near the river become flooded at unusually high tides. The sewerage throughout is very defective; in many parts it is effected entirely by open drains, while in some places there are mere cesspools, which have no connexion with any drain whatever.

"Cases of infantile dysentery do not occur with the same frequency in all parts of this district, but they are the most numerous and most severe wherever these noxious influences are most abundant." (p. 401.)

Dr. West illustrates the above remarks, and the intimate connexion subsisting between such conditions as are therein mentioned and infantile dysentery, by a very marked and interesting case, to which we direct the special attention of our readers.

Dr. Meigs has treated specially of *cholera infantum*, or *summer complaint*, a disease which was at one time thought to be peculiar to children in warm climates, and especially in the southern states of America. Its real nature, writes our author—

> "Was very imperfectly understood until within a few years, when the researches of Dr. Horner showed that it resembled very closely the follicular enteritis, described by Billard in his work on Children. Dr. Hallowell, in his recent and valuable paper which I have so often quoted, gives it the title of endemic gastro-follicular enteritis, and regards it as a disease chiefly of the follicular apparatus of the digestive canal. After a careful study of some of the more important works upon the diseases of children, after close comparison of these descriptions with those given by our own authors, and with my own observations, I am led to the conclusion that cholera infantum, or summer complaint, is the same disease, with differences of frequency, severity, and fatality, as that described by Billard as follicular enteritis; by Barrier as follicular diacrisis, apyretic, febrile, and complicated; and by the authors of the Bibliothèque du Médecin Praticien,' under the title of "enteritis of children," including in that term inflammation, softening diarrhœa, and diacrises. Rilliet and Barthez also describe most of the symptoms and anatomical lesions of the disease, in their chapter on inflammation and softening of the gastro-intestinal mucous membrane." (p. 295.)

We believe ourselves that the difference of this "cholera infantum" of America, from any diarrhœa we have amongst us, consists in this severity, and we would add *rapidity* of fatality mentioned by Dr. Meigs; and if all be true, so far as our own experience goes, and that of Dr. West, we have it not in such a form amongst the children of this country. Dr. Eberle says, it sometimes runs on to a fatal termination in five or six hours. But apparently this rapidity is not the rule even in America; for Dr. Meigs remarks, that the most rapidly fatal case he has met with lasted nine days. Of such a form we have no lack of examples, in the autumnal season, in bad neighbourhoods.

We are rather surprised that Dr. West has not alluded to the great value of the *nitrate of silver* given internally in the treatment of *chronic diarrhœa.* This is a remedy which has received great praises both on the Continent and in America, and from some British practitioners as well. We are constantly in the habit of employing it, and it is almost as constantly answering our intentions. Our author merely mentions the fact of its recommendation by M. Trousseau, in the form of enema, but states that he has no experience in its use. We regard it as one of the most powerful and certain astringents which we can use in the chronic entero-colitis of young children; and further than this, it appears in such cases to have often a very marked effect in improving the assimilative powers, and in acting as a general tonic. Even in cases which ultimately prove fatal, it is often remarkable how quickly a stop is put to the alvine discharges, although the child receives no benefit constitutionally from it; but, *vice versa*, we do sometimes find that it improves the tone and appetite, whilst the dejections remain unaltered in frequency. But in more severe cases, although the disease often appears to obey it at once on its administration, yet, as might be expected, it soon ceases to do so, and when that has happened, we have not then found its further administration, in larger doses, more beneficial. Still, in hopeful forms of this disorder, we recommend it with the greatest confidence. We used it once in a very bad case, in the form of enema, with advantage, after it had ceased to stop the evacuations, when given by the stomach.

Of full doses of the acetate of lead, as recommended by Dr. Golding Bird, we can speak well as an astringent, and we have far more confidence in the metallic salts generally, for this purpose in children, than in any of the vegetable productions, from catechu to logwood. The great vegetable astringent for children is *opium*. Dr. West has sometimes used gallic acid in the form of enema. Of this agent so employed, we have had no experience. Dr. West recommends the decoction of tormentilla as an enema in *prolapsus ani;* we have found Mr. Vincent's remedy—a sulphate-of-iron injection—answer satisfactorily.

Dr. West devotes a lecture to the subject of *peritonitis* through its various phases, from its occurrence during intra-uterine life, when it sometimes gives rise to intestinal adhesions, and effusions into the abdominal cavity, causing the death of the fœtus, up to the chronic form of the disorder, seen in connexion with tuberculosis of the mesenteric glands. Dr. Meigs does not mention the subject.

It is impossible for us to enter upon the question in detail, as to whether there is or is not such a disorder as *idiopathic gelatiniform softening of the mucous membrane of the stomach.* Some writers believe that there is such a primary disorder in children, and that it is marked by a train of symptoms sufficiently precise as to permit us, or at least MM. Baron and Billard, to diagnosticate its presence.

> "Some writers, among whom may be mentioned those eminent authorities, M. Cruveilhier and Professor Rokitansky, have endeavoured to distinguish between two kinds of softening, one of which they regard as a post-mortem occurrence, the other as the result of disease. The softening of the stomach observed in infancy, is believed by M. Rokitansky to be of the latter kind, and to depend upon a disease which he regards as almost peculiar to early life." (Dr. West.)

The late views of Elsässer, and of Herrich and Popp, are referred to also by our author, and an elaborate chapter is devoted to this subject, well worthy of perusal, in the 'Bib. du Méd. Praticien.' Dr. West, like many others, has not been able to discover any peculiar or constantly occurring symptoms which could indicate the existence of this gastric softening; our experience agrees with this, and we reject, for the following reasons, the notion that any of the various degrees of softening ordinarily met with, are to be regarded as the results of a special malady:—1. Because softening is an alteration which may be produced apparently by the action of the gastric juice upon the stomach, and by the acids generated during the decomposition of the matters contained in the stomach at the time of dissolution. 2. Because the most numerous observations prove it to be always associated with some other malady, as enterocolitis, phthisis, tubercular meningitis, muguet, &c. 3. Because of the impossibility of surely diagnosing its existence, or even of fairly assuming it may exist, except when some other disease is present, with which it is known to be frequently seen in connexion after death.

It is *possible* that idiopathic gastritis may occur in the infant and young child; and Billard and Ollivier of Angers believe in its existence, and that it may produce one form of the softening; but of such an idiopathic infantile disorder, we have ourselves no experience.

In treating of *Diseases of the Urinary Organs,* Dr. West makes the following remarks concerning a disease of which we ourselves are ignorant, as occurring in the child, viz. *diabetes:*

> "It would seem to be an exceedingly rare affection; for not only has no instance of it come under my notice at the Children's Infirmary, but Dr. Prout, out of his immense experience in diseases of the urinary organs, states that he has seen but one instance of it in a child of five years old, and only twelve in young persons between the ages of eight and twenty, out of a total of 700 cases of diabetes." (p. 437.)

According to Dr. Prout, there is a modification of diabetes occasionally occurring in very young children, to which the attention of the profession was first distinctly drawn by Dr. Venables. There is great diuresis present, the urine frequently containing albuminous matters; in other instances an excess or deficiency of urea exists; and, in a few, saccharine matters, more or less perfectly developed, are found. The specific gravity of the urine ranges, according to the form of the malady, from 1·025 to 1·005, or even less. We have at present a case under our care, in which there is great diuresis; the urine below 1·010, with deficiency of urea. It is transparent, and of the greenish tint alluded to by Dr. Prout. It contains certainly no albumen. The state of the skin, bowels, etc., are just such as are described by this philosophical writer. This is the only case, however, by which our attention has been thus arrested, after a very considerable experience with children's diseases; and here we are not inclined to regard the malady as one *per se,* or distinct in itself, but as dependent upon tuberculous cachexia, the child having had symptoms of subacute peritonitis; and, although these have been subdued, we believe the serous membrane to be deeply involved. The veins of the abdominal surface are very large, distinct, and seen ramifying everywhere, and the inguinal glands are enlarged and painful. The child is exceedingly weak, but still has much colour in the face, though the affection has been very chronic. Dr. West believes that, in *simple infantile diuresis,* the disturbance of the functions of the kidneys is purely secondary, and subsidiary to gastric and intestinal disorder. Dr. Prout affirms that, if the affection is neglected or maltreated, it most usually terminates in organic lesion of the kidney. A tonic regimen and treatment is that which is to be recommended, with the use of the salts of iron. According to Dr. Venables, the phosphate of iron is especially useful. Opium is to be given with extreme caution, and a diet containing more of the albuminous than of the gelatinous principles is to be enforced.

Dr. West does not add anything to our resources in the treatment of that most troublesome affection of children and young women—*incontinence of urine.* After all that may be said about the chemical conditions of the renal secretion, and the necessity of acting in our treatment on pathological principles deduced from them, yet, so far as beneficial results go, we do not get nearer success. Practice is, when successful, we believe, more empirical than otherwise; opium internally, and blisters to the sacrum, appear to us to be of more use than other remedies. We may often succeed in alleviating the *eneuresis* of the daytime, whilst the nocturnal incontinence remains obstinately unaffected. Dr. Chambers (Prov. Med. Journ.) has recommended, in obstinate cases, the cauterization of the orifice of the urethra by the nitrate of silver, as sometimes employed for the purpose of preventing masturbation. We have not found so much benefit as we expected from the use of large doses of the nitrate of potassa, which has been vaunted lately in several quarters. Dr. West has once or twice employed it with advantage.

The observations our author makes on *abdominal enlargement and tumours* are important. He has once seen the liver the seat of malignant disease of the fungoid kind, and has met with two instances of malignant disease of the kidney, at the respective ages of two years, ten months, and fourteen months. Enlargement of the spleen in a little girl, six years and a half old, who had lived at Fernando Po, was observed by Dr. West, the organ extending quite down into the pelvis.

The three concluding lectures are devoted to the *febrile diseases* incidental to infancy and childhood; and since, as the author states, few of the affections of early life have been watched so closely, or described with so much accuracy, as smallpox, measles, and scarlatina, we shall not detain our readers about them.

In taking leave of Dr. West, we can scarcely do more than reiterate our former praise of him. We have given, we fear, but a very faint notion of the scope of his work, and of its excellent execution. It is one standing by itself upon its important subject, in our language—unapproached—unrivalled. We do not mean to assert that Dr. West has not been indebted, and largely too, to the continental writers, for their assistance in illustrating many interesting points in pathology, and for often lending him a light for his direction through many intricacies. And it is well that he should have availed himself of such aid—aid, too, which was unprocurable from the writers of this country. His knowledge of what others have done is equalled only by his own extensive experience; and the results of both are combined in his valuable practical lectures now offered for the guidance of others. Admirable as they are, however, we have not the least doubt but that, when another edition of them appears, Dr. West will see fit to fill up a few vacancies. Some lectures on *rheumatism*, on *intermittent fever*, and on *scrofula*, etc., would help to do this. *Purpura*, also, might not be forgotten in the additions.

The works of Maunsell and Evanson, Rees, Underwood, and others, must now relinquish the field they have occupied for some time past. It will be long before that of Dr. West finds a rival in this country.

We promised to make a few general remarks upon the work of Dr. Meigs, junior. We cannot praise it much, because it is nearly all compilation, and such a kind of compilation as we do not think best adapted for those for whom it is especially intended—the student and junior practitioner. (It forms No. 3 of the Student's Library.) We give Dr. Meigs credit for being acquainted with the views of others, but not for having deduced from them such results, and presented them in such a form, as are readily available to the student, who will often be confused by Dr. Meigs's labours. The student expects his teacher to speak *ex cathedrâ*, and to be told rather what *he* ought to *do*, and not what *others have done;* to be informed of how his teacher *has acted*, and not how he *would* act. After consuming nine pages with an account of the treatment recommended by different writers in pseudo-membranous laryngitis, Dr. Meigs devotes twenty-four lines to giving the student some decided and definite modes of procedure, and these not as if already drawn from his own experience, but as what may be expected to come up from the bottom of the well some day or other. For instance (the italics are our own):

> "In children over two years of age I *would* take from three to four ounces of blood from the arm, once, twice, or three times in two days, according to the strength of the child, and the degree and obstinacy of the fever. In both forms of the disease, emetics, and I *would* recommend the alum in preference to any other, should be given once at least, very often twice, and in violent cases, three times in the twenty-four hours, so as to produce vomiting attended with a good deal of effort. At the same, I *would* give, as a general rule, two grains of calomel, with a quarter or half a grain of Dover's powder, every two hours, taking care not to give a dose for an hour before nor after the time selected for the exhibition of the emetic. In cases in which there is loud stridulous respiration heard, both in the inspiration and expiration, in which previous treatment has had no effect, and in which there is threatening of speedy death, I *would* give four grains of calomel every hour," &c. (p. 50.)

This is certainly not the method best adapted for teaching students. Dr. Meigs's work is also very incomplete, most important diseases meeting with no consideration. With all its drawbacks, however, the book proves that the author is sufficiently aware of the important position which his subject should assume in medical literature, and that he is acquainted with the labours of those who have so strenuously exerted themselves in its furtherance.

The two volumes of the 'Bibliothèque du Médecin Praticien' will be found very useful by those practitioners who are already conversant with the study of children's diseases, but are by no means books for the student. Like Dr. Meigs's work, they are a mere compilation, and that of rather a heterogeneous character. But they contain the *pith* of the works of the chief writers upon the subjects they treat of; and we find that, on the spur of the moment, reference to them saves us the trouble of pondering whether we should refer to Barrier, Billard, Rilliet, Guersant, Valleix, and a host of others, for information upon a special disease, or for a *particular view* of the pathology of any given disorder. These volumes, moreover, include many of the surgical diseases to which children are liable, as also notices of the rarer affections which are now and then observed during the earlier periods of existence.

In conclusion, we may be excused if we say to the student and junior practitioner, let the pathology of children's diseases, and a treatment based upon that pathology, be your earnest study for the future, and let your guide be the lectures of Dr. West. After a while, then glance over the above-mentioned volumes of the 'Bibliothèque,' and you will discover the rich sources of information to which you may proceed, if time and opportunity be at your command. If not, study these volumes themselves; as Dr. West's and your own experience will, combined, enable you to make many useful applications of the "gleanings" contained in them.

ART. X.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXXI. Second Series, Vol. XIII. —*London*, 1848. 8vo, pp. 522. With Seven Plates.

THE present volume, which was somewhat tardy in its appearance, ranks among the largest of the issues from the same source, containing five hundred pages of original matter; while it exceeds most of its predecessors in the variety of its contents, as these embrace no less than twenty-eight communications, of which the subjects are, with very few exceptions, entirely distinct. As one article extends to nearly one hundred and fifty pages, while five others occupy together almost an equal space, it will be seen that the remaining twenty-two must, on the average, be short. There is about the ordinary proportion of illustrations by woodcuts and lithographs, of the execution of which we can speak favorably; and with these brief prefatory remarks, we proceed to a notice of the individual papers.

I. *Observations on intestinal obstructions depending on internal causes, and on the means to be employed for their relief;* by Benjamin Phillips, F.R.S., Surgeon to the Westminster Hospital.

This paper is a carefully-written monograph, penned by an able surgeon, who has bestowed both pains and learning on its compilation. Its professed object is to discuss especially the merits of operative interference in the treatment of intestinal obstructions; a subject which the bolder advances of modern surgery, in reference to diseases affecting the abdomen, has brought more on the *tapis* of late. We do not know that the present investigation could have been prosecuted by one more competent to do it justice than Mr. Phillips; and we think the profession indebted to him for having undertaken the task of statistically showing us what has been done, and with what success, by gastrotomy, in obstructions of the alimentary canal. Having thus broadly stated our own opinion of the paper, we proceed to give a brief outline of its contents, that our readers may judge for themselves.

Mr. Phillips justly observes that the personal experience of any one man, however ample his opportunities, is insufficient to supply data from which a satisfactory conclusion can be drawn, in the discussion of an important practical subject like the present. He has, therefore, freely availed himself of recorded cases, to the number of 136, to which he has been enabled to add 33 unpublished cases, which have been communicated to him by his friends, or observed by himself; these materials he considers ought to be regarded as sufficient for deducing rules of conduct in the treatment of obstinate intestinal obstructions.

The author tells us, in what he makes the first division of his subject, that he comprehends, under the term intestinal obstructions, all mechanical obstacles situated within the abdominal cavity, which, by their action upon the intestinal tube, intercept the course of fecal matter.

In order to estimate the relative frequency of these obstructions as discovered after death, Mr. Phillips mentions that, in an aggregate of 2392 post-mortem examinations made by Louis, Dr. Boyd, Mr. Paget, and

Mr. P. Hewett, they were found in twenty-two instances. The different causes of these obstructions are gathered from the analysis of the 169 cases which constitute the foundation of his remarks; of which 63 were instances of invagination, 16 were caused by tumours from without, 19 were the results of stricture from disease of the parietes, 11 of intra-intestinal tumours, hardened fæces, or concretions, and 60 were caused either by constriction, by bands, by adhesion, by the passage of the intestine through some abnormal opening, or by a twisting of the intestine upon itself.

After the detail of nine examples of intestinal obstruction, in each of which the source of the malady was clearly made out, and ascertained to be of a different kind in every case, the writer arrives at the important and apparently very just general inference, that, no matter what may be the cause of the obstruction, no certain remarkable difference is observed in the more prominent symptoms by which it is accompanied. There are in all abdominal pain, abdominal tension, obstinate constipation, and sickness; but in the mode of their occurrence, it would be difficult to point out any distinct difference.

It appears that our means of detecting the seat of the obstruction are scarcely more satisfactory. Mr. Phillips conceives that, though there may be some truth in the remark that the vomiting and pains are more severe when the obstruction occurs in the small than in the large intestines, the exceptions to this rule are so numerous as to make it unworthy of reliance. The criterion derived from the urinary secretion, he appears to rank also among the broken reeds. The circumstances which he thinks may best serve for our guidance, are the history of the case, the occasional existence of a tumour, which is now and then detected in cases of invagination, and indeed in some other cases, before there is much abdominal tension, the distended convolutions, the greater or less length of enema tube, and larger or smaller quantity of injection which can be introduced per anum. At the same time, he remarks, it is certain that, in the experience of the ablest men, these have failed.

In the foregoing paragraphs we find a negative reply to the question which he had proposed to discuss in the second place, viz., whether the symptoms by which obstructions are characterised, are so clearly marked as to make the diagnosis satisfactory. It seems that neither their nature nor their seat can be determined in general with any degree of accuracy.

The latter part of the paper is devoted by the author to a practical point, that is, to the discussion of treatment.

The fact that, under the plans hitherto pursued, at least 133 out of the 169 cases of obstruction, or rather more than four fifths, terminated fatally, both warrants and suggests some alteration of practice. The mortality in cases of strangulated hernia is stated at rather more than one in three; so that if we take it at two fifths, it will be just half of that now spoken of. Granted that the operation of gastrotomy must be undertaken under less favorable circumstances, and that it is in itself much more perilous than that for external hernia, it is to be expected, therefore, that it will be far less successful; yet with this allowance it may increase by a large proportion the number of the survivors from an almost hopeless condition, while it can scarcely render the patient's state more desperate. Mr. Phillips cites, in favour of gastrotomy, the results of twenty-seven recorded cases of its performance for the removal of intestinal obstruction, of which thirteen are said to have terminated favorably; yet it is to be borne in mind that in many of these, though life was prolonged, it was at the cost of establishing an artificial anus. This appears from what follows at page 34:

> "The experience we possess shows that, whatever may be our notions or plans of relief, there are not more than two well-authenticated cases in which constricting bands have been severed, and, with the exception of cases of invagination, not more than two cases in which the integrity of the intestine has been respected, and the obstacle successfully removed; but there are many in which life has been saved by the establishment of an artificial anus; and I apprehend it is to that point our attention must be earnestly directed."

Notwithstanding this drawback, we think that the author has shown good reason for the conclusions with which he terminates his very interesting and valuable paper, and which we give in his own words:

> "8th. That the interference by surgical operation is justifiable when three or four days have passed without any relief from ordinary means (provided the constipation be complete, and vomiting of fæcal matter continue), because it affords a greater chance for the preservation of life than ordinary means.
>
> 9th. That if the indications as to the seat of the obstruction be sufficient to satisfy the surgeon, it is at or near that point that the incision should be made; but if there be much doubt, it is most prudent to make the incision on the median line.
>
> 10th. That, if it be found impracticable to remove the cause of the obstruction, or imprudent to make any extended search for it, relief may be obtained by forming an artificial anus as near as may be prudent to the seat of obstruction; and that if it be, as it frequently is, near the termination of the ileum, an incision on the median line admits of its accomplishment as near as may be to the termination of that intestine."

II. *Case of malignant tumour of the os uteri, excised during labour;* by James Moncrieff Arnott, Pres., Surgeon to the Middlesex Hospital.

The author of this paper, the esteemed President of the Society, and present Professor of Surgery in University College, commences by commenting on the serious and often fatal obstacle to delivery presented by malignant disease of the pregnant uterus. He then speaks of the comparative rarity of this complication; and refers to the monograph of Puchelt on the subject, showing the fatality of the cases in which it exists. The methods, Mr. Arnott remarks, by which assistance is to be afforded in these instances, are either purely obstetrical, or they may be surgical, by excision of the diseased parts, or by simple incisions into them. He then comments briefly on the objections raised against extirpation of the disease with the knife, and narrates the following case in proof of the safety and advisability of this course, where the extent of surface involved does not forbid its attempt.

In November, 1844, a lady, in apparently robust health, applied to him, having the anterior lip of the os uteri enlarged and indurated; "its surface smooth generally, but rugged and granular round the os itself." Being suspicious of the existence of pregnancy, Drs. Locock and Ferguson were consulted, who resolved the doubt by declaring the patient to be in her fifth month, and acquiesced in Mr. Arnott's impression, that the disease ought to be extirpated, but not till after the patient's confinement. Labour came on in due time, under Dr. Locock's superintendence; but the indurated state of the os uteri prevented it from dilating, and the author was summoned to operate. The excision was effected with scissors, curved on the blades, the tumour being previously drawn down with the assistance of Lisfranc's forceps: not a tablespoonful of blood was lost. In a quarter of an hour the patient gave birth to a living healthy child, and, at the end of the month, "everything had a favorable appearance." The tumour measured two inches and a half in length, being one inch and a half thick: it had a knobbed character, and its surface was partially ulcerated. On section, it presented a yellow colour and fibrous appearance.

The apparent recovery of this patient was delusive. After the lapse of some months, the disease returned in the posterior lip of the os uteri; and she died, sixteen months after her confinement, with all the symptoms of malignant disease of the womb.

Such is the history of a case of not very common occurrence; but, nevertheless, very interesting, as illustrative of the proper course to pursue, where the disease is sufficiently circumscribed to admit of excision.

The more common form of malignant disease in this part is admitted by the author to be such as the surgeon cannot hope to remove, though his knife may assist the efforts of nature and the accoucheur, by relieving the tension and resistance of the indurated mass during parturition. A case in point is added by Mr. Arnott. A woman, five months advanced in pregnancy, was admitted into the Middlesex Hospital, with nearly the whole circumference of the vaginal portion of the uterus affected with carcinoma. She was prematurely confined about a fortnight afterwards. The labour was protracted, and it was expected that incisions into the indurated os uteri would have been required, when nature (it would seem) saved the surgeon the trouble of interfering; for, though no rent could afterwards be detected, there appears to be very little doubt that the os uteri gave way suddenly during a severe pain. This patient recovered from her confinement, but died about six months afterwards, with extensive medullary disease, involving many of the bones to a remarkable extent: the uterus, also, was implicated in the morbid change, in common with the lumbar glands, &c.

This paper is written concisely and tersely, with all the interesting points standing out in salient relief. It is a valuable contribution, as illustrative of the different modes in which surgical relief may be afforded in these distressing complications; for though, in the latter instance cited, the interference of the surgeon was superseded, we must not hope for such a sequel in other cases; and, indeed, it would be unsafe, even if we had confidence in the power of the uterus to overcome the obstacle.

III. *Case of extensive laceration of the liver, with rupture of the gall-bladder and other lesions;* by Walter Fergus, M.D., House-Surgeon to the Staffordshire General Infirmary. Communicated by R. B. Todd, M.D., F.R.S.

Cases like the present are interesting, as evidencing the reparative powers of the frame under the infliction of severe injury, as well as in exhibiting the comparatively trifling amount of disturbance such extensive lesions may entail. Beyond this, the nature of the injury does not command especial notice for its rarity: there are but few surgeons associated with our hospitals who have not seen several such. This, however, as we have remarked, is not the point of the case, which we will now allow to speak for itself.

A labouring boy, of 17, was thrown under the wheel of a cart, which passed over his abdomen. He was brought to the Infirmary in the evening; a surgeon, who had examined him about an hour and a half after the accident, stating that then he complained of pain, which was not severe, and that the amount of shock was trifling. When admitted, he was much in the same state. On the following morning symptoms of peritonitis supervened, for which he was bled, leeched, &c.; and at the end of a week was so far recovered, as to express a desire to return home. On the afternoon of the day when this occurred, he was suddenly seized with extreme pain and a sense of tightness in the abdomen; effusion took place, and he died in fifty hours after this relapse. On opening the abdomen, a large quantity of fluid, apparently bile, gushed out. The peritoneum was acutely inflamed, and shreds of lymph were floating in the liquid. The liver was found to be ruptured in two directions, viz. to the extent of two and a half inches from its thin edge, along the side of the broad ligament, and quite through the substance of the organ; a second and longer, but more superficial, laceration extended along its convex surface, and this was "in a state apparently advancing towards reparation." The gall-bladder was also ruptured near its neck. A transverse section of the liver being made, a dense red band was found traversing its entire extent, and terminating anteriorly in the laceration first mentioned.

If we were disposed to speculate respecting the cause of this patient's relapse, we should be inclined to attribute it to the bursting of an overcharged gall-bladder, which was incapable, from some obstruction, associated probably with the primary lesion, to transmit its contents to their natural destination.

IV. *Case of laryngitis accompanied by the formation of a false membrane in the trachea and bronchi, in which the operation of tracheotomy was performed;* by T. A. Barker, M.D., Physician to St. Thomas's Hospital, and Lecturer on the Practice of Medicine.

The patient in this case was a single woman, of irregular habits, five months advanced in pregnancy, and came under Dr. Barker's care on the 5th of January, 1847. She had been suffering during nine months from cough, tightness across the chest, hoarseness, and loss of voice, symptoms severe at first, and which, though diminished, had never ceased altogether. The results of auscultation and percussion confirmed the opinion that the case was one of phthisis in an early stage, commencing on the right side. On the 15th, an unfavorable change had taken place; the inspirations were long, stridulous, and consisted of a series of convulsive efforts; the face was flushed, the lips livid, and the veins of the neck distended; the chest was resonant on percussion, but no breath-sounds could be heard. Tracheotomy having been resolved upon, portions of the cricoid cartilage and first ring of the trachea were removed by Mr. Travers, jun., without relief, till the orifice had been cleared by Mr. Mackmurdo's finger, introduced into the opening, and passed rapidly upwards and downwards, which was followed by a deep inspiration, and the subsequent expulsion of a large clot of blood. The respiration immediately became free. A whitish, tubular, membranous matter was expelled at first along with the blood, and a tenacious secretion had frequently to be removed through the wound. On the 19th, blood issued from the wound in such quantity as to threaten suffocation, and she was again snatched from impending death by Mr. Travers, who cleared all obstruction from the opening and trachea, and dashed cold water in her face. Of the matter removed, a portion appeared to be a fibrinous cast of the bronchial tubes. The next day the mouth had become affected, by mercury employed internally and by friction for some days. As hemorrhage recurred on the 24th, it was checked by acetate of lead; it did not recur, nor was there any return of the fits of suffocation. After this she suffered much from exhaustion. On the 15th of February, the orifice in the throat had almost closed; indeed, for some days she had breathed and expectorated through the mouth; the voice was only a hoarse whisper. After this, the cough became very troublesome, and, on the 28th of February, she died from exhaustion, the breathing remaining free till the last moment.

Examination after death showed that the whole mucous membrane of the larynx and upper rings of the trachea was destroyed by ulceration. The right lung also, especially in its upper part, contained many clusters of tubercles, and some intervening hepatization.

This case tells its own tale. It is manifest that life was prolonged for six weeks in consequence of the operation, and we have only to thank the author for putting it on record.

V. *Note in reference to a case of malformation of the heart;* by T. B. Peacock, M.D.

A female child, aged five years, had been livid at birth, but subsequently acquired a more natural colour, and was stout and healthy till between two and three years of age. Since that time she had been always delicate, and, when chilled or suffering from catarrhal affection, she became livid in the face, and had hurried and difficult breathing. It is needless to detail the stethoscopic signs, which did not furnish an accurate diagnosis. The child was some months under observation, and died three weeks after an attack of scarlet fever, attended with severe ulceration of the throat, and followed by vomiting of large quantities of blood.

In the cavity of the right ventricle there existed a septum dividing the sinus from the infundibular portion; and this septum was perforated by an oval aperture, twenty-one lines in circumference, by which the two divisions of the cavity communicated. The interventricular septum was perfect, and the ductus arteriosus closed. The case now reported, to adopt the words of its author, is an instance of permanent cyanosis, or at least of lividity of the face and extremities having existed to a very marked degree, though the venous blood did not enter at all into the systemic vessels; the only effect of the peculiar malformation of the heart being the production of constant congestion of the venous system.

VI. *Case of destructive ophthalmia, with extensive suppuration of the cellular membrane and severe affection of the knee-joint, occurring after premature delivery;* by A. M. M'Whinnie, F.R.C.S., Lecturer on Comparative Anatomy, and late Demonstrator of Anatomy, at St. Bartholomew's Hospital.

The present is a valuable addition to the few recorded cases of the severe complication of disease which its history illustrates; and the ultimate recovery of the patient enhances the interest which the singularity of the affection awakens. The patient, a lady of about forty, came under the author's notice in the spring of 1847, two or three days after she had been prematurely confined of a dead fœtus of four months, the decomposed state of which induced the impression that it had contaminated the system of the mother. At the time when she was seen by the author, the globe of the right eye became inflamed and prominent, accompanied by chemosis. This was followed in two or three weeks by swelling of the right lower extremity, and collections of matter formed at various parts, which called for the use of the scalpel as they were detected. The knee-joint next became involved in destructive inflammation, and the powers of the constitution were rapidly deteriorated. One large subfascial abscess involved the whole inside of the thigh, which required counter-opening to be made near to Poupart's ligament. The patient was thus reduced to a deplorable condition, the affected eye being irrecoverably lost and shrunken, the joint of the knee destroyed, and the tibia partially dislocated outwards. At this period, however, she began to mend; the knee was gradually extended, and became firmly anchylosed, and her health was permanently restored. It is needless to observe, that a tonic form of treatment was adopted from nearly the commencement of the attack.

In the Transactions of the Society, already so rich in this department of medical literature, the present case constitutes an appropriate addition, rendered, as we have remarked, more valuable from the rarity of the complication, and still greater rarity of the favorable termination: in the latter respect, the author considers it unique.

VII. *Two cases, in which the skin, hair, and teeth were very imperfectly developed;* by John Thurnam, M.D., Medical Superintendent of the Retreat, near York.

Cases, such as are narrated in this paper, are always worthy of record; and the 'Transactions of the Medico-Chirurgical Society' are a fitting depository for facts, which may for a time not find their full application, but are made available to the diligent cultivator of teratology, as that science advances. We are fully prepared to echo the sentiments of Dr. Thurnam, "that practical medicine itself, no less than the natural history of our species, may hope to receive additions from the study of these exceptional cases."

The two instances described by the author, occurred in relatives, both males, and cousins-german. One died at the early age of thirteen. Their physical resemblance to each other was such, as greatly to puzzle and perplex their friends; but their mental and moral endowments afforded as much contrast,—one being timid and sensitive, and the other bold and fearless, and fond of intellectual pursuits. The survivor alone came under Dr. Thurnam's notice during the last ten years of his life, which reached to fifty-eight years. His features were small and flat, his complexion yellow, and nostrils compressed. Temper sensitive and irritable. There was not so much hair on his head as on that of an infant three months old; eyebrows absent, eyelashes defective; hair of face scanty, and that of pubes consisted of few and scattered, but stronger, stems. His teeth were but four in number, the second and third molars of the upper jaw. Nasal passages very narrow; skin very delicate, void of perspiration, and free from trace of unctuosity; sudorifics only softened the skin, which was frequently harsh and dry. One interesting fact did not come to the knowledge of Dr. Thurnam until too late to verify the probable anatomical deficiency,—viz. that this gentleman had never been known to shed tears. He died of pulmonary affection, to which he had been frequently subject. To the account of the post-mortem appearances, a detailed description of the peculiarities presented by the skin under the microscope is appended: it is from the pen of Mr. Erasmus Wilson. The chief peculiarities noticed by this observer were "a state of extreme laxity and apparent atrophy of the derma, with a total absence of fat and of the usual filling-up material." The sudoriferous glands were not wanting, but their ducts were unusually delicate, and their epithelial lining was very thin. The condition of the skin was, in fact, that of an imperfectly developed secreting apparatus, or, as the author expresses it, of atrophy. Dr. Thurnam considers, and with show of reason, that this pathological state exerted an unfavorable influence on his patient's system generally, and contributed materially to the fatal result of his malady. On attempting to trace any hereditary tendency to a similar defect in the progenitors of these two individuals, the author could only discover that the maternal grandmother had an extremely delicate skin, and perspired but little; but neither she, nor any other of her descendants, to the fourth generation, have presented any deficiency in either hair or teeth.

VIII. *Observations on certain pathological conditions of the blood and urine in gout, rheumatism, and Bright's disease;* by A. B. Garrod, M.D., Assistant Physician to University College Hospital.

As regards the first of these diseases, Dr. Garrod's experiments lead him to the following conclusions:

1st. The blood in gout contains *uric acid,* in the form of urate of soda, which salt can be obtained from it in a crystalline state.

2d. The uric acid is diminished in the urine immediately before the gouty paroxysm.

3d. In patients subject to chronic gout with tophaceous deposits, the uric acid is always present in the blood, and deficient in the urine, both absolutely and relatively to the other organic matters, and the chalk-like deposits appear to depend on an action in and around the joints, &c., vicarious of the uric-acid-excreting function of the kidneys.

4th. The blood in gout sometimes yields a small portion of urea (no albumen being present in the urine).

The evidences upon which these conclusions are based must be sought for in the paper itself; the investigations were sufficiently numerous; and the results so uniform, as to give to most of them a high degree of probability. The quantities of the acid detected were, as might have been anticipated, very minute, amounting only, in one instance of mixed gout and phthisis, to 0·175 of a grain in 1000 grains of serum, while in the other cases it fell short of a third, and in one even of a sixth of that weight; yet, even in these very small proportions, its quantity was many times as great as that of the traces to be detected in the same weight of the serum of healthy blood.

In connexion with the second conclusion, it is remarked that when the affection of the joints has produced excitement in the system, then uric acid and urate of ammonia are frequently thrown down in abundance (from the urine); especially where an attack of the disease produces for a time an improved state of health; but in those persons who have suffered during many years from repeated attacks, the urine, after the fit, often ceases to be loaded with these matters, and then the disease becomes more frequent in its appearance, and tophaceous deposits are often formed in and about the joints. The third conclusion, embracing, as it does, a negative particular with regard to the constitution of the urine, can hardly be received as established by experiments made in the case of one patient only; yet this is all that at present appears in its support.

The subsequent detection of urea (as mentioned in a postscript) in perfectly healthy blood, much diminishes the importance of its discovery in that drawn from those labouring under gout.

It is a curious fact, as bearing upon the physiology of the secretion of uric acid, that none was discovered in the blood of a pigeon, although its urine consists entirely of urate of ammonia. A distinction was established between the condition of the blood in acute rheumatism, as compared with that in gout, inasmuch as, in the former disease, no more uric acid was met with than is found in that fluid in health. This result was verified by experiments made on five different patients suffering from this complaint. In some of these researches, the non-existence of urea was also shown.

The results of the analysis of blood taken from patients labouring under Bright's disease, and albuminuria after scarlet fever, are thus stated:

1st. Uric acid is always present in the blood in albuminuria. The quantity, however, greatly varies; when the functions of the kidneys are much impaired, it exists in quantities almost as great as in gout; in other cases its amount is small, but it usually exceeds that found in ordinary blood.

2d. Urea always exists in large quantities in this blood (a fact that has long since been proved), and no relation is found between the amounts of urea and uric acid.

3d. The kidneys are always deficient in their power of throwing off urea; but with regard to uric acid, their excreting function may be impaired or not.

We consider the present article to be a valuable contribution to humoral pathology. It is especially interesting, on account of the confirmation that it gives to the prevalent opinion, which considers gout to arise from the presence of an increased quantity of uric acid in the system, due to a deficient eliminating action of the kidney, as compared with the amount of acid formed, whether this formation be excessive or only normal in amount. The accompanying plate is well executed, and we hail with pleasure Dr. Garrod's intention of making known the results of his further researches on the history of uric acid.

IX. *On Petit's operation for the relief of strangulated hernia;* by James Luke, Surgeon to the London and St. Luke's Hospitals, and Lecturer on Surgery at the London Hospital School of Medicine, communicated by the President.

Mr. Luke's labours in this department of surgery are already well known to the profession, through the medium of the Medico-Chirurgical Transactions, and other channels. The difficulties surrounding a question such as that discussed in the present paper, are greater than might at first sight appear; and they are ably pointed out by the author, who remarks with reason, that statistics founded on the contributions of various authorities are likely to be fallacious, from the probable want of parallelism in the instances adduced, and the fact of their being often selected rather than average cases. Mr. Luke's large personal experience has enabled him to obviate this difficulty; and the chief interest of his valuable paper is derived from the weight of original facts by which the author supports his opinion in favour of Petit's operation.

We need scarcely state to our readers what the question at issue is. It is now long since the proposal was made to relieve a strictured gut without opening the hernial sac which contained it. This operation, obviously advantageous in some respects, was supposed to have its more than counterbalancing evils, and has been superseded by the more common method of operating by exposing the contents of the sac; until a comparatively recent period, when Mr. Key, in particular, again advocated the operation which Mr. Luke has named after Petit, who was not, however, we believe, its originator. To reduce the risk of an operation, provided that operation can be effectually performed, is doubtless at all times a great boon to the patient, and a source of infinite satisfaction to the surgeon. But this is peculiarly the case in an operation such as that for hernia, where loss of time is fraught with danger, and yet where the dread of the knife withholds the consent of the sufferer, and too often tempts his medical adviser to countenance unwarrantable delay. So strongly are we impressed with the danger of procrastination in cases of strangulated hernia, and with the fact that this, operating conjointly with persevering manipulation of the hernial tumour, is the most rife cause of failure in the ordinary mode of giving relief by division of the stricture, that we think it incumbent on surgeons not to diminish their patients' chance of recovery by any such trifling; but rather to risk performing occasionally an unnecessary operation, than to bring operative interference altogether into disrepute by reserving the knife as the deferred resource, when, in fact, the probability is that its employment will be useless. If there were less rough handling of hernial tumours, and if the practice of operating earlier (whether the sac be opened or not) were more universally adopted, we are well satisfied that the results would be infinitely less disastrous (nearly one half being fatal) than the tables referred to by Mr. Luke show them to be in cases where the ordinary operation has been performed. With this view we cannot but hail with satisfaction any simplification of the mode of giving relief, as it holds out an inducement to both patient and surgeon to eschew delay. This consideration, doubtless, influenced Mr. Luke, and is rightly to be regarded as an important element in the comparison instituted between the two modes of operating.

Mr. Luke states that he has almost exclusively adopted the plan of relieving the stricture (where practicable) without opening the sac since 1841; but that for ten years prior to this he had been in the habit of selecting cases for the same purpose; making in all 84 cases, a large experience, as we remarked, for any one man to have accumulated. In these cases the operation, as originally contemplated, was successfully completed in 59; in 25 it was necessary to open the sac to effect a reduction of the hernial contents; of the former number (the 59) only seven died; of the latter (the 25) eight died. It must be admitted that these results, as compared with the tabular view of cases treated in the more ordinary way, and referred to above, are very striking. Even the per centage of mortality in those cases in which the sac was opened by Mr. Luke is below the average; and this he accounts for by the limitation of his incision of the peritoneum to an extent varying from a quarter of an inch to an inch and a quarter, and by the avoidance of that prejudicial exposure and immediate handling of the intestine itself, which is so rife a cause of subsequent mischief.

It would be out of our power and beyond our province to follow the author through all the necessary details included in his paper, by which he guards himself from the imputation of selecting or excluding cases; and we must admit that he makes out a good case in favour of the operation he advocates. We believe, however, that his success has been very remarkable; and whether the same reduction of mortality, viz. from forty or fifty to twelve per cent., may accompany the operation in the hands of others, remains to be proved. We do not mean by this observation to intimate that surgeons should be deterred from practising this plan of operating, which we do not hesitate ourselves to adopt, though with more reservation than Mr. Luke; on the contrary, we wish to guard the operation from the discredit which might naturally be attached to it, if found to fall short of the anticipated results. Sure we are that something, probably much, of his success is attributable, as implied by the author, to losing no time: and we have as little doubt that, were the more ordinary operation had recourse to with less delay than is usual, the fatality would be materially diminished.

We must find space briefly to notice the mode in which Mr. Luke operates. Femoral hernia is that form of rupture to which Petit's operation is most applicable: in inguinal hernia it is more frequently necessary to open the sac, in order to reduce its contents. In this latter form of rupture, the author first ascertains the exact seat of stricture (which he believes to be usually at the neck of the sac) by pressing the tumour firmly with one hand, whilst, with the fingers of the other, the spot where the impulse thus caused ceases, is clearly ascertained. The skin is then pinched up and transfixed with a bistoury, the incision being in the long axis of the tumour, and its centre opposite the stricture. The dissection is then pursued until the sac is exposed, when the thickened peritoneum, which is the seat of stricture, at the sac's neck is scarified so as to "weaken it, and place it in a position most favorable for yielding to the pressure of the taxis;" a delicate step we trow: and that it occasionally occurs, as remarked by Mr. Luke, that the sac thus treated gives way under the taxis, we can readily conceive. Where the external ring is the seat of stricture, Petit's operation is easy of performance; but difficult when it is necessary to divide the internal ring. In femoral hernia, the crural ring may be assumed as the ordinary seat of stricture, and the steps of the operation conducted accordingly. We will allow Mr. Luke to describe this operation in his own words:

"It will be recollected that there is almost always a groove or depression of the integuments, which defines the upper limits of the tumour upon the abdominal surface. Where no depression can be seen, the boundary of the tumour may be ascertained by the pressure of the finger in that situation. A fold of integuments is to be pinched up at that part, and divided by transfixing it with a narrow knife, so that the incision, when the skin is replaced, shall fall perpendicularly to the body, with its centre opposite to the depression referred to. By a few strokes of the scalpel, the tendinous expansion of the abdominal muscles is to be laid bare; after which a finger should be introduced as far as Poupart's ligament, between the tendinous expansion and the tumour, when the latter rises upon the former. The ligament being thus exposed, a hernia-director is guided under it by the finger into the femoral ring, the margin of which may be safely and easily divided in an upward direction with a common-probed bistoury, and the taxis applied in the usual way. Should the margin of the ring have formed the stricture, the taxis for the most part succeeds very readily, and the operation is completed in a very short time." (p. 112.)

If, however, it be found that such operation is not followed by relief of the stricture, then the source of strangulation must be sought for lower down, in the bands of fascia which are not unfrequently found crossing the neck of the sac about half or three quarters of an inch below the ring: these must be divided with a probed bistoury, using the nail of the left forefinger as a director, and insinuating it under the bands in question from above.

We must not extend this notice further, although we feel that we have by no means done justice to the subject in this brief review. We trust, however, that we may have succeeded in exciting the interest of our readers on the subject, and that they will be stimulated to peruse and study Mr. Luke's present paper, as well as its predecessor, if they have not already done so; we are sure they will be repaid for their trouble.

X. *A case of very extensive gelatiniform cancer of the peritoneum, involving the lymphatic glands of the abdomen and the ovaries, and simulating ascites;* by Edward Ballard, M.D., Physician to the St. Pancras Royal General Dispensary.

This case is remarkable for the rarity and extent of the morbid change, involving organs which many able pathologists have denied to be the seat of the disease in question. Thus, colloid of the liver is not admitted by Dr. Walshe or Cruveilhier; and the former writer has never met with it in the ovaries, and considers that it is insusceptible of "assuming such variation of internal arrangement as to call, in this point of view, for the establishment of varieties." Now the extent, character, and localization of the disease in Dr. Ballard's case, controverts these opinions, as will be seen in such brief notice of the paper as we can find room for.

The age of the patient was 55, when she first came under the author's notice at the St. Pancras Infirmary, in January, 1848. Ten years previously she had suffered from irregular but profuse catamenial discharge, which ultimately ceased when she was 51 years of age. About this time a medical man told her she had a tumour connected with the liver. In 1846 she had an accident, which produced prolapsus uteri, and soon afterwards observed that her belly began to swell at the lower part. In October, 1847, she began to vomit her food, and was treated for a time in King's College Hospital, but without benefit, her health becoming more impaired, and emaciation increasing. When first examined by the author, the abdomen was found to be generally enlarged and rounded, "the epigastrium very full, and the umbilicus remarkable as not being prominent, but stretched and flattened out." Several small tumours could be felt in the abdominal wall, and one, of a lobulated character and larger size, just above the pubes. There was the most perfect fluctuation in every part of the abdomen. Dr. Ballard's conclusions were as follows:—that there was ascites, and that the liver and abdominal wall were the seat of cancerous disease; that there was fibrous tumour of the uterus, and encysted ovarian disease.

The patient desiring to be tapped, and the operation being approved of by the author and his colleagues, a trocar was introduced just below the navel; but nothing issued from the canula but a teaspoonful of clear gelatinous matter; the same result followed a repetition of the attempt two inches higher up. On the second day after this she died.

When the abdomen was opened, as much as six quarts of very tenacious gelatinous matter were removed from the peritoneal cavity; and on turning aside the abdominal wall, the peritoneal lining was found infiltrated with colloid, "in the form of nodules, and little masses, intersected with little fibro-cellular bands, forming loculi." A similar infiltration affected the peritoneal investment of part of the liver, of the diaphragm, the spleen, and the greater curvature of the stomach. The omentum and mesocolic glands were the seat of a corresponding degeneration; and one large tumour of the same matter occupied the right side of the abdomen; whilst another extended from the iliac fossa to the margin of the ribs on the opposite side. This latter sac consisted of very dense fibrous tissue, divided into sacculi, being more or less vascular at all parts, and capable of containing not less than three pints of matter; there were several openings in it, communicating with the cavity of the peritoneum. Tubercular elevations of colloid, varying in size from a hemp seed to half a walnut, were scattered over the peritoneal surface; and portions of the small intestine were glued together by lymph, but not of recent production. There was one spot, on the cæcum, where the colloid mass had penetrated through the muscular coat of the intestine, "and could be seen through the thin layer of mucous membrane which covered it internally." The pancreas was free from disease. The uterus presented some fibrous elevations at its fundus; and some fibrous sacs occupied the place of the ovaries. On examination under the microscope, the characteristic cells of cancer—simple, compound, and caudate—were discovered.

We have anticipated many of the remarks of the author on this remarkable case in our introductory observations; and we believe with him that, in point of extent, this case has not its parallel on record. He considers, in contradistinction to the opinion of Dr. Walshe already cited, that three forms of the colloid species of cancer are clearly distinguishable in the present instance, viz. "the locular, the encysted, and the free or unincluded form." With respect to the singular deposit of so large a quantity of this colloid matter within the peritoneal sac, the author does not believe that this was derived from the burst ovarian cyst, though it does not seem very clear to us whether he considers it as the product of the diseased part of the abdominal wall alone, or of the general peritoneal surface. Lastly, he makes one practical remark, which we can fully confirm, from an analogous disappointment to that which occurred to the operator in the present case having happened to ourselves; it is this:—that the fluctuation of gelatinous matter cannot be distinguished from that of ascitic fluid. The instance we refer to was that of a large ovarian cyst, with most palpable fluctuation over every part, but in which the same phenomenon and same measure of disappointment followed the withdrawal of the trocar, as in Dr. Ballard's case: we did not repeat the experiment.

XI. *Cases of ununited fracture successfully treated;* by William Bousfield Page, Surgeon to the Cumberland Infirmary, Carlisle.

These cases constitute an instructive series, illustrative of many of the causes of non-union of fractured bones, and of some of the approved methods of promoting a more energetic effort on the part of Nature in restoring the continuity which had been so roughly solved. We promised, in an earlier article in the present volume, to take another glance at this subject, and we now proceed to redeem our pledge.

The first case is that of a young man, who, about seventeen weeks before his admission into the Cumberland Infirmary, had sustained several fractures and other injuries, from all of which he recovered, excepting a fracture of both bones of the left fore-arm, which had been broken at the junction of the middle and lower thirds, and apparently, from his account, neglected or lost sight of in a measure, in the attention which his other and more urgent injuries called for. The arm was much attenuated, and quite powerless, from the great overlapping and mobility at the seat of fracture; there was shortening to the extent of an inch and a half. Four months' trial was given, to see what a liberal diet, local stimulants, and entire rest would effect, but as no attempt at union was apparent at the close of this period, the author removed the ends of each bone with Hey's saw and the cutting forceps. Some difficulties seem to have existed at certain steps of the operation, but they were cleverly surmounted, and the arm was afterwards immoveably fixed in an angular splint, in an intermediate state between pronation and supination. At a later period, when the wounds were healed, dextrine splints were substituted, and in three months he left the hospital, the union being complete. Some months afterwards he came to exhibit himself, and stated that the affected arm was as strong as the other.

The second case was also one of complicated injury, in a lad of 16, both thighs being broken, and one leg. The fractured femora had united, but not so the tibia and fibula, which had not been kept properly at rest. Entire repose was therefore enjoined, and persevered in for two months, but in vain; the fracture was just as loose at the end of this time as when the splints were applied. Two setons were then introduced, each consisting of four silken threads, one over the outer, the other over the inner *surface* of the tibia. The inflammation excited by them necessitated the removal of the threads on the tenth day; the limb was again kept at rest for ten weeks, and at the close of that period union was complete.

In the third case (of a similar injury) the preparatory step of giving the limb entire rest for three months was effectual, evincing the importance of affording Nature a fair chance before resorting to active interference.

The fourth case is equally instructive. It is that of a poor, emaciated, ill-fed woman, whose humerus had been broken some weeks before her admission into the hospital. Rest and good food accomplished a cure in one month.

The fifth and last case was one of fracture of the tibia and fibula, in which the author believes that he traced the non-union of the broken bones to a syphilitic taint in the system; the patient had, indeed, a combination of primary and secondary disease at once, presenting the sore throat, eruption and nocturnal pains of the latter, and a recent chancre to boot. The system was brought under the influence of mercury, and union speedily followed. We are not quite satisfied that Mr. Page's conclusion regarding this case, that the mercury was productive of the desired result by combating the syphilitic poison, is strictly logical; but, on the whole, this collection of cases is very creditable to him.

XII. *On the irritability of the muscular fibre in paralytic limbs;* by Marshall Hall, M.D., F.R.S. L. AND E., &c.

This paper may be considered as a reply to that of Dr. Todd on the same subject, which was inserted in the preceding volume, and fully noticed by us in our account of it (Vol. I, p. 355). The explanation which we then offered, of the cause of the discrepancy between the experimental results obtained by Dr. Todd and Dr. M. Hall, is in full accordance with the subsequent researches of the latter; and we think that the matter is now in a fair way of being cleared up. It will be remembered that Dr. M. Hall's original statement was to the effect, that the irritability of the muscles of paralysed parts is *greater* than that of the sound muscles, in cases of cerebral paralysis, whilst it is *less* in paralysis dependent upon disease of the spine or upon lesion of the nervous trunk; the real import of this assertion being, that the muscles are excited to contraction by a feebler stimulus. On the other hand, Dr. Todd asserted that the muscular irritability of nearly *all* paralysed parts is *less* than that of the healthy parts;—that is to say, their contractions, under the influence of a powerful electric stimulus, are less forcible. We pointed out that these statements, properly interpreted with a knowledge of the senses in which the term "irritability" is employed by the two parties respectively, and with a due appreciation of the difference of their modes of experimenting, are by no means incompatible with each other; and this view of the case is identical with Dr. Hall's. The galvanic trough, he tells us, "is, from its simplicity of operation and its low intensity, really a test of the *irritability* of the muscular *fibre*," that is to say, of its readiness to respond to the slightest stimulus; "whilst the electro-dynamic apparatus, from its extreme intensity and mode of action, displays the *power* of the muscular *mass*." The following case exhibits the result of the employment of both instruments.

> "In a case of hemiplegia of three and a quarter years' duration, in a man aged twenty-eight, the arm being slightly wasted and the hand contracted, the leg only a little enfeebled, we at first applied the current from the common Cruikshank's battery, the hands being placed in salt water in one basin, the feet in another: on using a small number of plates, the muscles of the paralysed arm were found to be slightly affected by a current which did not influence those of the unaffected arm; on augmenting the number of plates, both arms were moved, but the paralytic arm more than the other. We now substituted Hearder's electro-dynamic apparatus for Cruikshank's battery: both arms were moved, but the unaffected more than the paralytic arm, the muscles being more powerfully contracted, as observed both by the sight and touch; a difference which became still more obvious as the power of the apparatus was augmented." (p. 152.)

Dr. Hall then returns to his former position, that this difference of irritability between the paralysed and unaffected muscles is so characteristic of the seat of the paralysis, as to be of value as a means of diagnosis; and he relates several cases in illustration, of which we shall select the following.

> "Mrs. ———, aged thirty-five, affected with extreme paralysis of the right side of the face. The right eye could not be closed by the most strenuous efforts, but remained widely open; the right side of the face remained flaccid, whilst the left was drawn into forcible contraction on any attempt to speak, and under the influence of effort or emotion. The case was regarded by all present as paralysis of the facial nerve. We applied galvanism from five, ten, fifteen, and twenty plates of Cruikshank's battery: our surprise was great to observe that, although the left or unaffected eyelid was only, or most, moved, it was the muscles of the right or paralysed side of the face which alone were thrown into a state of contraction. The current was directed from the forehead to the chin, and from temple to temple, first from right to left, and then from left to right; still the effect was invariably the same. Now in every case of paralysis of the facial nerve, it is the muscles of the unaffected side of the face which are most susceptible to the galvanic influence. In the present instance, this was the case with the orbicularis only. On further inquiry, we found that the attack came on rather suddenly, with uneasy feeling—a sense of confusion—about the head, the noise as of a cricket in the right ear, and numbness in the right side of the tongue. It became plain that it was a case of cerebral paralysis; and although its precise nature still remained obscure, two things were obvious enough,—first, that it presents an instance of pure paralytic affection, uncomplicated with spasm, in which the fibre of the paralysed muscles is more irritable than those of the unaffected muscles; secondly, that it is not a case of paralysis of the facial nerve; and thirdly, that its diagnosis was made out by means of the galvanic trough." (p. 156.)

Another case of somewhat the same nature is subsequently related (p. 159). The paralysis was confined to the left leg; it had come on gradually, and without any sudden seizure, fifteen months before; and it had induced debility and atrophy of the glutei, and of the muscles of the thigh and leg, with diminished temperature and circulation, the limb being thin, cold, and livid. It was supposed that the case was one of spinal paralysis; and it was expected, therefore, that the muscles of the paralytic limb would be less readily affected by the galvanic influence than those of the unaffected leg. The reverse, however, proved to be the fact; and upon carefully interrogating the patient, it now appeared that in the commencement there had been slight tingling of the left hand; from which it might be fairly concluded that the seat of the paralysis was really in the brain. In this case, also, the effects were completely reversed when the electro-dynamic apparatus was employed in place of Cruikshank's trough; the muscles of the unaffected limb being thrown into violent contraction, when the paralytic limb was scarcely moved. Here too, therefore, the application of galvanism afforded a valuable diagnostic indication.

Dr. M. Hall gives some needful cautions as to the performance of such experiments. Thus the result is often complicated by the timidity of the patient on the first application of the galvanism; and the effect of surprise and of sudden sensation, in wincing, starting, &c., must be carefully distinguished from that of the galvanic influence on the muscular fibre. Again, the galvanic force should be the mildest which can produce an obvious effect, and should never be such as to give pain, or to produce a violent jerking of the limbs. Further, as the first effect of a paralytic seizure is one of shock, *time* must be allowed for the restoration, as well as for the augmentation, of this property. In some cases there would appear to be no such restoration.—He also proves that the greater movement of the paralysed muscles cannot be dependent upon reflex action, as suggested by some. Such movements may be limited to a single muscle, either in the face or in a limb, and are perfectly dissimilar from all reflex movements, to the experienced eye,—these last being movements of groups of muscles, and of a totally different character. Again, if a frog be decapitated, so as to destroy sensation, and the two inferior extremities be subjected to the same galvanic influence, after the crural nerve of one of them has been divided so as to prevent all reflex action, the movements of the two limbs are the same. And in a hemiplegic patient, the muscles of whose paralysed arm and fore-arm were caused to perform marked contractions when the current was passed through them, these muscles remained unaffected when the current was passed through the fingers and hands only as far as the wrist, the muscles of these parts only being thrown into contraction,—whereas, if the contractions had been of a reflex nature, the former would have not been thus excluded.

Whilst we thus fully recognise the correctness of the facts adduced by Dr. M. Hall, and admit the value of this new means of diagnosis (which, however, even on his own admission, is by no means constantly to be relied on), we must still express our dissent from his theoretical interpretation of these phenomena,—namely, that the irritability of the muscles is derived from, or directly dependent upon, the spinal cord. Nothing can, to our minds, be more convincing than the proof afforded by Dr. J. Reid's experiments,* that the muscular irritability is the proper endowment of the tissue itself, depending (like other vital properties) upon its state of nutrition, which in its turn is regulated, *ceteris paribus*, by the degree in which the muscle is exercised. Dr. Reid has shown that the irritability of muscles may be maintained for any length of time after the complete division of their nerves, if they be duly exercised; and he has further shown that it may be recovered, under the same circumstances, after it has been entirely exhausted. How, then, is it possible that this property can be derived from the spinal cord? Dr. Hall has never, so far as we are aware, attempted to explain these results; but persists in his old theory, as if they had never been presented. It is not difficult to explain all the facts adduced by him, in accordance with the doctrine which we advocate. For it is obvious, as Dr. Reid has shown, that the continuance of the connexion between the muscles and the spinal cord, after the influence of the brain has been withdrawn, will tend to sustain the nutrition, and therefore the irritability, of the muscles, in consequence of the occasional excitation of reflex contractions in them.† On the other hand, as the *irritability* is not liable to be exhausted by cerebral influence, it will tend to increase, so that contractions will be occasioned by slighter stimuli; at the very same time that the whole *power* of the muscle is undergoing diminution, in consequence of its impaired nutrition.

XIII. *Case of extra-uterine fœtation;* by Donald Dalrymple, Esq., Surgeon, Norwich. Communicated by John Dalrymple, Esq.

There are some circumstances associated with this case, which have induced the author to conclude that the fœtation was not an extra-uterine one originally, but that the fœtus escaped from the uterus at about the fourth month of pregnancy, this organ being ruptured at that period. Thus much we gather from the concluding remarks of Mr. Dalrymple; we cannot say that a careful perusal of the case has led us to the same inference; but a brief narration of it will enable the reader to judge for himself. The patient believed she became pregnant early in January, 1847, and was first seen by the author's partner in June, who found her uterus of the size of a full-grown fœtal head, hanging out of the os externum; she complained much of the movements of the child high up in the abdomen. At the close of September she had labour-pains, which ceased after a few hours, and with them the movements of the child likewise ceased. After a few days she diminished in size, and improved in health. In December she again became ill, suffering from dyspnœa, and pain and enlargement of the abdomen; she died towards the close of the month. On examination, a large, full-grown, dead fœtus was found occupying a cavity lined by a thick layer of soft, yellow lymph, and stretching from the left hypochondriac region, where the head was, to the right iliac fossa where the feet were lying. The cord was traced to a maroon-coloured, spongy mass (we presume the placenta) occupying the region of the pubes, and adherent, apparently, to the uterus. The dissection could not be accurately made, nor the parts removed.

---

* Physiological, Pathological, and Anatomical Researches, p. 7.

† Op. cit., p. 13.

Now, the facts on which the author founds his opinion, that the case was not one of original extra-uterine conception, are these: 1. The cervix uteri arrived at the condition of the fourth month of pregnancy, and was not afterwards altered in its character. 2. That about the time she believed she quickened, she lifted a full pail of water, after which her feelings became different from what she had experienced in her former pregnancies. 5. Her husband stated, after her decease, that at the close of April (soon after the pail was lifted) during coition, "she suddenly felt something give way within her, and that, immediately afterwards, both of them perceived, on placing the hand on the umbilicus, a strange, fluttering sort of movement, from which time she always referred the movements of the fœtus to a position higher than before." We cannot but regard this evidence as quite inconclusive of the inference drawn from it by Mr. Dalrymple; to say nothing of the absence of all symptoms (none are mentioned) which might be expected to attend so serious a lesion as that of a uterus in the fourth month of pregnancy, and thus to constitute more direct and positive evidence in favour of the author's opinion. We repeat, therefore, that, in default of stronger evidence, we regard the case as one of original extra-uterine conception; and as such it is interesting.

XIV. *Observations on some peculiarities of polypus of the uterus;* by Charles Locock, M.D., First Physician-Accoucheur to the Queen, &c. &c.

The practical hints of experienced men in the treatment of diseases which are met with every day, are always valuable; and in that light we regard the suggestions of Dr. Locock in the present short but graphic paper.

He states that he has met with many cases of serious and dangerous hemorrhage from the uterus, dependent on the presence of a very small polypus, which eludes the notice of the practitioner, and which, in fact, cannot be detected with the finger, in consequence of the os uteri being closed against its introduction, unless at the period of the attacks, when the mouth of the womb is more open and flaccid. In one instance, where the bleeding had well-nigh proved fatal, and the ordinary remedies had proved ineffectual in restraining the hemorrhage, the author succeeded in "picking off, or digging out" the polypus with his finger-nail, and the patient speedily recovered. The difficulty, however, of reaching such small polypi, when situated far within the cervix, led him to adopt the use of an artificial nail, in the form of a sharp scoop, or small, fine carpenter's gouge, inclosed in a canula, from which it may be made to protrude to a definite distance by a regulating screw (a woodcut illustrates this description). This concealed scoop is passed through the os uteri, and then projected to a sufficient extent to enable the operator to cut through the peduncle of the polypus, by giving the instrument a rotatory movement. The only difficulty which strikes us in connexion with the use of this ingenious contrivance, is the discovery of the polypus; for though the cutting edge may represent the nail of the operator, we take it that he cannot impart the sense of touch to the canula; and the neccssity for the substitution of this instrument in place of the nail, is a proof that the diseased growth is beyond the reach of the finger. We wish Dr. Locock had explained how he manages this, as we presume he would not cut at random, albeit the uterus will bear a good deal without resenting the injury. Bodies as small as a bean or a pea must, we should have apprehended, be difficult to find and cut off in the manner recommended.

Dr. Locock favours us with some further remarks on the treatment of larger polypi. Those of moderate size he prefers excising with curved scissors or bistoury, having previously employed torsion (as suggested by Sir B. Brodie) to prevent the risk of hemorrhage. In very large polypi he considers it safer to apply a ligature first, and then, after the lapse of two or three days, to divide the diseased growth through the neck above the ligature.

XV. *Remarks on unnatural descent of the womb, with a proposed new instrument for its alleviation;* by James Reid, M.D., Physician to the General Lying-in Hospital, &c.

Dr. Reid is quite correct in believing that any practical and efficient suggestion in the treatment of this distressing malady is a matter of interest to all branches of the profession; for cases such as he refers to are brought under the cognizance of us all, and we are sure that there are few who will not regard as a boon the invention of an instrument well adapted to fulfil the object proposed by the author of the present paper. It is to cases of simple descent of the womb, uncomplicated with disease, that he wishes to direct attention, and he rightly considers that the end of interference by mechanical means is not attained, unless the uterus be *sustained* in its normal position, and that too without interference with the usual duties required of the patient. The requisites the author enumerates are, that the support should be constant, that it should be as little prejudicial as possible to the soft parts, and be removable at pleasure and with facility; lastly, that it should not interfere with the proper functions of the sexual organs.

Dr. Reid makes some remarks on the obvious and acknowledged insufficiency of ordinary pessaries, and the serious consequences occasionally accompanying their careless use, and then proceeds to comment on the vaginal supporter of Professor Kiliau (of Berne), who considers that the uterus need not be cared for, and that the support should be applied to the upper wall of the vagina alone. It was this instrument which originated the idea of that which Dr. Reid submitted to the notice of the Society, and of which there are two illustrative woodcuts, that are, indeed, essential to the clear comprehension of this piece of mechanism. Before describing the instrument, we may remark that the principle on which it acts may be illustrated, as the author observes, by its resemblance "to the easy introduction of two *closed* fingers within the vulva, which, after passing through it, are *separated*, and applied to either side of the cervix uteri, so as to hold it up in its proper position." There are two forms of the instrument; and when complete, each has very much the appearance of a long pair of castanets connected at their narrow extremities. Each instrument consists of two steel springs, narrow at the ends where they are joined together, and one inch and three quarters broad at their free extremities, being convex externally, and concave within, and protected with cork covered by elastic gum: the open extremities of these springs are separate about two inches from each other. When pressed together, these broad extremities allow of ready introduction within the vulva, and are then permitted to expand, and are applied on either side of the cervix uteri. In the more complex instrument of the two, there is an additional arrangement for facilitating the introduction and removal of the support. Dr. Reid seems to have been most successful in his treatment of prolapsus with this simple and ingenious apparatus: its merits, we cannot doubt, will recommend it to genera use.

Here for the present we must pause; as the number and interest of the remaining papers necessitate the postponement of our notice of them until another opportunity.

Art. XI.

On Bandaging and other Operations of Minor Surgery. By F. H. Sargent, M.D.—*Philadelphia*, 1848. 12mo, pp. 380.

This little book, clearly and intelligibly written, evidences great personal experience in the subjects to which it relates.

Its chief merits, for it does not profess originality, are a judicious selection from the writings of others, and an air of sterling good sense and honesty, which on the one hand makes it very readable, and on the other claims our respect for its author as a candid and judicious compiler. He has made no attempt to gain credit for improvements at the expense of others; but, on the contrary, shows an evident desire to state everything fairly, and to give references wherever they are required.

The book is printed in a type, and on a paper, superior to those of most of the productions of the American press; and it abounds in illustrations, which, albeit mostly copies, are not always felicitous.

It is not, however, on these accounts that we are anxious to bring it under the notice of our readers; these are characteristics common to many works of a higher scientific value, and in other respects more worthy their attention; but we do so because we recognise in it a step towards supplying a want in a particular kind of instruction, that is felt by many a student of surgery at the outset of his career—a want unsupplied by systematic treatises, and inadequately cared for by lecturers on surgery.

Some experience amongst students has convinced us that the simpler matters of practical surgery are too much neglected in the present day, and that there is a craving for the purely *theoretical* at the expense of the *useful*. The fashion truly has changed since Abernethy taught "the Big Wigs at the College" how to make a poultice; but we question if, in all respects, it has changed to a better or a more efficient one.

The object of all professional education should be, to fit a man for that situation in life which he is hereafter to occupy. One individual may be destined to cultivate, almost exclusively, the more scientific parts of the profession, and to him it may be a matter of little consequence whether he be or not a good *dresser;* but to the vast majority it is, and necessarily must be, otherwise. That knowledge and those acquirements which go to constitute the skilful dresser, are not the *minor* matters they are so often considered. A clumsily-adjusted bandage has been the knell of many a man's success.

We think it would be well if some of those manipulations which are commonly intrusted to sisters and nurses were undertaken by students, who would feel the benefit of practising what may seem easy enough of performance until experience has taught otherwise. Few of the students in London would hesitate in describing how to strap and bandage an ulcerated leg, or would fail in correctly stating the rationale of the process; but we are much mistaken if a large proportion of them could carry the measure into execution with any degree of neatness. So it is with many other things: description is but a small step in the process; without repeated practice they cannot be learned, and the opportunity, if neglected, never returns.

These few observations, with which we preface our notice of Dr. Sargent's book, relate to a subject of great importance, and are made in a spirit of sincere love for the best interests of the profession. We might easily extend them, but we prefer not to do so, confiding in our belief that what has been said will find an echo in the breasts of a number of Hospital Surgeons, who do not see unmixed good in a system of scientific cramming which excludes these *minor* details as beneath its notice.

Dr. Sargent divides his subject into five sections.

The first seventy pages are devoted to an account of the more common surgical instruments and surgical dressings; to bathing, fumigation, and the use of disinfecting agents.

In the second part there is a full description of bandages and their application.

The third speaks of the mechanical treatment of fractures; the fourth of dislocations; and the fifth section includes the minor operations of surgery, such as bleeding and the use of the catheter. A short chapter on the means of diminishing pain during operations, a list of useful formulæ, and a copious index, conclude the volume.

A work like the one before us, whose data are facts obtained by observation, and acknowledged by all, presents but small food for criticism, and requires no detailed notice from us.

Our observations must be few, and confined to those points which have struck us most in the course of its perusal.

In the first division of the subject there is a detailed account of the *douche,* which is a mode of using water that is hardly sufficiently estimated in England. In hysterical contractions of the limbs, its value is well known to surgeons; and as a means of producing counter-irritation, it is extensively employed on the continent. We remember being present at a painful spectacle, witnessed doubtless by many of our readers also, in the madhouse at Berlin, when a number of wretched imbeciles were undergoing the application of the cold douche to the spine. The sufferers described the sensation as of a fiery ball rolling along the spine, and producing a burning pain, proportioned in intensity to the length of the application, until at last, as we witnessed, the torment becomes intolerable. Who that has once seen will ever forget the poor, shivering maniacs; or the doctor smoking his cigar; or the dark, unwholesome cellar, its walls covered with the green vegetation of damp; or the crazy tub in which the patients sat; or the brutal men at the forcing-pump?

No remarks occur to us in reading the chapter on bandages, we can only concur with Dr. Sargent, that "there are few of the ends ordinarily to be attained by bandaging, which may not be gained by a skilful dresser with the simple roller." Every individual case requires some modification in the adjustment of its bandage; and this circumstance, while it detracts from the value of systematic treatises on bandaging, proportionally increases the necessity of constant practice in their application.

It appears to us that our author is a little too mechanical in his treatment of fractures. There are few facts of which the generality of modern surgeons are more convinced, than of the superiority of treating fractures with a simple apparatus, and a sparing application of bandages. Most of them, too, join in condemning weights and pulleys as something worse than useless, except in very rare cases.

The notion is a very strange one that regards muscles as analogous to cords pulling against each other—the strongest having the advantage; and we think we can trace the germ of many an evil in practice to the expressions *extension* and *counter-extension.*

The reader will find some excellent observations on the treatment of fracture of the clavicle, which is an injury so frequently followed by deformity, that is especially to be lamented among females, as to require the most serious attention on the part of the practitioner. The clavicle is so immediately subcutaneous, as to be readily acted upon by direct pressure, with strong plaster and dossils of lint; and this treatment, together with the figure-of-eight bandage, has appeared to us to afford more satisfactory results than any other.

We are glad to meet with Dr. Sargent's views on the occurrence of fracture of the lower end of the radius, simulating dislocation of the wrist; and the more so, because we have been surprised to find Mr. Lizars speaking of the latter accident as a common one, an opinion in which few practical surgeons will coincide.

Dr. Sargent makes no mention of his countryman Dr. Jarvis, who was here, a year or two ago, with an ingenious apparatus for reducing dislocations of all kinds.

The chapter on bleeding presents nothing calling for remark; we cannot agree, however, in any commendation of the spring lancet, an instrument, we believe, wholly discarded from practice in this country.

There is a very proper caution inculcated by our author, on the subsequent employment of leeches that have been used to abstract blood from persons suffering under infectious or malignant diseases. At an institution in which leeches were twice used, we are informed that several serious cases of infection occurred; resulting in the formation of abscesses and other troublesome symptoms.

Dr. Sargent, on the authority of Dr. Buschenberger, speaks highly of matico, both as a local, and also as an internal remedy in the form of infusion, in cases of obstinate bleeding. We had reason to think it very serviceable in a case of recurring and uncontrollable hemorrhage from a wounded vein.

The chapter on the use of the catheter is very meagre, and not commensurate with the importance of the subject. It is the least satisfactory of any in the volume.

The brief observations on the means of diminishing pain during operations contain nothing new, and might as well have been omitted.

On the whole, then, we have been much pleased with Dr. Sargent's little book. It is methodically arranged, and simply written; two points especially important in an elementary work, and calculated very greatly to increase its value.

ART. XII.

1. *A Treatise on the Practice of Medicine.* By GEORGE B. WOOD, M.D., Professor of Materia Medica and Pharmacy in the University of Pennsylvania, one of the Physicians of the Pennsylvania Hospital, &c. In Two Vols.—*Philadelphia,* 1847. Royal 8vo, pp. 792, 840.
2. *Clinical Lectures on the Practice of Medicine.* By ROBERT J. GRAVES, M.D., M.R.I.A., &c. &c. Second Edition, edited by J. MOORE NELIGAN, M.D., M.R.I.A., Physician to Jervis Street Hospital, Lecturer on the Practice of Medicine, &c. In Two Vols.—*Dublin,* 1848.
3. *Clinical Lectures, delivered in the Theatre of Mercer's Hospital during the Session of* 1847-8. By JAMES F. DUNCAN, M.D. T.C.D., Assistant Physician to the Hospital, Fellow of the King's and Queen's College of Physicians, &c. &c.—*Dublin,* 1849. 8vo, pp. 122.

THERE is a demand for a species of literature which, we think, has never been fully comprehended and provided for. The freedom of inquiry and discussion enjoyed by the cultivators of medical science, causes the press to teem with new views, new theories, new facts; and with compilations and amalgamations of the old and the new in science. The latter arise, of course, out of the former, and are intended to satisfy the demand alluded to. To those who are acquainted with the details of professional life, and who therefore know how little the majority of practitioners study the new or current literature of the day, it can be no matter of surprise that a brief condensation or compilation of its main results is a desideratum in the profession, and that works professing to attain this object enjoy a decided preference over those of a more philosophic and original character. The latter is, in fact, the raw material which the Editor of the half-yearly 'Retrospects,' or 'Abstracts,' or 'Year-Books,' or 'Encyclopædiacal Résumés,' must digest or spin into an "article" fit for immediate appropriation. All our periodical literature is in reality subservient to this demand, in a greater or less degree. There is another class of writers, however, who seek to attain the same object by a different method, and on different principles. These are the 'systematic' writers; and they differ from the periodical compiler in this, that while he assumes his readers to be acquainted with medical literature up to the period at which he publishes, and the literature of the past twelve months consequently limits the field of his operations, the systematic writer goes back to the earliest ages, and the doctrines of medical worthies reappear in due order, from Hippocrates to Graves or Watson. Works of this kind have been undertaken with very various talent and on very different plans. In the first place, there is the compendium by one hand, such for example, as Brook's 'Practice of Physic' of the last century, or Craigie's 'Elements of the Practice of Physic' of this. Some of these ambitiously attempt a new pathology or nosology; Mason Good may be mentioned as an example; others have given their compilation the form of a dictionary or encyclopædia. This plan permits of a division of labour, and the wide domain of medical science and art has been parcelled out amongst a select band. The 'Dictionnaire des Sciences Médicales,' and the 'Dictionnaire de Médecine et de Chirurgie Pratiques' of a more recent date, each with some of the most eminent names in French literature on its title-page and on the list of its contributors, are good examples of this class of publications; the 'Encyclopædia of Practical Medicine,' and the 'Library of Medicine' of our own country, are also examples of the same kind. German literature, we need scarcely say, abounds with such. Although this plan, by admitting of a division of labour, facilitates the "getting up" of works of this class, it has this defect, that there is a want of unity, of method, and of equality of merit. Hence some individual, more adventurous than the rest, has occasionally undertaken the Herculean labour of writing the whole himself; such are James's 'Medicinal Dictionary' of the last century, and Copland's 'Dictionary of Practical Medicine' of this. We need hardly point out the objections to this plan, as the course of publication of the last-mentioned work has sufficiently demonstrated them. If the editor be determined to do justice to his undertaking, he may well become old before his task is done, seeing that the reading and experience of a life must be one of the requisites to its successful fulfilment; or if by unflinching industry and unwearied perseverance he succeeds in completing his work before the snows of age chill his energy, still the course of research and discovery is more rapid than his own, and by the time that he has fairly done with the letter L, he must begin anew, and recast the articles under letter A, as they are, for the most part, sufficiently behind the time to render their imperfections manifest.

This remark applies to the systematic 'Treatise on the Practice of Medicine,' by Dr. Wood. The author informs us that he has devoted the leisure of several years to the preparation of his work; but with the acknowledged facilities which the 'Dictionnaire de Médecine,' the 'System of Practical Medicine' edited by Dr. Tweedie, and other works of his own class have afforded him, Dr. Wood has not been able to keep up with the current, for, indeed, his principal storehouses alluded to, were themselves rapidly becoming the storehouses of views and facts effete, corrected, or abandoned.

There is a class of treatises on the practice of medicine, of which Dr. Graves's work is a good example, which occupy a medium position between the 'systematic treatise' and the 'Retrospect.' While the writer assumes a knowledge on the part of his readers of the written or recorded tenets of the medical art, and while he draws from his own observations or the observations of others new stores of information, he is not content with a mere statement of what is new, but he enters upon a critical analysis of what is written and received generally, and a comparison of it with more modern views and observations. This plan includes the advances made in physiology, and the application of them to improved theories, to improved methods of treatment, or to the correction of what is erroneous in either respect in our written experience.

Such a plan demands great and varied attainments on the part of him who undertakes to fill it up. He has, indeed, greater labour entailed upon him, if he would *perfectly* attain his object, than is entailed upon the compiler; for to the most varied and extensive literary research, there must be added an intimate and sagacious experience of the phenomena of disease. The perceptive, reasoning, and meditative faculties must be brought into united vigorous operation, a combination rarely indeed found to exist in nature, and still more rarely in connexion with sound physiological knowledge, or rather with what may be termed the *physics* of medicine; for physiological and pathological chemistry daily exercise a wider and wider influence on the theory and practice of medicine. We were not therefore surprised to find that Dr. Graves had not attained to this perfection of fitness; but we do think we might fairly confess to a surprise that that eminent and sagacious physician should have permitted the following to go forth to the world. Dr. Graves places chemistry in the same category with botany, and thinks too much attention is paid to it, whether it be considered in reference to the choice and prescription of medicine, or the analysis of poison, adding—

> "As to any benefits derived from analytical chemistry in solving the problems of vital action, or elucidating the functions of the various organs in health and disease, they may be said to be few, and unimportant and inconclusive. . . . . Chemistry has failed most remarkably in revealing the arcana of life; and, notwithstanding all her boasted discoveries, we are still very little in advance of those who practised the healing art some centuries ago." (Vol. i, p. 20.)

Some centuries ago! Suppose we say two centuries, and take up a medical work of 1649—what comparison can be instituted between the doctrines and practice of a physician who knew nothing of the components of the atmosphere, and their relations to the blood, and to the tissues through the blood—and the doctrines and practice of one whose whole mind is thoroughly interpenetrated with a science, the principles of which are to us so simple, and yet as compared with the infancy of "some centuries ago," so grand and so glorious in their results? How many fatal errors in hygiène and regimen have not those principles dispelled! That the extreme statement just quoted is not made simply to round a period, or to excuse a neglect of the science so undervalued, is manifest from other remarks of a like nature.

Although Dr. Graves has not that ideal perfection which is required for the production of a complete work of the kind he has undertaken, and although his labours must soon become but a storehouse for a more advanced successor, the work is most valuable in itself, and will, we feel certain, maintain the popularity it has already obtained.

The success of a periodical must in some degree be dependent upon the same circumstances which give popularity to works of the class last alluded to. The editors or contributors must not be content with a dry analysis or an elaborate criticism, but must add to the stores of recorded knowledge before them from their own reading and experience, and render that which is really meritorious in the new more acceptable to practitioners by an estimate at once philosophical and practical. In some such way, as well as we may be able, we propose on this occasion to notice the works before us.

The first five hundred pages of Dr. Graves's first volume are occupied with considerations as to the various forms of fevers; and, amongst these, the maculated or exanthematous typhus, or, *par excellence*, the Irish fever, occupies the most prominent position. Since the publication of the first edition of Dr. Graves's work, that contagious fever has broken from its usual limits, and crossing not only the Irish Channel but the broad Atlantic, has made itself painfully manifest in localities to which it was previously unknown. The famine of this century will hold a prominent position in medical as well as in political history. It produced epidemics, as, for example, the scurvy, which had been witnessed by none of the living generation; and in Ireland and other portions of the United Kingdom, led to scenes of devastation and destruction as appalling as any of the middle ages. Let us take this picture of Cork:

> "The incursion of rustic paupers into the city still continues unabated, the only change being, that it is less observable, as they wait in the outskirts of the town until dusk, when they may be seen coming in droves, the bed-clothes strapped to the shoulders of the father, while the children carry pots, pans, jugs, old sacks, and other articles, . . . walking masses of filth, vermin, and sickness. They squat on straw in the principal streets, and teem in the lanes and alleys, fruitful sources of contagion and disease. . . . . On Wednesday a country woman deserted her child, which was a pitiful object, half-naked, and full of smallpox, and left it in the middle of Patrick street as a legacy to the citizens." (Vol. i, p. 95.)

In four months, from December 22, 1846, to the 24th of April, 1847, 2130 persons died in the union workhouse at Cork. The inmates were placed three, four, and five in a bed; and in the convalescent ward of the hospital, there were 45 beds for 120 persons. Such are a few of the facts connected with the disastrous famine of 1846, and the accompanying pestilence.

In saying pestilence, we do not mean to express the opinion that famine was, or ever can be, the *direct* cause of pestilence: but it is assuredly the indirect cause, inasmuch as it leads to overcrowding, and it is a direct cause, from the imperfect formation of blood to which it gives rise, why a contagious fever, previously harmless and sporadic, becomes much more fatal and much more prevalent. And if we take the meaning of the word pestilence in this sense, namely, as signifying a greater mortality and malignity, and a wider range, of an ordinary endemical fever, then famine may with perfect propriety be termed a cause of pestilence; and history demonstrates to us, with the greatest fulness of proof, that in this way famine is the cause of pestilence. It is by a parity of reasoning that famine is a cause of rebellion, insurrection, and civil and international wars; and inasmuch as famine is often accompanied by great and rare meteorological and telluric changes, as immense floods, hurricanes, intense frosts or heats, earthquakes, meteors, &c.—these changes being in fact the cause of the defective supply of food, it is not wonderful that immense social changes, like those witnessed during the last few years, should be deemed to have a direct relation to the elements, or that "plague, pestilence, and famine" should be linked with "battle and murder" in the prayers of the National Church, and connected in the popular mind with each other, and with mighty and mysterious changes in the earth and its atmosphere.

To some such changes the development and spread of epidemic fevers have been often attributed. We find, for instance, various electrical theories adduced in explanation of the spread of influenza and cholera. Dr. Graves attributes the increase of fever in Ireland, from time to time, to some general atmospheric change which affects the whole island simultaneously, independently of aspect, height above the level of the sea, dryness or moisture of the soil, or any other circumstance connected with mere locality; and (he adds) the fact is undoubted, that fever is neither so prevalent nor so fatal in any of the western kingdoms of Europe as in Ireland. The immunity from fever enjoyed by large English towns is in striking contrast with the liability to fever in Ireland; and the statistics of the two countries corroborate, Dr. Graves thinks, the opinion already made by him, that there is something in the climate of Ireland and the western parts of Britain which predisposes to fever, and from which the eastern and central parts are exempt:

> "It is curious, that, in those towns of England which have greater intercourse with Ireland, as Liverpool and Manchester, typhus predominates more than in others not similarly circumstanced. It was on this account, that Dr. Lombard concluded that maculated typhus fever was imported into England and Scotland by Irish labourers, who go over every year in such numbers to reap the harvest. But, from the statistical reports of Dr. Cowan and others, it appears that, as regards Scotland, this explanation is anything but satisfactory; and it seems more probable, that the West of England, Scotland, and Ireland, in which the climate is almost the same, possess the same combinations of circumstances which produce typhus. Nothing, indeed, can be more remarkable than the facility with which a simple cold (which in England would be perfectly devoid of danger), runs into maculated fever in Ireland, and that, too, under circumstances quite free from even the suspicion of contagion,—in truth, except when fever is epidemic, catching cold is its most usual cause." (vol. i, p. 88.)

The opinions of the profession are eagerly listened to by the public at large; and the establishment of the General Board of Health will soon introduce popular polemics into medicine and hygiène. Already no little ridicule and contempt are expressed for the contrariety of opinions expressed by medical writers; and we fear that this state of things will not improve as public opinion becomes more enlightened. The etiology of fever, and, indeed, of epidemics generally, is, we feel certain, as capable of being reduced to fixed and intelligible principles as any other branch of medicine; but we can see no hope of such a reduction, so long as writers of the eminence to which Dr. Graves has attained, give the sanction of their names to crude, illogical, and unphilosophical statements. We have quoted the above paragraph at length, for the purpose of critical analysis, and with particular reference to the etiology of fever; and we repeat, that, at this moment, there is no question more interesting to the profession at large, or one which more vitally concerns its reputation and estimation with the public, than this.[*]

We fully admit that the words crude, illogical, and unphilosophical, can rarely be applied to the views of Dr. Graves; but this is a reason why it is the more imperative on us to show their applicability to the views and statements of the preceding paragraph, and to prevent that and similar failures from having a deleterious effect on the minds of the reader.

The fever in question is the maculated typhus, which we considered at length in our First Volume.[†] In that article we showed that the disease was a specific exanthema; with Dr. Graves, we stated our full conviction as to its contagious nature, and also that, like the other exanthemata, it attacked the same person but once; and we stated our opinion that the fever was not in reality *peculiar* to Ireland, although much more prevalent in Ireland. Let us bring these considerations to bear upon one or two of Dr. Graves's statements in support of his views of Irish climatic peculiarity. Firstly, it is impossible to reconcile the preceding general facts with the statement contained in the last paragraph of the passage quoted. If those general facts be what they pretend to be, then it is as impossible that in Ireland a simple cold would more readily run into maculated fever, without even a suspicion of contagion, than in England; just as impossible with reference to exanthematous typhus, as in respect to scarlatina or variola. Exanthematous typhus can only arise from contagion; that is to say, however the predisposing or exciting causes may prevail, the proximate cause—a reception of especial febrific poison into the blood—is essential; unless, indeed, we admit the doctrine, that the disease is *continually* arising *de novo*, of which, however, there is no satisfactory evidence.

We are willing to admit the general fact stated by Dr. Graves, because we believe it absolutely correct, although there is some discrepancy in his statements. Thus subsequently he observes, that "the fact of fever not often spreading in families in this city, when brought from the country, proves that the causes which acted in the country were such as I have assigned," &c. Still the main point is, we believe, correct; and the direct inference is, that when in a population there is this proclivity to fall into exanthematous typhus from a simple cold, that population must be saturated, as it were, with the contagious *materies;* because each individual so attacked must necessarily have had the poisonous *materies* in his system, or else had fomites in close contiguity to him.

---

[*] The editor of the Journal of Public Health gives us, in the March number of that journal, a striking illustration of the above remarks. In a periodical, intended for popular circulation, and adapted to the popular polemics of medicine and hygiene, Dr. Milroy roundly asserts, that physicians generally are "wholly ignorant" of the causes "why, in some years, certain fevers are singularly mild, and evince but little tendency to spread; while, in other years, they are fatally diffusive over the length and breadth of the land," &c. For ourselves, we altogether repudiate this imputation of entire ignorance on so important a subject; and we think Dr. Milroy has been incautious, to use the mildest term, in imputing such ignorance to the profession in a periodical designed for circulation amongst the educated classes generally. The fact is, that the profession at large knows more on these subjects than Dr. Milroy suspects; and will, we think, read his own expositions with some surprise.

[†] Page 285 et seq.

Such being the condition of masses of the Irish, it is easy to see why the disease should be so readily imported into England; but Dr. Lombard's opinion is not only rendered probable by this inference,—it has been fully and fatally confirmed by appalling facts witnessed in all the principal towns in England. Scenes like those described as occurring in Cork have been repeated in Liverpool, Manchester, York, Glasgow, &c.; and although the supposed *climatic* influence of their country accompanied these miserable hordes of famishing men, women, and children, and they appeared to the astonished citizens of English towns in the interior, unaccustomed to such spectacles, as nothing else than pestiferous barbarians, the fatal effects of the poison were not limited to them. They, it is true, contributed an abounding majority of victims; but the contagion they imported struck down many medical practitioners, clergymen, Roman Catholic priests, relieving-officers, and nurses. Thus the importation of men having the seeds of the disease in them, and scattering the seeds about them wherever they went, sufficiently explains that mortality for which Dr. Graves seeks a reason in some mysterious combination of climatic circumstances.

The plain, unvarnished truth, when human life, and even human civilization is at stake, should be plainly told. The supposed climatic influences of Ireland are indeed none other than the habits and customs of a domestic economy, at which many tribes of barbarians would blush; of which the existence was often doubted by the English people because they were thought incredible; but of which the horrid reality has been too strikingly exhibited before their eyes, ever to be forgotten or perhaps excused by the present generation.

Dr. Graves is so fully convinced of the evils of over-crowding, filth, and famine, that we need not say one word as to these. We would only suggest that, in investigating the etiology of the fevers of Ireland, a stricter attention should be given to the scholastic differences of predisposing, exciting, and proximate causes,—to a more philosophical and minuter arrangement of facts, so that like shall be grouped with like,—and to a greater care in deducing inferences from loose statements or facts half observed, half imagined. For example, the "*entassement,*" or over-crowding, acts as a predisposing cause of fever in two ways. Firstly, when hundreds of individuals are crowded together during the prevalence of a contagious epidemic, its spread will be thereby facilitated, by a greater number of people being brought into a focus, and thus the chances of contact with contagion-bearing persons very much increased. Secondly, over-crowding may take place and there be no contagion-bearing individual present, but the morbific influence of an atmosphere loaded with emanations from the bodies of vast crowds is considerable, and a person coming within the sphere of contagion shortly after leaving such an atmosphere will be highly disposed to be acted upon by the febrific poison.

We very willingly pass from this feverish and depressing subject, to other matters treated ably by Dr. Graves, and find in Lecture 30 (we should prefer terming it a chapter), "On the connexion between diseases of different organs," a highly important and very practical question mooted, and a question not often entertained, we fear, by the routine practitioner; yet we know no one point in treatment in which practical skill may be so eminently exhibited as in detecting this connexion, when, leaving the more obvious, and perhaps more pressing symptoms, the physician attacks the more hidden but more real sources of the morbid phenomena. Dr. Graves often exhibits this kind of practical tact—this acute detection of some hidden point in etiology—and we should be inclined to think that much of his reputation is to be attributed to it. The following may serve as an example of Dr. Graves's method:

> "Let me first direct your attention to a train of morbid phenomena, sometimes observed coexisting with arthritic inflammation. A person, labouring under inflammation of the joints, gets an attack of hepatitis, accompanied by jaundice; and this is followed by urticaria. I have observed this sequence of disease in eight or nine cases. The first was a gentleman, residing in Lower Mount street, whom I attended with Dr. Cheyne. This gentleman, in consequence of exposure to cold, was attacked with arthritic inflammation and fever. After he had been ten days ill, he became suddenly jaundiced, and in a day or two afterwards a copious eruption of urticaria appeared over his body and limbs. Exactly the same train of phenomena, and in a similar order of succession, was observed in a man, treated in the Meath Hospital, in 1832. A short time before this, I had been attending a medical friend in Baggot street, who had been affected in the same way; and I mentioned to the class, as soon as I perceived the man was jaundiced, that he would most probably get urticaria. I made a similar prediction in a case which occurred recently in our wards, and it was verified by the event. Now this is not a mere fortuitous occurrence; the various symptoms must be connected in the relation of cause and effect." (Vol. i, p. 446.)

Dr. Graves is not happy, we think, in his unravelling of this series of phenomena. In the first place, nothing, he observes, has been longer recognised as an established fact, than the intimate sympathy which exists, both in health and disease, between the digestive organs and the skin; then acute hepatitis always produces more or less derangement of the stomach and alimentary canal, and *therefore* we may consider its connexion with urticaria in the same way that we are in the habit of viewing the cases so frequently observed, in which certain sorts of fish have produced serious symptoms of indigestion, followed by nettle-rash, and not merely by stomach complaints, but also by inflammation of the joints and rheumatic pains. Dr. Graves then quotes illustrations of poisoning by eating the flesh of certain fishes, in which the above symptoms were noted, and also paraplegia, or paralysis of the lower extremities, and thus establishes, as he thinks, the connexion between disease of the digestive organs and urticaria; but, in fact, are not the symptoms and phenomena referred to rather coincident than consequent? What proof is there (Dr. Graves does not even attempt the proof) that they are in sequence? It appears to us that they are the common symptoms which arise together from the action of a poison on the system; and we should just as correctly consider the ptyalism which supervenes after diarrhœa or tormina, from taking mercury, to be the consequence of the intestinal affection, as that the urticaria in the examples quoted is the result of stomach or bowel complaint.

It is remarkable that Dr. Graves takes, in reality, this view of the pathology, in an obscure way, but, to those who refer the phenomena of gouty inflammation to the presence of a poison in the blood, sufficiently intelligibly; at least, the obscure working of the imperfect idea in Dr. Graves's mind may be traced. It is, we think, one of the results of the hasty method which so-called clinical medicine sanctions, that the ideas are often obscure and imperfectly wrought out, and the inferences hasty and imperfect. In the following sentence we have the doctrine of arthritic metastasis applied to the phenomena in question, wrapped up in certain other notions only indirectly connected with it:

> "Every one has observed how frequently inflammation of the joints becomes, in its course, complicated with inflammatory affections of internal viscera. In general, those viscera whose component tissues are most similar to the articular, are the organs affected. Hence the heart and pericardium are so often attacked in the course of rheumatic fevers. It sometimes happens, however, although less frequently, that the internal organ attacked has little analogy in point of tissue with the joints. Thus, in rheumatism and in gout, the stomach, the bowels, the lungs, or the liver may become engaged; and of these, none perhaps so frequently as the liver. We need not be surprised at this, when we consider how intimately the digestive function is connected with arthritic inflammation, which is, indeed, generally preceded or accompanied by well-marked symptoms of hepatic or stomach complaints. Indeed, almost all medicines that afford relief in arthritis, are attended with well-marked symptoms of their having acted upon the secretions of the alimentary canal and liver. Thus, colchicum seldom diminishes the pain and inflammation of the joints, until it produces copious bilious evacuations." (Vol. i, p. 448.)

Nothing here said explains the series of phenomena observed by Dr. Graves. All happens as stated with regard to the abdominal viscera in gouty inflammation of a fugitive character, and yet there is no urticaria. Nevertheless, there can be little doubt that the latter symptom is metastatic, but whether it be a result of the antecedent hepatitis is much more doubtful than that it is a link in the chain of morbid phenomena having a deeper but common origin.

Dr. Graves mentions another series of morbid phenomena—venereal disease, abuse of mercury, periostitis, the establishment of the mercurial cachexy; and, finally, hypertrophy of the liver, ascites, jaundice, and death. Singularly enough, he observes, that this connexion has not been hitherto noticed by any writer, so far as he has ascertained. The impression on our mind is that nothing is so well known as this series of morbid changes. The disease of the liver is the well-known fatty degeneration. The state of constitution is closely allied to the scrofulous; and in examples of the latter kind, that is to say, of scrofulous fatty degeneration of the liver, as well as in that resulting from the combined effects of the venereal and mercurial poisons, Dr. Graves has observed an intermittent action of the liver, indicated in a curious way in a case quoted by him. His attention was directed to the phenomena by the patient, who observed the cylinder of fecal matter to be composed of parts differing in colour and appearance; two or three inches consisted of pale clay-coloured substance, and immediately after this another portion, of about the same length, was observed, presenting the ordinary bilious or brown colour of natural excrement; and then again another mass of clay-coloured matter, without any obvious trace of bile. This appearance Dr. Graves has frequently witnessed, and the conclusion he draws from it is, that in such forms of disease the functions of the liver are performed, as it were, intermittingly;

it secretes bile during a certain period of the digestive process, then stops, and then secretes again. We have reason to think, however, that this intermittence of action is a phenomenon common to most chronic diseases, and is usually of the tertian type. The marking of the fecal cylinder so closely observed by Dr. Graves could hardly take place in the course of a few hours, during a certain period of the digestive process; for most practitioners well know that the movement of the intestinal contents is slower in chronic cases than in health, so slow, indeed, that undigested particles of food have been satisfactorily ascertained to have been six weeks on their journey from the stomach to the rectum.

It is interesting to see how Dr. Graves's perceptive powers serve his purpose without the aid of the scientific (or, we would rather say, microscopic and chemical) investigations which he somewhat undervalues. Thus the fatty degeneration of particular tissues is a change in nutrition closely connected, physiologically, with atrophy,* and on this head Dr. Graves states:

> "It is also curious to observe, that the same cachectic state which gives rise to emaciation and decay of the body, generally occasions hypertrophy of some particular organ. What we most commonly observe in such conditions is, general wasting of the system, accompanied by increased morbid nutrition in certain organs." (Vol. i, p. 452.)

The sequelæ of scarlatina are noticed by Dr. Graves, and here again, we find acute and practised observation dissevered from a sound philosophy, and from a knowledge of written experience. Indeed, it is obvious that the former has been cultivated to the neglect of the latter; for, in fact, there is nothing new in Dr. Graves's statement under this head; while there is a total absence of any explanation of the series of phenomena, which, as is now well known to the more intelligent practitioners, belong to the same group as those of rheumatic or gouty changes, and to the same series of morbid phenomena. The general inflammatory diathesis which follows upon an attack of scarlatina can be traced to functional disturbance of the kidneys; and the most frequently fatal termination in these cases is by endocarditis, pericarditis, or effusion into serous sacs.

The connexion of disease of the liver with disease of the heart as a sequel of the latter is noticed also by Dr. Graves; we do not think, however, that any intelligent practitioner would fail to notice this hepatic disease, or to determine its true etiology. The *connexion of diseases of the spleen* with affections of other organs is less a matter of general observation; indeed, we suspect there are hundreds of practitioners who have never observed a single case of disease of that viscus during the whole course of their practice. This may arise partly from the circumstance that, with a diminution in the prevalence of agues, or in the cause of it, namely, malaria, enlargement of the spleen, and other chronic affections have become more rare. Be this as it may, English systematic writers have given only brief and imperfect descriptions of splenic affections, and have written without, apparently, any personal acquaintance with them. These remarks do not apply, however, to Dr. Wood, as is shown by the subjoined quotation from his work. The affection noticed by Dr. Graves in connexion with chronic enlargement of the spleen, is a superficial ulceration of the legs; and he observes that Aretæus mentions it, and in connexion with a peculiar cachexy. The patients lose their appetite, become cachectic and swollen, and the legs ulcerate, the ulcers being hollow, livid, sanious, and difficult to heal. Dr. Voight noticed the disease in Bengal, where the disposition to ulceration is so great that leech-bites and blisters occasionally give rise to foul or phagedenic ulcers. Dr. Graves might have mentioned Celsus, in addition to Aretæus, as noticing the great tendency to ulceration—"Ulcera aut omnino non sanescunt, aut certé cicatricem vix recipiunt." (lib. iv, cap. ix.) Morgagni mentions this symptom also, and we may add Mr. Twining to the list, in whose 'Clinical Illustrations of the most important Diseases of Bengal,' this tendency to ulceration and hemorrhages is particularly mentioned. Dr. Wood touches, we think, upon the true pathology of these symptoms and the accompanying cachexy, in noticing their analogy with the symptoms which mark the scorbutic diathesis:

---

* Lectures on Atrophy and Hypertrophy, in London Medical Gazette, vol. ii, 1849, by Professor Paget.

> "As chronic splenitis prevails in miasmatic regions, it is often associated with a peculiar cachectic condition of system, with a depraved constitution of the blood; though it is difficult to say, how far this state of health bears to the splenic disease the relation of cause and effect, and how far they are both coincident results of the same morbid cause acting upon the whole economy. It is probable, on the whole, that both the disease of the spleen and the cachectic state of the system are common effects of the same cause, though they may tend to sustain and aggravate each other. The constitutional affection alluded to is characterised by a pale, anæmic condition of the skin, which is also sallow, dry, and shrunken; by general lassitude, inertness, muscular weakness, and mental depression; by great liability to hemorrhages, and to sloughing ulcers of the legs from slight injuries; by pains in the extremities, sometimes followed by collections of pus; by amenorrhœa in the female; and, in general terms, by all the symptoms which mark the scorbutic diathesis." (Vol. ii, p. 517.)

Up to a recent period the spleen has been considered of secondary importance in the animal economy; and, perhaps, while all the functions go on regularly, and while, especially, there is no morbid condition of the blood, this view may be admitted. But if morbid conditions arise, the relations of the spleen to the organism may be much modified, and it may play a much more important part than in health. There is a well-marked morbid state of the blood constantly coincident with a morbid condition of the spleen, a condition allied to that observed in scorbutus; but we must not take the extreme examples of either of these morbid conditions as the best illustrations of the diseased state. Scorbutus more frequently exists without the more prominent symptoms of vibices and hemorrhages than with them. There is a dull, waxy pallor of the skin, paleness of the gums, weariness and lassitude, indigestion, or gastrodynia, and often pain in the left side, when scorbutus might be expected, and which symptoms are, in fact, those of the primary stage of the disease. So, also, with regard to the chronic affection of the spleen. The intimate connexion between it and malaria justifies the term malarious cachexy, in instances of this kind; and it must not be forgotten that smaller doses of the poison may produce less strongly marked symptoms, and we may have only debility, palpitation, waxy greenish pallor of the surface, a watery condition of the blood, and all the morbid states resulting therefrom. The malarious poison stops short in its action of those more fully-developed phenomena produced by full or large doses. Such a state of things is very likely to occur in our own temperate climate and improved soil, and such we suspect may be the true pathology of chlorosis. Nay, even the gastric pain and the stitch in the left side can be attributed, with perfect propriety, to the malarious poison, for, in small doses, gastric neuralgia is one of its most common results. Taking this view of the subject we have often been enabled to treat cases of mere gastrodynia with the greatest success. Nor is this view of the pathology of chlorosis confined to the general resemblances between it and splenic affections; for chlorotic females of a lax fibre are not unfrequently subject to small round superficial ulcers of the legs, having a glairy sanious surface, healing with the greatest difficulty by the ordinary means, but yielding to a course of quinine and chalybeates. In thus referring to chlorosis, we do not mean to include, under that term, cases of mere anæmia, but that true *green* sickness, in which the colour of the skin resembles that observed in chronic splenitis; and this condition differs essentially from that anæmic condition observed often in young females when the skin is of a waxy, *white* tint. Dr. Graves concludes his lecture by observing how great is the advantage which the physician gains from a knowledge of the connexion between the diseases of different organs, how much precision it adds to practice, and what facility it gives to prognosis. In these observations we cannot but cordially concur; and we trust to find the rising generation of practitioners imbued more and more with a spirit of inquiry, which avoids the trammels of scholastic definitions and arrangements, and inquires into the natural history and connexions of disordered function and structure, regardless of conventional views and nominal differences.

The thirty-first lecture is *on Gout;* and as some curious illustrations of the morbid states known under that name are given, and as views as to its nature are still received by practitioners generally, of a most restricted character, and by no means in accordance with a comprehensive and enlightened pathology, we will devote some of our pages to its consideration.

Dr. Graves first notices the momentary twitches or sudden pains experienced by persons of a gouty habit, which last only for a few minutes, or even for a few seconds, and which he attributes to a momentary congestion:

> "As there can be no doubt, then, that a momentary congestion may produce a momentary pain, we may infer that, in many instances, gouty twitches are owing to the same cause which determines an instantaneous congestion of the affected part. Sometimes the congestion is more lasting, and the pain is proportionately intense and persistent. Thus, the late Mr. Daly, of Henry street, mentioned to me the case of a gentleman, the lobe of whose ear was sometimes attacked suddenly by gouty congestion, accompanied by agonizing pain, but which never lasted more than a few hours. And I have myself recently suffered from a similar attack in the cartilage of the ear, which did not last longer than an hour, disappearing on the occurrence of gouty pains in the fingers." (Vol. i, p. 462.)

The gouty affections of the cutaneous tissues have not received much attention; indeed, their nature is seldom recognised. Gout much oftener attacks the nose than is suspected; the fiery tint observed in many is usually arthritic.

A paroxysmal affection of this kind is mentioned by Dr. Graves: A lady of decidedly gouty habit, aged about sixty, felt her nose to become hot about three o'clock every afternoon, the heat continuing for four or five hours. The nose became first of a bright red, and then of a purplish red colour, in which the upper portion of the cheeks participated. We have seen examples analogous to this, and have observed that the line of red extends symmetrically across each cheek, in a direction at right angles to the nose. Sometimes a slight scaliness is left by the irritation, and, as in Dr. Graves's case, pain never was complained of.

The following is a rare instance of gouty disease of the skin:

> "One of the most remarkable instances of fugitive inflammation, affecting various parts of the body, which has come under my notice, occurred in the person of a gentleman, lately under my care. I shall not go through the whole history of his disease, of which he has favoured me with a very minute account, but shall merely state, that he is of a gouty habit, has had an attack of gout in the stomach, and is at present subject to a gouty affection of a very extraordinary character. After labouring for some time under languor and weakness, accompanied by spasms, pain, and sense of weight in the stomach, the pain of the stomach ceases, and his face begins to swell at various points, generally commencing on the forehead, and involving the cheek and eye, so as to close up the latter. He first feels as if a small current of air was directed on the face; then, as if it were the fillip of a finger, or the bite of a gnat; and, on looking in the glass, he suddenly perceives a tumour rising on the forehead, which, in the space of half an hour, becomes as large as a pigeon's egg, and, as he expresses it, moves down until it closes the eye. Sometimes it attacks the lips and other parts of the face, but never the nose. . . . . . . The tumours arise, run through their course, and disappear in a few hours. Sometimes the lips, inside of the mouth, palate and uvula are attacked, giving rise to very considerable inconvenience. Were such tumours to occur in the neighbourhood of the glottis, I need not say that they would be pregnant with danger of no ordinary character." (Vol. i, p. 463.)

The anomalous forms of gout are as singular and remarkable as those of hysteria, to which, indeed, they have a close pathological resemblance. There is hardly a peculiar form of hysteria that has not its gouty analogue. Some of these have a great practical importance, as, for example, gouty affections of the larynx, trachea, and lungs. Those physicians who have closely watched the proteiform varieties of gout will bear us out in the assertion that a cough from gouty irritation of the larynx and bronchi is one of its most ordinary forms. Indeed, many gouty persons of quick perception notice this circumstance themselves.

Dr. Graves mentions several examples of a novel form of gouty irritation of the dental nerves, so that the patient is obliged to grind his teeth. The first example is that of the late Countess of Egmont, who was obliged to grind continually, so that the teeth were worn down to their sockets, and she was obliged to give up all society for several years before her death. The moment she desisted the uneasy sensation in the teeth became insupportable. Dr. Battersby communicated the following case:

> "Henry W., County Meath, aged 60, has suffered from attacks of gout for the last thirty years, which are now so tedious and severe, as to confine him to his bed for at least five months annually; about three years ago he was observed gradually to get a habit of grinding his teeth, which he now does constantly while awake, and so loudly as to be heard into the next room; he is not conscious of it, unless when spoken to, I believe; and his teeth are quite ground down." (Vol. i, p. 466.)

We should be inclined to look upon these singular affections of the masseter muscles as being rather reflex actions from gouty gastric, or intestinal irritation, or from gouty centric irritation, than as being dependent upon a painful sensation; and this even in the Countess of Egmont's case, for in her the sensation appears to have been only coincident. The grinding is probably one of that class of automatic movements which, having a definite object, and resulting from a special combination of muscular action, present the *appearance* of being volitional.

. In gouty neuralgia, Dr. Graves recognises another of the proteiform manifestations of gouty disorder, and in the first case related by him, we have a decided illustration of the centric action of the poison that constitutes the proximate cause of gout. It is the case of a gentleman suffering from neuralgic pains in the lower extremities. Dr. Graves allows his patient to describe his symptoms:

> "When first the pains in my limbs commenced, they were confined to my feet; then, for a long time extended no higher than my knees; latterly, they had ascended as far as my hips, where, and in the groin, I sometimes experience great suffering. I have had occasional twitches in my arms; and very slightly across the chest. The pain always comes on with sudden violence, which renders it very hard to bear, especially when it attacks me during sleep. I am frequently aware of its approach, from a general feeling of discomfort and depression; from which, in the beginning of my complaint, I used to suffer very much for two or three days before an attack. These paroxysms have for four years shown a great tendency to periodicity, recurring generally once every week, commencing on Saturday or Sunday, and sometimes on Friday, and lasting till Monday. They have twice or thrice lasted for a week together, but sometimes continue only a few hours." (Vol. i, p. 469.)

If the patient had not been occasionally free from these paroxysms for two or three months together, they might have been attributed to chronic inflammation, or degeneration of the spinal cord. The pain was characteristically neuralgic, the weight of the bedclothes, or the slightest touch increased the intensity of the pain; even the air blowing on the part brought on violent torture, although strong pressure, or even a blow had no such effect. It was unaccompanied by any redness or swelling; the pain would cease instantaneously in one limb, and be felt as instantaneously in the next, but rarely occurring in both at the same time. Some debility of the motor system was excited by the paroxysms, since, for a day or two after, the patient was obliged to walk with a stick, and occasionally with two sticks. Sometimes there was vertigo and tinnitus aurium to a very distressing degree.

We believe Dr. Graves touches upon the true pathology of this class of affections. He considers that, in the first instance, the nerves are attacked with gouty congestion and inflammation; that, by frequent recurrence, these acquire a deeper root and a wider extension, until they involve the spinal cord, and thus gouty inflammation may lead to softening and structural degeneration of that organ. In support of this doctrine Dr. Graves relates two cases, but we cannot quite allow that they establish his views so decisively as he seems to think. The first example is that of a gentleman very fond of angling, who would wade up to the waist in water for many hours together, even in cold weather. He had occasional attacks of pains and tenderness in one or both feet for two years, when he was attacked with sudden frequent paroxysms of violent colic, accompanied by obstinate constipation, by nausea, and occasionally by vomiting, and always preceded or followed by a gouty affection of the feet. After a while a visible tremor of the fingers was observable, but pains in the wrists and fingers preceded them. These pains became gradually more severe and extensive, and the torture he felt in both hands and arms was beyond description. This was about two years after the first attack of colic. The arms shortly became paralytic, and two months afterwards paraplegia began to manifest itself, coming on gradually, however, but at last becoming complete. In three years from the first attack of colic the patient died, greatly emaciated, and paralytic in all his limbs. The viscera of the thorax and abdomen were found healthy on a post-mortem examination, but the spinal cord was softened to the consistence of thick cream opposite to the last cervical and first dorsal vertebræ; the remainder of the cord was also softer than natural.

In the other example, a highly arthritic subject fell from his horse, and afterwards complained of pain in the back and around the trunk, and his health, though often apparently restored, was never firm; in particular, he became subject to sudden attacks of pain, particularly in the chest, which became continually worse, until the diaphragm, intercostal spaces, and dorsal region generally was implicated, and he could only obtain relief by lying on his face, and carefully avoiding all motion. After various sufferings and changes, he became permanently paraplegic, and, about ten days before his death, the upper extremities, as well as the lower, together with the trunk, became quite paralytic: from the cervical vertebræ downwards all power of motion and sensation was lost. After death the spinal marrow, from the fourth cervical vertebra to its termination, was found converted into a morbid mass, of an ash-gray colour and pulpy consistence. When it was slit from above downwards, various shades of colour were observed on the surface of the sections; opposite to the third dorsal vertebra a blackish colour prevailed, and from this downwards a yellowish hue. Two little tumours, about the size of filberts, were found attached to the course of the fourth dorsal vertebra.

In looking carefully into the details of these cases (of which we have given a sufficient outline), we find that there is ample evidence to connect the disorganization of the medulla spinalis with a constitutional tendency to arthritic or gouty inflammation, but nothing which warrants Dr. Graves's conclusion, that the disease first attacked the nerves. The perfect intermissions of pain are mentioned by Dr. Graves as affording strong evidence that the pains, however violent and excruciating they might have been during the paroxysm, did not depend on an original affection of the spinal cord. If, however, we consider the metastatic character of gouty inflammation in those instances in which it is visible to us, as when it attacks the surface and the extremities; when we consider, too, that usually, after the earlier paroxysms of gouty inflammation, the parts affected are restored to their normal condition, and a total interruption of the gouty phenomena takes place; when we remember, further, that the frequent recurrence of gouty inflammation in a part certainly induces disorganization with a rapidity proportioned to the frequency of the attack, and with a certainty proportionate to its fixed and chronic nature, we are led directly to the conclusion, that, in the instructive cases detailed by Dr. Graves, we have nothing else than a gouty inflammation of the spinal medulla or its investments, frequently recurring, and ending in structural change.

In the one case, so far from this extension occurring, we learn that, the feet being affected, paralysis of the *upper* extremities first occurred; in the other case, also, paraplegia was the last symptom to supervene, the disease attacking first the dorsal region, and obviously traceable to the injury done to the back by a fall from horseback. But, in fact, Dr. Graves candidly states, that "there is no reason why gout should not attack the spinal marrow and its investing membranes in the first instance, or in consequence of metastasis," and this is, indeed, the shortest and simplest explanation of the cases detailed. The doctrine is by no means new. In 1715, G. Bude, quoted by Professor Marx of Göttingen, published a case of paraplegia from metastatic gout; it occurred in a man, aged 30, who had lived an intemperate life. First, he had a violent cardialgia, then gout, then debility and paralysis of the extremities, with paralysis of the sphincter ani. He was cured by the baths at Hirschberg.*

In the seventh volume of the 'Edinburgh Medical and Surgical Journal,' the case of a female, aged 40, is detailed, who had paraplegia, spinal tenderness, constipation, and eneuresis, together with hepatitis, who recovered upon gout attacking the hands and feet.

Nervous gout is considered specially by Dr. Wood, and, with some important exceptions, we agree generally with the opinions he expresses. He divides gout into the *acute*, the *chronic*, and the *nervous*. Amongst the structures and organs affected by acute gout, Dr. Wood mentions the *lungs*, leading to intense bronchial congestion, or to mere asthma; the *skin*, when urticaria, erythema, or some other exanthem is observed; the *brain*, leading to apoplexy and palsy; and the *eye*, under the form of ophthalmia. Nervous gout is apt to affect persons who have inherited a gouty diathesis, but who, from original temperament or abstemious habits, are little liable to inflammation. Women, and individuals generally, of a nervous temperament, who have descended from gouty ancestors, are very liable to it. The females only of a gouty family may be affected by it, while the male members have regular gout.

> "Nervous gout shows itself in the form either of simple neuralgic pain, or functional disorder. The pains may be dull and constant, or short, lancinating, and intermittent, and may occur in any part of the body. Sometimes they remain long fixed in certain parts; but are more frequently changeable, appearing now in one part, and now in another, without any regularity in their course. I have known an individual, who was for many years subject to a fixed pain in the region of the liver, without the least sign of hepatic derangement, and in whom the pain suddenly disappeared upon the occurrence of an attack of insanity, to return when the insanity ceased. That the affection was of the character supposed, was evinced by occasional external attacks of the disease in a subacute form." (Vol. i, p. 453.)

Sometimes these pains remain fixed in one spot, varying, however, in intensity. The right iliac region, the precordial region, extending to the left arm, the right side of the chest, and the scalp are mentioned by Dr. Wood as being not unfrequently the seats of this kind of chronic pain. Hemicrania is also one of the forms it assumes; but the severe, shooting, neuralgic pains are more especially apt to affect the portions of the surface and face supplied by the fifth nerve, although they may also attack the smaller joints and the viscera. In the following paragraphs, Dr. Wood touches upon the opinions of Dr. Graves:

---

* De Paralysi Membrorum inferiorum. Auctore C. F. H. Marx. With a coloured plate. Gottingen, 1840.

"Sometimes these pains, as well as various functional disorders, are found to be dependent upon the disease seated in one of the nervous centres, or in the neurilemma of one of the larger nerves, from or through which nervous influence is extended to the part affected. Thus, pains in the trunk and extremities may proceed from the disease in some portion of the spinal marrow, or its envelope. But, in most instances, the pains can be traced to no such origin, appearing to be the direct result of the morbid cause acting on the part." (Ibid., p. 454.)

"The spinal marrow and nerves become not unfrequently the seat of irritation, which displays itself in irregular muscular contractions, cramps, or spasms, and occasionally a vague and unaccountable uneasiness, extending through the limbs, with a universal feeling of oppression, heat, anxiety, weariness, soreness, &c., which they can only fully appreciate who have felt them." (p. 455.)

Now, the plain truth is, that the connexion between that morbid constitutional state, termed the gouty, and diseases of the nervous system, has been noticed from time immemorial. Statements abound in authors, from Aretæus to Graves, illustrating the connexion between gout and paroxysmal and other affections of the nervous system. Whytt, in particular, may be quoted as stating everything which Dr. Wood states, and more; illustrating his views with the details of several interesting cases, and giving a curious list of the diseases to be classed under the head of nervous gout.* It would be an empty and useless parade of learning, to quote (as we might and could) from Stahl, Stoll, Sauvages, Baillie, Darwin, Royle, Parry, Williams, Copland, Holland, and from other writers of less celebrity, as to the connexion between gout and functional disease of the nervous system in all its forms; we may, without fear of contradiction, we think, assume this to be one of the best established general facts in pathology.

Dr. Wood, in discussing the etiology of gout, displays his critical acumen rather in throwing doubt upon the received or advocated doctrines, than in resolving them. This mode (which is also adopted by him when other disputed and difficult questions arise) is very convenient to the author, and saves a world of trouble; but we do not think that Dr. Wood's readers will appreciate it; and the more especially as this author does not display any profound physiological knowledge, or the possession of those faculties which enable a man to judge almost instinctively of the truth and error contained in new doctrines. All that we can gather from his book, as to the etiology of gout, is, "that there is a morbid state of the system, which probably involves all its essential constituents, and which evinces itself now and then by purely local phenomena, which may be either purely nervous, purely functional, or inflammatory." Any practitioner who has made himself at all acquainted with gouty phenomena, in the widest sense of the term, who has observed carefully, and who has read thoughtfully, will recognise in the above quotation an expression of the barest and vaguest generalities.

We think that the theory of a morbid poison circulating in the blood, and derived from the excreta, or, in other words, the humoral pathology of gout, is sufficient to explain all its widely-varied phenomena; and we think that very strong arguments may be deduced from animal chemistry, empirical therapeutics, and clinical observation, in support of the theory, that that morbific *materies* is one which is naturally excreted from either the skin, or the kidneys; in other words, that the presence of the uric and lactic acids and their salts, in excess, in the blood, constitutes the proximate cause of the phenomena of gout and rheumatism.

---

* Observations on the Nature, Causes, and Cure of those Disorders which have been commonly called NERVOUS, HYPOCHONDRIAC, and HYSTERIC, &c. Edinburgh, 1765. 8vo. p. 154 et seq.

In our last Number, we attempted to introduce something like a philosophic method into the investigation of fevers, derived from a new consideration of their etiology. In particular, we directed attention to the doctrine, that the cause of fevers was the presence in the blood of one or more febrific *materies*, and that the action of this or these *materies* on the nervous system gave rise to the general phenomena of fever; the local complications being determined by the special nature of the febrific poison, or by peculiarities in the condition of the individual. The whole of the views and doctrines there stated are applicable to the phenomena of gout, and are sufficient to explain them.

The functions of the different portions of the nervous system, and the general laws by which they are regulated, give us the clue to an immense number of pathological phenomena, and must therefore be thoroughly understood and applied by any one taking on himself the duty of teaching others. Dr. Graves has comprehended this fact; Dr. Wood has not. The last-mentioned author will therefore find that his reputation will not be equal to his industry, and that he has forgotten or omitted to notice the circumstances which have led to the enduring fame of his predecessors in authorship. The careful perusal of Whytt's works will, we feel sure, prove very instructive to authors of Dr. Wood's class, provided they can appreciate his method of handling his subject. We now know much, that to Whytt was involved in impenetrable obscurity; and we can *à priori* determine accurately, although not completely, by the aid of established physiological principles, what results will follow the circulation of poisoned blood through the cerebro-spinal and sympathetic system.

To take the phenomena of gout in the order in which they have come under our notice; first, we have neuralgic affections implicating the sensory nerves of the cerebro-spinal system; secondly, positive inflammatory affections of the surface to be referred to the sympathetic system, or system of organic nerves; thirdly, a motor affection of the jaws, involving the motor nerves of the cerebro-spinal system, or "the true spinal system" of Hall. With regard to the first and last mentioned forms of nervous gout, we apprehend little difference of opinion will exist, at least with those who are accustomed to the observation of the neuroses that are dependent on malarious poison. Although less obviously analogous to those dependent on the arthritic poison, the rarer forms of these neuroses, in which limited portions only of the nervous system display the influence of the malarious poisons, are really the most corroborative of the general principle, that the pathological phenomena developed by poisons circulating with the blood are mainly due to their action on the nervous centres; and although this view does not exclude the idea of the direct action of the febrific poisons on the nerves, it lessens the probability of its frequency, as the sole cause of the morbid manifestations.

The most frequent of these local neuralgiæ are the intermittent neuralgiæ of the head and face. But a local ague of the arm has been observed (we forbear loading our page with literary references, except to Dr. Macculloch), the paroxysm beginning about seven in the morning; sometimes the vertical half of the body only has been affected, sometimes the horizontal, that is to say, there has been hemiplegic and paraplegic ague.

Other febrific poisons display the same partial action. Thus, measles have been observed to attack one half of the body only. In Rust's Magazin for 1829 a case of this kind is detailed of a child, aged three years, in which this occurred. It had previously perspired very imperfectly, and at one time not at all on that half of the body free from attack. Now, in theorising on this symmetrical development of the eruption, we can only adopt one theory, namely, that the cause is seated somewhere in the nervous system. No other meets all the phenomena of the case. The theory also explains a curious phenomenon, noticed by Dr. Graves, in the development of an exanthem, which he terms erysipelas; and the circumstance is the more interesting as Dr. Graves recognises the contagious nature of the disease, and consequently its dependence upon a specific poison.

> "In many cases a peculiarity was observed . . . . . namely, the spread of the erysipelatous redness in a symmetrical manner. I believe I was the first who directed attention to the fact, that, when erysipelas commences at any point of the mesial line of the body, it is very apt to spread in a symmetrical manner. Thus, in the present instance, the inflammation commenced in the majority of cases about the nose, and then extended in a perfectly symmetrical manner over the face, head, and down the neck; or, when it appeared first on the nape of the neck, it travelled down between the shoulders with a very remarkable symmetry of extent and outline. Sometimes this precise correspondence did not exist; but I can assert, that, in more than two thirds of the cases, it was extremely well-marked. It appears, then, that this occurrence is not so very rare as Dr. Johnson supposed. When I first noticed the fact of the occasionally symmetrical spread of erysipelas, he said it was an observation of very little importance, and that it was to be looked upon as a matter of mere curiosity,—a phenomenon which a man would not see twice in the course of his life. I have, however, shown it to many of the students half a dozen times during the last two months." (Vol. ii, p. 327.)

We believe this symmetrical development of morbid changes is a much rarer phenomenon in cutaneous and other diseases than is at all suspected. Herpes zoster, or shingles, is a symmetrical affection of this kind, and dependent, we think, upon similar pathological states. But there are also examples of retained excreta, showing their effects on one half of the body only. Vesper relates the case of an old man afflicted with hemiplegia, who presented the singular phenomenon of one half of the body (the paralytic) being completely jaundiced, while the other retained its natural colour. In the face it was very remarkable, as exactly half the nose was yellow. To the same class of phenomena belongs the case of a lady under our care at this moment, who, when she blushes, displays a deeper blush of red—a redness like erysipelas—upon the neck, and partly over the lower jaws; the line of demarcation is irregular, being indented with bays and promontories, but the line on the two sides follows indentations and undulations almost exactly similar.

Perhaps one of the most singular and at the same time instructive illustrations of the views under consideration, is furnished by the details of a case related by Dr. Graves under the title of a "singular and painful affection of the feet," of a paroxysmal nature.

> "The attack generally commenced at night, involving the foot, ankle, and leg, half way to the knee. It is generally confined to one foot and leg at a time, and when it subsides in one extremity, begins in the other. The affection commences with heat and tingling of the sole of the foot, then of the instep, ankle, and leg,

as high as the middle of the calf. These symptoms go on increasing for some time, the sensation of heat becomes extreme, and the pain agonizing. In proportion to the increase of these symptoms, the vascular congestion and fulness of the limb are augmented; the smallest veins are rendered distinct; and the larger ones become prominent. This state lasts for eight or nine hours, the sensation of heat and pain being all the time nearly insupportable.

"The resulting congestion of the cutaneous capillaries occasions a change in the skin, which, as the fit proceeds, grows at first red, and then gradually assumes a more suffused appearance, and a deeper hue, until it becomes swollen, smooth, and shining, and resembles very much in colour a black cherry when nearly ripe. When the hot fit ceases, the slight swelling and this discoloration subside, and the affected parts remain, during the next stage, pale, deadly cold, and comparatively free from pain; but as soon as the pain and heat have disappeared in the limb first affected, the same series of phenomena commences in the other leg, and lasts for the same length of time, after which both limbs are in their natural state, and for two or three hours she is comparatively free from suffering, although some uneasiness still remains, which she compares to a numbness," &c. (Vol. ii, p. 511.)

This disease returned daily for six or seven years, and at last got well, "almost in spite of medicine," as Dr. Graves observes.

These phenomena not only illustrate the nature and origin of these fugitive inflammations of the surface before described, but point to a similar origin as the inflammation which characterises an ordinary attack of gout. The constancy with which the ball of the great toe and the surface of that part is the seat of inflammatory action, would lead us to suspect that it is some change in the innervation of the part, having its origin in the dorso-lumbar portion of the spinal cord, which determines the outbreak of the paroxysm at that point. The periodicity of gouty attacks has also been specially noted by Pitcairn, Cheyne, and others; an ordinary attack of gout is, in fact, a quotidian or tertian intermittent, the hour of accession of the paroxysm being shortly after midnight. These attacks may, however, when the chronic form is established, cease to display this exact periodicity, either because the regularity of the phenomena is disturbed by the symptoms of local chronic disease, or because the affection itself has ceased to be acute. And we are inclined to place the profuse and often offensive perspiration of the feet observed in persons of arthritic diathesis (a form of disease of a most painful kind), in the class of chronic gouty affections.

In thus dwelling upon the centric origin of many of the phenomena of gout, of fevers, and of other diseases dependent upon the circulation of a poisonous *materies* in the blood, we do not intend to exclude the idea of peripheral irritation or morbid change in the parts specially affected. We have ample evidence, indeed, in support of the proposition, that local irritation and a special morbific condition of those parts are absolutely requisite for the localization of the symptoms. Thus the neuralgic affections of the head and face, so common in arthritic and malarious poisoning, may be traced partly to the anatomical relations of the facial nerves to the osseous foramina through which they pass, and partly to their physiological relation to the gastro-intestinal mucous membrane. Perhaps the pathological states of the latter ought more properly to be considered as centric causes of local irritation, inasmuch as they influence the facial and cerebral nerves pathologically through the nervous centres; still, however, they determine the local morbid condition. Another well-known class of local morbid states, determining the action of the poisonous *materies*, is that of local injuries. The case of the gentleman in whom arthritic inflammation of the spinal membranes and cord followed upon an injury done to the back in the dorsal region, is a fitting illustration of the same general law. Another well-known class of local morbid states is that comprising diseases in a part as a sequence to a diminution of temperature, or, in other words, consequent on "taking cold."

The views just stated have most important bearings on practice, as our more intelligent (not the so-called "practical") readers will readily perceive. As to the diagnosis and prognosis of all those varied affections complicated with the constitutional disorder termed gout, or arising out of it, we need say little, except that Dr. Wood is, we think, in error, when he states that the liability to gout is not indicated by certain corporeal characteristics. Experience in our case has not by any means proved this statement, but we can safely aver that it continually convinces us of the truth of the great principle, that certain corporeal characteristics, at least in the inhabitants of northern Europe, do manifestly indicate this liability to arthritic disease. In the United States the same characteristics may not be displayed, and this is perhaps the consequence of the climate; for we can never contrast the traits of feature and the general form of the native American without being struck with the departure he presents from the characteristics of the parent Anglo-Saxon stock. A taller stature, a less tendency to corpulence, a uniform sallow complexion, lanky hair, high cheek-bones, sunken eyes, aquiline nose,—such are the characteristics which every successive generation of native Americans displays more and more decisively, and lead at once to the notion that a continued exposure to the atmosphere of the American continent, and a pursuit of the chase, would inevitably modify, from generation to generation, the characteristics of the European possessed by the inhabitants of North America, and assimilate them more and more, in some corporeal characteristics at least, to those of the Indian race they have supplanted. In northern Europe, however, we can have no difficulty in detecting the liability to gout from corporeal characters, but it is necessary to remember that this liability may be mixed up with other diatheses or cachexies, as the scrofulous or bilious, and thus be rendered less perceptible.

The following characters may be relied upon as indicating the predisposition to gouty disorders: Firstly, the characteristics of the sanguine temperament, namely, a florid complexion, regular firm teeth, large bones, a highly-developed thoracic system, and a fully-developed muscular system, together with a love for stimulant drinks and animal food. Secondly, superadded to these, and constituting the marks of the arthritic diathesis, hair early gray or white, a portly form, a deeply florid complexion, a large mouth, well supplied with teeth, even to old age. Men of this kind are invariably attacked with gout in some of its forms, but ordinarily by the regular paroxysm, if they indulge in eating and drinking. But although the greater number of decidedly gouty persons present the above characteristics in this country, persons of a directly opposite build suffer much also from the disease, although rarely from its regular forms. The persons we allude to are sallow, small-boned, under-sized individuals, the complexion being almost sooty, the teeth small and apt to decay, the powers of the constitution feeble. Such have often hepatic disease or tubercular affections, especially of the lungs, complicating the disorder; their appetite is capricious, but is always turned to wine or other alcoholic stimulants, and animal food; their bowels constipated, the urinary secretion very variable, the perspiration inclined to be offensive, especially from the axillæ. While the former bear colchicum well, these require either very small doses only, or a combination with stimulants, chalybeates, or alterative doses of mercury. While in the former class, the circulating system is rather affected by chronic disease, leading to atheromatous or ossific degeneration of the heart, arteries, and serous and sero-fibrous tissues, but without exciting any immediate or very manifest symptoms; in the sallow variety metastatic inflammatory affections are common, and the whole series of phenomena dependent upon fatty degeneration of the heart, liver, and kidneys; while in the florid diathesis, stone and gravel are not unusual, in the sallow, chronic inflammations of the joints are more frequent. In fact, the two forms of gouty diathesis may be termed the sthenic and asthenic. Very frequently, however, the arthritic diathesis is complicated with the scrofulous or nervous diathesis, or with the malarious cachexy; and hence a train of phenomena differing widely from those of the characteristic form, and often requiring the practised eye to detect.

It has been noticed from time immemorial that gout, at least in its more characteristic forms, attacks only men, and men of a certain age. Dr. Wood might have enlightened his readers upon this point, if he had consulted some of our more recent English writers. The explanation of the general fact is, in reality, to be sought for in the physiological relations of the sexual organs, and especially of the ovaria and testes to the kidneys. The close and intimate connexion between the essential portions of the genito-urinary system is very manifest in the embryo and in the fœtus before the descent of the testes; and it is in reality the difference in functional activity of the ovaria and testes, and in the physiological relations to the kidneys, that determine the age and sex in which gout manifests itself. And the relations of these organs have also a vast importance in determining the locality in which the attack shall take place. The whole subject is, however, too subtle and too extensive for the pages of a review.

As to the therapeutics of gouty affections, we need scarcely advert to the necessity of investigating the condition of the kidneys and skin, but especially of the former, and of specially noticing the urinary and cutaneous secretions. To restore the due action of the skin and kidneys is, in a great majority of cases, the principal indication, and, when fulfilled, suffices for the cure. Modern chemistry has shown that colchicum has the effect of increasing the amount of lithates, or lithic acid, in the urine, when its excretion is suppressed—a fact long known, although recently "discovered" by some modern investigators. Professor Chelius found that the quantity excreted was nearly doubled after taking the remedy for twelve days. In those cases, however, in which the urine is loaded with lithates, due probably to an inordinate production of them in the organism, colchicum has, apparently, the effect of clearing the urine of them. Dr. Graves published statements to this effect, in a clinical lecture in the seventh volume of the 'London Medical Gazette' (and, by the by, we do not find this lecture republished in the volume before us), manifestly being of opinion that they were inconsistent with, and opposed to, the generally-received humoral pathology of gout. There does not appear, however, any discrepancy in these facts; colchicum may facilitate excretion of urea or the urates if retained, by its direct action on the blood and the excreting organs; or when formed in inordinate quantities, and at the same time excreted—at least in part—may restrain that inordinate formation by its action on the tissues.

Be this as it may, colchicum is an invaluable remedy in every form of gout. It should not be given indiscriminately, and in large doses only in the peracute forms. In all the more chronic forms, minute doses are indicated, as, for example, the fifth or fourth of a grain of the acetous extract, and in combination with alkalies, chalybeates, minute alterative doses of the salts of mercury, or with the salts of iodine, particularly the iodide of potassium. The chlorides and bromides, especially in the homœopathic doses found in mineral waters, are very useful in the asthenic forms. Where colchicum cannot be borne, even in minute doses, the ranunculaceous plants may be tried, as the black hellebore, or the common buttercup of the meadows—ranunculus acris. The ranunculaceæ were well known to the ancients as being powerful remedies in gouty affections.

Amongst the many interesting and sagacious observations contained in Dr. Graves's volumes, we find great difficulty in selection. A clinical lecture on "Purpura, Scurvy, the Hair and its affections," contains some singular additions to the histories already known, in which a species of rejuvenescence took place, so that gray hair became black or brown; and reference is made to instances in which teeth and hair were renewed at very great ages, as, for example, that of John Minnikin, whose hair grew so abundantly in his old age, that twenty wigs were made of it between his 80th and 112th years. Due attention has hardly been paid to examples of this kind, and their scientific bearing not estimated. That such a rejuvenescence takes place at all is extraordinary, and it seems well worth while to inquire into the special circumstances under which it occurs, as it does not seem altogether impossible that what occurs accidentally may, when the laws of its occurrence are discovered, be made to occur *secundem artem;* at least, such an inquiry would be more likely to lead to a knowledge of the means by which life may be prolonged or renewed, than the *a priori* inquiries and theories of general hygiene.

In Dr. Duncan's thin volume we have ten disquisitions or commentaries on cases under the guise of clinical lectures. The fourth treats of spinal arachnitis, and is interesting, as being an example of a new, and in many instances singularly fatal, epidemic. A florid-complexioned, healthy-looking girl, aged eighteen, complained for two days of an acute pain in the abdomen; on the third, Dr. Duncan saw her, and found her screaming out with agony. On the next day the epigastric region was found to be exquisitely sensitive, and she had pain in the back; acute tenderness over the two lower dorsal vertebræ; was unable to stand upright; and had a sensation as of cold water dripping down the spine and legs. She complained also of headache and vomiting; had thirst, and loss of appetite; the pulse was 80; the skin hot; the tongue moist, and covered with a slight bilious fur; and the urine scanty. The case terminated favorably after a general bleeding from the arm, and calomel and tartar emetic in full doses frequently repeated.

This disease has appeared as an epidemic in several large towns on the

Continent; and, from its so frequently attacking the young soldiers in various garrisons of France, it became of sufficient importance to require a special investigation by commissioners deputed from the Royal Academy of Medicine. It 1846 it appeared in Ireland, being first observed at Loughlinstown by Dr. Darby, who published an account of its symptoms and progress in the 'Dublin Medical Press;' then it was observed in the workhouse of the South Dublin Union, and lastly, in that of Belfast; but in all these localities about the same period. Dr. Mayne communicated an account of the disease as it appeared in the South Dublin Union to the 'Dublin Quarterly Journal,' and in its more aggravated form it was thought to resemble the Asiatic cholera very closely in the suddenness of its attack, the vomiting and purging that frequently attended it, and the early period at which collapse took place. In Ireland, as on the Continent, youths of the male sex were almost exclusively the subjects of the disease: in France, young recruits; in Ireland, boys under twelve years of age. Instances of females being attacked were, however, occasionally met with.

This epidemic spinal arachnitis was a fatal affection on the Continent; at Meiningen, Jahn lost twelve out of twenty-one patients; and Rilliet states that it decimated the French garrisons in which it appeared, and was equally fatal amongst the civic population. In the majority of cases it terminated fatally about the fourth day; in some, however, in fifteen hours. Dr. Duncan states that no satisfactory theory as to its etiology has been made out. Probably it will be ultimately traced to the presence of some poison in the food; we have, in the action of spurred rye on the spinal cord, an affection which may be considered as somewhat analogous.

Although Dr. Wood's work bears the marks of much thought and research, we cannot think that it will be esteemed as a suitable addition to the libraries of any but the most literary; and even these would refer to it rather for information as to the manifestations of disease in the wide territories and varied climates of the United States, than as a mine of medical lore.

This new edition of Dr. Graves's 'Treatise on the Practice of Medicine' (for such, in truth, it is) is in a more convenient form than the first edition. It is also enriched with some of the more recent contributions made by Dr. Graves to medical literature, and we have no doubt but that Dr. Neligan has spared no pains in fittingly accomplishing his honorable task.

ART. XIII.

Elements of Anatomy. By JONES QUAIN, M.D. Fifth Edition. Edited by RICHARD QUAIN, F.R.S., and WILLIAM SHARPEY, M.D., F.R.S., Professors of Anatomy and Physiology in University College Hospital. In Two Volumes. Illustrated with numerous Engravings on Wood.—*London,* 1848. 8vo, pp. cccvii and 1363.

ALTHOUGH the title-page of the above work bears the date of 1848, yet its publication has taken place at three distinct periods. With the first part, issued in 1843, and forming about a third of the whole, the following announcement was made: "The concluding part, which will be published as soon as possible, will contain a preface by the editors, explanatory of the changes they have made." The second part, which is dated 1846, contains the following explanation of the delay in its appearance: "The changes made in this work by the editors have been much more extensive than had been originally contemplated. In fact, the principal part of this volume has been written anew; and to this circumstance the delay which has occurred in its publication is in a great measure due. The last part, comparatively a small portion of the work, is in active preparation, and will be published as speedily as is compatible with its satisfactory execution." It was, in fact, announced for publication "before the end of the year," and still more positively promised before the commencement of the session 1847-8. We were beginning to despair of ever witnessing the completion of the work; when to our surprise and gratification it actually made its appearance on our table soon after the commencement of the present session.

We are quite willing to admit that the large proportion of new matter in this edition furnishes some excuse for the tardiness of its publication. But we cannot deem this excuse by any means sufficient, when it is borne in mind that the labour has been divided (as we shall presently see) amongst several hands, and that the subjects of the work are annually expounded in detail by the editors to their respective classes, so that the materials must be (to use a colloquial phrase) at their fingers' ends. Valuable as we deem the introduction on General Anatomy contributed by Dr. Sharpey, and greatly as we admire the comprehensive knowledge and cautious observation which it evinces, we cannot find in it the evidence of any such originality of research, as might fairly require five years for its completion,—the author's labours not having been productive of new results on more than one or two subjects of importance. And highly as we estimate the style and manner in which the Descriptive Anatomy is treated, we are at a loss to find in it an adequate reason for the retardation on which we feel called upon to animadvert. It must be remembered that this work is one of the class particularly designed as text-books for the pupil during the prosecution of his academical studies; he is induced to purchase the former parts near the outset of his curriculum, relying on the promise of the speedy completion of the whole; and at the conclusion of his term of study, he either finds himself possessed of an incomplete work, or has the option of expending a further sum, for which he has now another class of demands, in acquiring that whose special value to him has altogether ceased. Every student, therefore, who has purchased a copy of the earlier parts of this work, and who has not protracted his anatomical studies so long as to find the last part of special use to him, has a right to complain of a breach of faith on the part of those concerned in its production; and the only apology which we could at all admit in such a case,—namely, the necessity of a great extension of original and laborious research, for the carrying-out of the primary design,—is one which we do not see to be applicable to the present instance. Not only does any serious want of punctuality in the fulfilment of engagements of this kind operate to the prejudice of the chief purchasers of text-books; it also tends to impair the confidence of the public in the regular appearance of other works, which, from their very nature, can scarcely be issued in any other way than in successive parts; and thus it deprives the publishers of that return for their outlay on the earlier portions of such works, without which they can scarcely be carried on. We shall steadily set our faces against the whole system of the piecemeal publication of text-books and other such volumes; such a system being only legitimately applicable, in our apprehension, to works of great extent, such as dictionaries and encyclopædias, or to costly illustrated works, which, even under the most favorable circumstances, scarcely repay the outlay they require.

Having thus discharged our consciences of their burthen, we can address ourselves to the pleasanter task of giving to our readers some idea of the contents of the volumes before us. Although still entitled 'Dr. Quain's Anatomy,' they do not contain much of the original treatise, as the following extracts from the preface will show.

> "The whole of the section on General Anatomy is rewritten. The descriptive anatomy of the osseous system has undergone various alterations; and some portions, including those which treat of the formation and growth of the several bones of the skeleton, belong exclusively to this edition. Under the head of the Muscular System, many additions have been made; among which may be especially mentioned the account of the variations of form and attachment observed in individual muscles. Several parts have been rewritten. . . . . The principal changes to which the section on the Vascular System has been subjected occur in the description of the arteries. The history of each of the larger arteries has been recast, and a statement of the varied forms which these vessels present in different cases has been abridged from a special treatise published by one of the editors. In the remainder of the work, including the description of the brain, nerves, and organs of the senses, the heart, with the digestive, respiratory, urinary, and generative organs, little or nothing remains of the former editions. The Surgical Anatomy, which has been introduced partly in connexion with the history of the principal arteries, and partly at the end of the work, has likewise been written for the present edition." (p. vi.)

The division of the subjects between the two editors is much the same with that which, we believe, is adopted in their respective courses of lectures; Dr. Sharpey having undertaken the General Anatomy, with the Descriptive Anatomy of the viscera, and other parts most intimately connected with physiology; whilst Dr. Quain's portion of the work comprehends the Descriptive Anatomy of the bones, muscles, vessels, nerves, &c., with the Surgical Anatomy of the different regions. Each of the editors, however, has delegated a considerable portion of his task to his junior colleagues; the description of the nerves having been in great part written by Mr. Ellis; much of the description of the fasciæ and organs of sense having been contributed by the lamented Potter; and a considerable part of the Descriptive Anatomy which fell to the share of Dr. Sharpey having been prepared by Mr. Marshall, on the basis of the notes of Dr. Sharpey's lectures. "But while they willingly concede to their colleagues," says the editors, "a full share of any merits which may be found in those portions of the work in which they were respectively engaged, the editors assume to themselves the whole of the responsibility." We are bound to say that we do not find, in the several portions of the work, any indications of the diversity of minds and of pens engaged in its production; but that, on the contrary, it possesses a remarkable unity and completeness in every department.

A treatise on Anatomy obviously affords but little scope either for analysis or critical discussion. There are few questionable points in the whole subject; and little of novelty can be reasonably looked for in the entire mass of matter of which such a work is composed. The portion of most general interest at the present time will, undoubtedly, be the General Anatomy, contributed by Dr. Sharpey; and this, after very careful examination, we have no hesitation in pronouncing to be the most complete, and at the same time concise, view of the subject that has yet appeared. It is paged separately from the Descriptive Anatomy; and we cannot but think that the publishers would find their account in issuing it as a distinct treatise. Its materials are, of course, in great part derived from the labours of the German histologists; but it is obvious that the author has, throughout, aimed at presenting a definite account of each topic, founded upon his personal observations, rather than that heterogeneous farrago of discordant statements which are brought together by the indiscriminating compiler.

The portions which show the greatest originality of research are those relating to the structure and development of Bone, and to the ultimate structure of Muscular Fibre. The former topic was fully discussed in the pages of one of our predecessors (Brit. and For. Med. Rev. Vol. XXIV, p. 386), soon after the appearance of the second part of the work; and we shall, therefore, now satisfy ourselves with briefly stating that Dr. Sharpey has called the attention of microscopists to the fact, that the animal basis of bone is *not* cartilaginous, but is made up of an interlacement of gelatinous fibres; and he has rendered it certain that in some instances the development of true bone takes place in the substance of fibrous *membranes*, no cartilage being present where it is in process of formation. He has further rendered it highly probable that, after the first *moulding* of the bones (so to speak), by the cartilage which usually precedes them, the solidification of the tissue, and the increase of the bone in diameter, are entirely effected by the calcification of *fibres*,—taking place in the one case within the Haversian canals, and in the other at the inner surface of the periosteum, which is continually in process of conversion into bone. Of late, the intra-cartilaginous mode of osteogenesis has been studied almost to the exclusion of every other; but Dr. Sharpey rightly points out how very small is the extent of osseous formation that can be accounted for in this manner, and how shallow is the presumption which some have been bold enough to hazard, that cartilage must pre-exist wherever true bone is formed.

On the ultimate structure of muscular fibre, Dr. Sharpey's observations concur with those of Dr. Carpenter, which were published contemporaneously with his; both these observers agreeing in the statement that the ultimate fibril is a string of *cells* united end to end, like those of a confervoid filament. Most of the principal London microscopists, we understand, fully accord with them as to the appearances actually visible, and as to the interpretation to be put upon them; and where a dissent has been expressed, we believe that it has been because the objector has not seen the preparations upon which the original observations were made. Thus Mr. Hassall, who called in question the correctness of the view of Drs. Sharpey and Carpenter, has since, we are informed, been convinced that he did so unjustly; having satisfied himself, by a careful examination of preparations made by Mr. Lealand, that their account of them is true. And we are confident that the same acknowledgment will, some time or other, be made by a still younger observer, Mr. Dobie, who has lately taken upon himself to dispute the correctness of the statement in which so many independent and highly-qualified observers are now agreed.* For ourselves, we can say that, having brought the preparations in question under the scrutiny of some scores—we might almost say hundreds—of observers, of various degrees of qualification, from the most accomplished to the most inexperienced, we have never met with any discrepancy as to the appearances actually visible—namely, 1st, an alternation of dark and light spaces; 2d, a division of the light space by a sharply-defined but very narrow line, into two equal parts; and 3d, the extension of the pellucid limbus, thus attached to each *end* of the dark spot, around its *sides* also. The mode in which these appearances are to be *interpreted* will, of course, depend in part upon the previous qualifications of the observer; we can only say that, to regard each dark space as the cavity of a cell, of which its pellucid circumference is the cell-wall, is by far the simplest account of them that offers itself, and is remarkably confirmed by the exact similarity in the appearances presented by very minute confervæ, which we *know* to be composed of linear aggregations of cells. Only those who have *not* examined these preparations for themselves, can deem it a sufficient explanation of the pellucid border of the fibril, to say that it is an optical illusion, resulting from a want of proper focal adjustment; for no experienced microscopist can possibly fall into such an error.†

We can strongly recommend the entire section on the General Anatomy of the Nervous System, as giving a complete *résumé* of the most modern researches on this subject, by one who is well qualified to judge between the opposing statements of different observers, and who has bestowed a large amount of labour (the results of which, though not ostensible, are not the less valuable) in following their tracks and verifying their explorations.

The following statements regarding the arrangement of the nerve-fibres at their peripheral extremities, will probably be new to a large proportion of our readers. Every contribution to our positive knowledge on this subject is of high interest, although we are far from being yet in possession of the whole truth respecting it:

> "The results of modern microscopic discovery seemed for a time to lead to the conclusion that the fibres of nerves do not, strictly speaking, end in the tissues in which they are distributed, but merely dip into those tissues, as it were, and, after forming slings or loops of greater or less width, return sooner or later to the nervous trunks. The further progress of inquiry has, however, failed to establish the generality of this conclusion, and has even gone far to disprove the existence of the alleged mode of termination in various cases in which it had been previously held to take place; and, indeed, it must be admitted that the arrangement of the nervous fibres at their peripheral extremities is still but imperfectly understood.
>
> "In no case was the termination of nerves by loops more generally acknowledged than in voluntary muscle; but certain observations, very recently made known by Wagner, are calculated to throw considerable doubt on the opinion hitherto received, especially when viewed in connexion with the results of collateral inquiries

---

* Annals of Natural History, February, 1849.

† The clearest view of the structure in question which we have ever had, was with one of Messrs. Smith and Beck's achromatics of 4-10ths of an inch focus. Notwithstanding its comparatively low degree of magnifying power, the definition of this lens was such as to render the elements of the fibril most distinctly visible; and, under such an amplification as this, it is quite impossible to suppose that an error in the focal adjustment could have produced the appearance of the pellucid border. Besides, it is perfectly obvious that the lines dividing the light spaces are continued, clearly and sharply, *beyond* the breadth of the dark spaces, into something on either side of them.

respecting the nerves of various other textures. Wagner states that, in the muscles of the frog, the tubular nerve-fibres may be observed to be at last divided into fine branches, which appear (although he is not quite certain on this point) to perforate the sarcolemma of the muscular fibres, and to divide into still finer filaments, not more than 1-10,000th or 1-12,000th of an inch in size, that run between the muscular fibrillæ, where they elude further scrutiny." (p. ccxvii.)

With regard to the well-known plexiform arrangement of the nerve-fibres in the skin of the frog, it is remarked that—

"Although the arrangement appears to the eye such as is described, further inquiries have shown that there is something beyond. Thus, fibres have been seen (by Todd and Bowman) passing from the plexus, through the superjacent layers of membrane towards the surface of the skin, and not visibly ending in loops, but becoming at last lost to view. In the eyelid of the frog, also, in which the plexiform arrangement of the fine nervous branches is readily seen, Henle observed nerve-fibres which ran singly a long way, and then disappeared, there being no evidence that they were continuous with others in the form of loops; some seemed to end abruptly, and this appearance, which Henle was disposed to consider fallacious, has since been described again by Hannover, who, moreover, saw other primitive fibres dividing into finer filaments, which were arranged into a plexus, and ultimately eluded the sight.

"To these examples must be added the remarkable observations of Schwann on the terminations of the nerves in the web or fin of the tadpole's tail. In that instance, as well as in the mesentery of amphibia, it appeared to Schwann that the ordinary primitive nerve-fibres, after separating from the fasciculi, divided into other fibres of much smaller size, and that the finer fibres resulting from this division, which were destitute of white substance, and wanted the dark outline, here and there presented little enlargements or nodules, from whence, again, delicate fibres spread out in various directions, and connect themselves in form of a network. Subsequent observations made by myself on the nerves of the tail of the tadpole, are confirmatory of those of Schwann. The fine fibres, which are derived from the division of the ordinary ones, want the bold, dark outline which usually marks the tubular fibres; they also present, here and there along their course, elongated corpuscles, like cell-nuclei, and, from their similarity in aspect to the gelatinous fibres, it might be supposed that they are really prolonged from gelatinous fibres mixed up with the tubular kind in the nervous branches; there can be no doubt, however, as to their source, for fine tubular fibres may be traced, which change their character as they proceed, lose their dark outline, and pass continuously into these pale nucleiferous fibres; moreover, many of the decidedly tubular fibres in this situation are marked with nucleus-like corpuscles. The tubular fibres might thus be represented as laying aside their white substance and dark outline before dividing or terminating, like those ending in the Pacinian bodies; but in the present case (of the growing tail of the tadpole) the pale fibres are, in reality, to be considered as an earlier condition of tubular fibres in progress of development. Schwann, who adopts this view, states that the pale fibres are the forerunners of tubular fibres, and he conceives that they are converted into the latter by acquiring the white substance (medullary sheath), and with this the dark outline. Still, whether perfect or not, these fine fibres must be capable of receiving and conducting sensorial impressions applied to the decidedly sentient membrane in which they are distributed." (p. ccxviii.)

We have quoted, in another part of our present Number (p. 366), from Dr. Sharpey's account of the two classes of nerve-fibres; and shall now cite his confirmation of Wagner's observations upon the connexion of the nerve-fibres with ganglionic cells:

"The alleged connexion of tubular fibres with ganglionic cells has been confirmed in a very decided manner by Wagner, who states that he has been able to trace it out most unequivocally in the ganglia of the spinal nerves, trigeminus, and vagus, or the torpedo, skate, and dog-fish, in which, from the small amount of cellular (areolar) tissue intermixed, the nervous fibres can be followed with comparative ease. He has also suceeded in observing it in the sympathetic ganglia of some of those fishes. He finds that almost invariably two fibres are connected at each ganglion-cell, at opposite sides or poles, as it were, one directed centrally, or towards the root of the nerve, and the other outwardly, towards its branches; and he thence infers that every fibre which issues from a ganglion-cell corresponds to an entering one with which it is connected through the medium of the cell, and that there is no multiplication of the fibres in a ganglion. The nerve-fibre, as it leaves the cell, appears like a mere prolongation from it, containing the same kind of granular matter; but further on it enlarges, and acquires the dark double contour, indicating the presence of the white substance. The cell-wall appears to be continuous with the sheath (tubular membrane) of the fibre, which in these fishes is remarkably strong. Some cells, generally smaller than the rest, are unconnected with fibres. On repeating Wagner's observation on the spinal ganglia of the common skate, along with my friend Professor A. Thomson, of Edinburgh, we very readily found ganglion-globules, with a tubular fibre proceeding from their opposite poles, as described by Wagner." (p. ccix.)

The account of the sympathetic system given by Dr. Sharpey is particularly complete, and his general summary expresses with great clearness and conciseness our actual knowledge of its structure and functions:

"From what has been stated, it seems reasonable to conclude that nerve-fibres take their rise in the ganglia both of the cerebro-spinal and sympathetic nerves, and are, in both kinds of nerves, mixed with fibres of cerebral or spinal origin; that the ganglia are nervous centres which probably receive, through afferent fibres, impressions of which we are unconscious, and reflect these impressional stimuli upon efferent or motor fibres; that perhaps, even, certain motorial stimuli emanate from them, the movements excited by or through the ganglia being always involuntary, and affecting chiefly the muscular parts of the viscera, the sanguiferous, and perhaps the absorbent vessels; and that, in fine, the chief purpose served in the animal economy by the ganglia and the ganglionic nerve-fibres, whether existing in acknowledged branches of the sympathetic, or contained in other nerves, is to govern the involuntary, and, for the most part, imperceptible movements of nutrition, in so far, at least, as these movements are not dependent on the brain and spinal cord." (p. ccxxiii.)

We must content ourselves with one more extract from this valuable introduction, relating to a subject on which there has been a vast amount of unprofitable discussion, but which now seems likely to be cleared up by microscopical inquiry. We refer to the development of capillary blood-vessels; Schwann's account of which, deduced from his observations on the germinal membrane, has been confirmed, in all essential particulars, by Kölliker's study of the process as taking place in the growing tail of batrachian larvæ:

"The smaller vessels and capillaries originate from nucleated cells similar to those which at first constitute the different parts of the embryo. The cell-wall or envelope of these cells shoots into slender pointed processes, tending in different directions, so that they acquire an irregularly star-shaped, or radiated figure. The prolongations from neighbouring cells encounter one another, and join together by their ends; and the irregularly ramified or reticulated cavities thus produced, are the channels of the rudimental capillaries. In growing parts, where new vessels are formed in the vicinity of those already existing, not only do the processes of the stellate cells join those of neighbouring cells, but some of them meet and join with similar pointed processes which shoot out from the sides of neighbouring capillary vessels, and in this manner the new vessels are adopted into the existing system.

The junctions of the cells with each other, or with capillary vessels, are at first of great tenuity, and contrast strongly with the central and wider parts of the cells; they appear then to be solid, but they afterwards become pervious, and gradually widen, blood begins to pass through them, and the capillary network acquires a tolerably uniform calibre. The original vascular network may become closer by the formation of new vessels in its interstices; and this is effected by similarly-metamorphosed cells, arising in the areolæ, and joining at various points with the surrounding vessels, and also, according to Kölliker, simply by pointed off-shoots from the existing capillaries stretching across the intervals, and meeting from opposite sides, as when enlarged to form new connecting arches. From observations made on the fœtal membranes of sheep, Mr. Paget has found that the mode of formation of capillaries described by Kölliker in batrachians, holds good also in mammiferous animals." (p. cclix.)

The larger blood-vessels, however, appear to be formed by the coalescence of cells arranged in linear series; those of moderate size taking their origin in single or double files of such cells, of which the coalesced walls form the primitive simple membranous tubes of such vessels; whilst the larger trunks, like the heart, are formed of agglomerations of cells, of which the central part liquefies to form the cavity, whilst the peripheral are metamorphosed into the fibrous tissues of which their walls are composed.

In concluding our notice of this portion of the work, we must remark that we have not been able to discern upon what principle the tissues are arranged; and that, in desiring to refer to any portion of it, we consequently find ourselves at a loss to know in which division to look for the information we seek. The simple *fibrous* tissues, for example, are interposed between the *adipose* and the *cartilaginous*, whose truly *cellular* character is peculiarly obvious. And we are sorry to find the term "cellular" tissue retained, in apposition with the more correct designation "areolar," to indicate a tissue which, whatever may be thought as to its origin, has nothing of the truly "cellular" character in its perfected condition, but is pre-eminently "areolar" in its texture. This, however, constitutes all that we have to remark in the way of criticism; and the faults we have found are of but small amount in comparison with the distinguished merits of the treatise.

With regard to the Descriptive Anatomy we have, for obvious reasons, very little to say, and all that we have to say is in its commendation. The qualities which rendered the original production of Dr. Quain so generally appreciated, distinguish this *re*-production; and we believe that any country might safely be challenged to produce a treatise on anatomy so readable, so clear, and so full upon all important topics, without dwelling tediously upon those of less consequence. In this respect, its superiority over those descriptive works which follow the minute branches of a nerve or artery with the same detail that they bestow on the course of the main trunks, is so obvious, that we are not in the least surprised at the preference which the preceding editions enjoyed. We must not omit to mention that a large number of first-rate wood-engravings have been added to the copious illustrations formerly given; and that the General Anatomy is elucidated by an entirely new series, the greater part from original drawings. We are very glad to see that the sections on the *development* of the principal organs have undergone considerable extension, and we have only now to express the hope that, in the next edition, some idea may be given of the "vertebral theory" of the skeleton, as expounded by Professor Owen.

Art. XIV.

A Practical Treatise on the Domestic Management and most important Diseases of Advanced Life; with an Appendix. By G. E. Day, m.d., Fellow of the Royal College of Physicians, and Physician to the Western General Dispensary.—*London*, 1849. pp. 338.

We are quite disposed to agree in the opinion with which the author of this work opens his preface, viz. that the influence of age upon diseases is a subject of extreme importance. The physician does not, indeed, ever banish from his calculations of prognosis and treatment the effect of age on the course and progress of maladies. He cannot, for a moment, forget that, to each epoch of life, to its youth, its maturity, and its decline, his art bears a different relation, and has to assume for each a different office. And if it is important to recognise the peculiarities which, in the commencement of life, demand a peculiar investigation, or render necessary a special treatment, so also in its ultimate term, when that life is drawing to its natural close, it is not less necessary to note the diseases which may be proper to the age, and to know the modifications which diseases common to all ages may undergo under the influence of that particular condition of system.

It is also a physiological problem of extreme interest to note the course of natural decay, and to determine the influence which original constitution, mode of life, or antecedent diseases may have upon it. For the body, like its several elements,—which, as pointed out by one of the great physiologists of the day, have allotted to them a limited period of functional activity, after which they die and are replaced,—would no doubt decline by regular gradation, towards that point at which the least vicissitude can arrest the action of some organ essential to life, if this regular course were not disturbed by a thousand accidents. But innumerable contingencies vary, and ever-changing influences modify and mask, the normal process of decay. To determine accurately the influence of age, abstractedly considered, in altering the structure and functions of organs, and, as a consequence, the manifestation of the diseases which may occur in them; to express the principal variations which occur in this manifestation from the additional influences of co-existing or pre-existing circumstances, of constitution, natural or acquired, is a task of difficulty equal to its importance.

With all these contingencies and varying agencies the physician must, however, deal; and, in the absence of positive knowledge on many points, he is obliged to act, in the present day, more on general principles, than on any set of rules which have been yet deduced from observation.

The present work is one well calculated to afford aid to the practitioner, both as regards the hygiene and the treatment of the diseases of old age. It is apparently intended also for the non-professional public, and therefore enters into many particulars which, to professional readers, will appear superfluous. For the same reason, it avoids entering on numerous points which the medical reader would have desired to see discussed. We think, indeed, that the author would have done more wisely to have entered fully and systematically into his subject, and to have abandoned his semi-popular form of exposition, by the adoption of which he has run the risk of doing injustice both to his subject and to himself.

In the following abstract we shall notice what appears to us most likely to be useful or interesting to our readers.

Dr. Day proposes to discuss the diseases of advanced life; that is to say, of that period which may be said to commence in women at the termination of their active sexual life, viz. from the 40th to the 45th year, when the uterine functions are more or less slowly and irregularly suspended; and in men from the 48th to the 50th year, when, after the activity and maturity of adult life, there is a gradual but certain deterioration of the just balance and mutual relations of the organs of the frame. Of this period Dr. Day makes four subdivisions, viz.: 1st, Declining age, extending to the 52d year in women, and the 60th in men; 2d, Advanced age, extending to the 65th year in women, and to the 70th in men; 3d, Mature old age, extending to the 75th year in the female, and the 80th in the male; and, 4th, Decrepitude, or second infancy, including all the period beyond the ages last named. These arbitrary divisions are, of course, liable to continual variations; and it must never be overlooked that, in a medical point of view, the number of years which an individual has passed through do not correctly indicate the exact point at which he has arrived in his physiological life. The flight of time is only one of the agencies which have been operative in the production of a certain condition of organs; and we must be on our guard lest the easy estimation of this single quantity should throw into the shade the other essential elements of this complex problem.

In his first chapter, Dr. Day describes the modifications which occur in the several more important organs in advanced life.

In the respiratory organs, both the thorax and its contents undergo considerable alteration. There is a general shrinking of the intervertebral cartilages, an approximation of the ribs to each other, and ossification of their cartilages;—the vertical diameter of the chest is therefore lessened; the transverse diameter is diminished; the sides become flatter; the vertebral column is more or less curved. The lungs are altered in various ways, which, according to Hourmann and Dechambre, quoted by Dr. Day, may be included under three types. In the first type, the chief change occurs in the position of the interlobular fissure, which becomes vertical instead of horizontal, so that, in the right lung, the middle lobe projects downwards, and the lower lobe is elevated behind; in the left lung the upper lobe becomes anterior, and the lower posterior. The air-cells are also double the size of those of the adult lung. In the second type, the cells are still larger, and the vessels seem lessened—the whole lung is, indeed, diminished in size. In the third type, the conical form of the lungs has disappeared; the summit is often larger than the base; the fissures nearly, or entirely, disappear; the air-cells are large and irregular; the blood-vessels still smaller; the density of the lungs diminishes, as does the quantity of blood they admit, with the progress of age. From this state of things it results that the movements of the ribs are lessened; inspiration is performed chiefly by the diaphragm; the inspiratory murmur is shortened and roughened; the expiratory is increased; and, in addition, the lungs themselves admit a smaller quantity of blood, which is less perfectly aerated and depurated. The average number of respirations per minute, according to Hourmann and Dechambre, is 22; but this is rather at variance with the observations of Pennock.

In the nervous system, the brain, cord, spinal nerves, and sympathetic are diminished in size and weight. The membranes of the brain and cord, particularly the pia mater, are thickened, opaque, and sometimes contain calcareous deposit, or vesicles containing plates of cholesterine. The diplöe is absorbed; the skull is therefore thinned; and, as the brain shrinks from it faster than it can contract, the fluid in the subarachnoid space, and in the ventricles, is generally increased in quantity. The gray matter of the brain is thinned, the convolutions are shrunk, and the sulci are much deepened; the medullary substance is often firmer than natural—the quantity of blood is diminished, and the arteries at the base are often atheromatous. All the organs of sense, also, undergo changes with which our readers are familiar.

The functions of the nervous system are of course impaired, and Dr. Day calls attention in particular to the fact that the bond of nervous sympathy which connects healthy organs seems weakened or almost lost. A disease which in childhood would develope sympathetic symptoms in numerous organs, in old age seems often to be limited to the affected organ. Here, however, it is necessary to remember that the changes of both the circulation and the blood may participate in the effect.

In the digestive organs, the muscular coat is atrophied; the mucous membrane thickened; the villi and glands shrunken.

In the circulating organs, the heart is often diminished in size; sometimes, however, it is increased in size and power; spots of atheroma are found on the endocardium, and in the coats of the arteries; the capillaries are diminished in diameter, and the veins become tortuous and distended. Dr. Day states that the average number of cardiac pulsations per minute, as given by 562 healthy women, of the mean age of 73 years, was 79; and as given by 197 healthy men, of the mean age of 68 years, was 72·5. He does not allude to the observation of Dr. Guy, who gives a slight decrease during declining life, until the period of decrepitude, when an average rise of seven beats per minute occurs.*

Such are the prominent changes in the great bodily organs; a diminished activity coincides with an impeded nutrition, or, to speak more correctly, a gradual deterioration in the assimilative processes impresses a simultaneous alteration on every part of the body.

We may pass over the following chapters on the hygiene and medical treatment of advanced life, on senile marasmus, and on the climacteric disease, with the remark that the author does not give us any new information on this latter interesting, and as it appears to us, ill-understood affection.

In the sixth chapter the author commences the discussion of the diseases most fatal to persons in advanced life. Taking the Registrar-General's table for the last five years, he finds that in the metropolis 55,048 persons, aged 60 or upwards, have died during that period of time. Reducing this to a scale of 1000, he gives the following table:—

---

* Cyclopædia of Anatomy and Physiology, art. PULSE, part xxxi, p. 183.

"Of 1000 persons who have attained the age of 60 there die—

Of old age or of diseases not registered			285·3
Diseases of the respiratory organs, viz.	Bronchitis	79·3	237·5
	Asthma	62·4	
	Consumption	35·7	
	Pneumonia	27·1	
	Hydrothorax	10·4	
	Other diseases	22·6	
Diseases of the nervous system	Apoplexy	53·0	130·9
	Paralysis	51·2	
	Other diseases	26·7	
Diseases of the digestive organs			59·2
Diseases of the circulating system	Diseases of heart	51·3	53·5
	Pericarditis	1·3	
	Aneurism	0·9	
Diarrhœa			20·3
Influenza			14·2
Erysipelas			7·8
			808·7
Other diseases, including continued fever and dropsy			191·3
			1000·0

We question the propriety of using these returns for the purpose of determining the relative frequency of the diseases of old age. The Registrar-General's Reports, invaluable in some points of view, are not adapted for use, when a minute diagnosis has to be made between affections which at all resemble each other. If we wanted to know what proportion of old people died of diseases of the lungs, and what proportion of diseases of the abdominal viscera, we should not wish a better guide than the Reports alluded to; but if we wanted to know what relative mortality a special pulmonary disease bore to another affection of the chest, we should use them with the greatest caution. For example, in the above table it is stated that out of 1000 persons 79 died of bronchitis, and 27 of pneumonia, the proportion being as 2·92 to 1. But in Paris, out of 390 persons, over the age of 60, Prus found that 77 died of pneumonia, and only 8 of bronchitis; so that the pneumonic cases were nearly ten times as frequent as the bronchitic, instead of being three times as infrequent. Dr. Day, alluding to this point, states that his observations lead him to concur with Prus, and therefore concludes "that in all probability there is an enormous error in our mortality tables, which are not based on post-mortem examinations." Again, even at this advanced age, consumption appears in the table to be nearly half as fatal as bronchitis, and much more fatal than pneumonia; this great mortality is in all probability partially erroneous, and arises from a want of discrimination between low infiltrating forms of pneumonia, and true phthisis, although, of course, true phthisis will occur at a very advanced age. Under the head of diseases of the circulating system, we have also 51 cases returned under the vague name of disease of the heart, while only 0·9 appear as aneurism. But aneurism is, without a doubt, relatively much more frequent than this, but is merely overlooked from want of post-mortem examinations. We allude to those points chiefly, because we are convinced that serious errors may arise from the employment of statistical returns, if the items from which the totals are made up are not accurately determined.

Dr, Day indeed displaces bronchitis from its prominent position in his table, and justly assigns the most important post to pneumonia. His remarks on this disease are short but practical. He observes that the influence of age in the production of pneumonia is indicated by the observation of Prus, who found among old people that it furnished one ninth of all cases of illness, and one sixth of all cases of death; whereas in adult life, Grisolle determined that pneumonia formed only one sixteenth of all the cases of illness, and one tenth of the whole mortality. Cases of pneumonia in old people chiefly occur in the cold months; as it appears, from the observations of Hourmann and Dechambre, that 140 out of 156 cases occurred between November and May inclusive. The predisposing causes proper to the individual are habitual bronchorrhœa, and consequent congestion of the lungs, which may also in some cases be produced by position. Pneumonia in the aged may be either declared, or, as in rather less than half the cases, latent. When indicated by symptoms, there is often pain in the side, sometimes dyspnœa, but frequently very slight cough, and often little expectoration, which frequently does not present the rusty colour of the pneumonia of earlier periods of life. In reference to the sputa, Dr. Day calls attention to the ramifying bronchial coagula, described by Remak, in the pneumonic expectoration, which he considers have not attracted sufficient attention in this country. He does not inform us whether he has found these fibrinous ramifications in the pneumonic sputa of the aged. We doubt very much if they are found constantly in true pneumonic expectoration, and are inclined to believe that Remak's cases were examples of broncho-pneumonia, and allied to the "kroupöse Pneumonie" of Rokitansky. In 17 out of the 67 cases observed by Hourmann and Dechambre, the sputa were bloody; in other instances they were gray and opaque, and in a few cases transparent and viscid. The physical signs of pneumonia are modified by the previous emphysematous condition of the lungs proper to old people; the percussion note is therefore often tolerably clear, even with considerable consolidation; pneumonic crepitation is often represented by a coarse subcrepitant râle; and the bronchophony of the second stage often assumes a squeaky character approaching to œgophony. Sometimes feebleness or absence of the respiration and bronchophony are the only physical signs. After remarking that the prognosis is very unfavorable, in direct ratio, *cæteris paribus*, to the age of the patient, Dr. Day proceeds to the treatment. He objects to general depletion as a general rule, but thinks leeches and cupping serviceable. As internal remedies, he recommends antimony cautiously administered with ipecacuanha, squills, or occasionally camphor. Frictions with oil of turpentine or camphor liniment, with a little cajeput oil, or flying blisters, are of service. Calomel and opium are not so useful as in the pneumonia of middle age.

The next chapter, on Bronchitis, does not call for any remark. Dr. Day advises emetics of sulphate of zinc, when the tubes are filled with mucus; and in the asthenic varieties he recommends camphor, combined with colchicum or with antimony. He also calls attention to the fact that the humidity of the air of the apartment may be often advantageously increased.

Passing over the next section on bronchorrhœa, we arrive at a chapter on Asthma. Two varieties of asthma are made, one depending on organic changes either in the lungs or heart, and the other attributable to impurity of the blood. Pure nervous asthma is said to be hardly ever seen in old people. On the first form we need not delay; the second variety is called by Dr. Day "cachectic asthma." This includes, apparently, the forms described by Schönlein under the terms "asthma podagricum et urinosum."* Dr. Day, indeed, follows Schönlein very closely in his descriptions. The following are his remarks on this form:

> "I proceed to the subject of *cachectic asthma*, an affection that seems to have been almost altogether overlooked by English practitioners, although of very common occurrence. The impurities contained in the blood seem here to be the exciting cause of the paroxysm. There is an attempt on the part of Nature to make the bronchial mucous membrane eliminate the effete matter of the blood in the form of expectoration. Asthma is very often associated with a deficient or morbid action of the kidneys. I have seen so many cases of this form of asthma, that I cannot doubt the intimate connexion between the state of the respiration and the morbid condition of the kidney, and, for the sake of convenience, shall term this *urinous asthma*. The term has been already used by Schönlein, Canstatt, and other continental writers.
>
> "Urinous asthma seldom occurs before the sixtieth year, and is most common at and beyond the seventieth year. On examining a patient with this affection, we usually find a general suppression of the secretions, the skin being dry and rough, and the bowels acting slightly about twice a week. The urine is scanty, rather turbid, of a reddish-brown colour, and so acrid, as to produce a sensation of scalding in the urethra, and to give rise to frequent calls to make water. There is usually a feeling of dull deep-seated pain about the loins. The skin is the seat of intolerable itching, and presents the appearance of prurigo, which, like the asthma, arises from the retention of the urinary constituents in the blood. The eyelids are red, and discharge an acrid humour, and ulcers often form on the lower extremities. The paroxysm of asthma usually occurs an hour or two before midnight, and lasts some hours, terminating most commonly in a copious expectoration of viscid and very salt mucus, which frequently has a strong urinous odour.
>
> "Another cachectic form of asthma is connected with the gouty diathesis. It sometimes comes on as early as the fiftieth year, in persons suffering from asthenic or anomalous gout. The premonitory symptoms are the same as those of a fit of regular gout. The patient is led by these symptoms to expect a fit of gout; but instead of this he is seized, usually about midnight or a little before, with a feeling of intense and terrible suffocation. The paroxysm lasts, with slight remissions, for some hours, and towards its close a considerable amount of thick mucus, frequently mixed with blood, is ejected. The fit is often succeeded by other efforts to depurate the blood by copious sweats, urinary sediments, &c. If they do not occur, a second paroxysm of asthma may be very shortly expected." (pp. 113-14.)

We are sorry that Dr. Day has not entered a little into the etiology, minute symptomatology, and morbid anatomy of these forms of asthma. This has not yet, as far as we know, been ever thoroughly done by anybody, but it is a subject well worthy of further investigation. Every one is aware how important an element in the production of certain inflammations, as pericarditis and some forms of pneumonia, is the condition of the blood; and the influence of impure blood on the bronchial or the alimentary mucous membrane is a cognate subject, full of practical interest.

The "asthma podagricum" is, as we take it, a form of gout, in which it is to be inquired whether it is by reason of some particular condition of the pulmonary mucous membrane, or on account of some particular chemical state of the *materies morbi*, uneliminated by its normal channel, that this particular form of the paroxysm is produced. These inquiries are the more interesting, because gout is one of those simpler forms of disease, if we may so express ourselves, the several phases of which, and the periods and reasons of their occurrence, will probably be determined sooner than in the case of any other malady. It is not unreasonable to expect that we may soon discover, not only why the urate of soda accumulates in the blood, but why this accumulation gives rise to the symptoms of regular gout, to the several forms of irregular gout of which this "podagric asthma" is one, to tophaceous deposits in one part rather than in another; and, in fact, to a special manifestation of some one among the many phenomena which may be consequent on the non-elimination of the uric acid. It seems very probable that certain chemical and anatomical conditions, apart altogether from vitality, will explain all the differences of symptoms. We are almost sorry to find our author so far condescending to the hypothetical language of the day, as to assert that, in this disease, "there is an attempt on the part of Nature to make the bronchial mucous membrane eliminate the effete matter of the blood." Such an expression is to us merely a figure of speech, in the same way as if we were to say that the effete matter tended to pass off by the bronchial mucous membrane. But it would be a mistake and not a metaphor, if we intended to imply that Nature, that is, a power superior to the ordinary forces which direct the effete matter to this or that channel, according to certain physical and chemical conditions, did actually, for the purpose of preventing injury to the system, impel the effete matter to a more convenient outlet than, in the ordinary operation of the chemistry of the body, it would have found; or if we asserted that the effete matter tended to pass off by this rather than by that surface, in consequence of any activity, or by virtue of any force, inherent in itself, and opposed to the ordinary chemical laws which would otherwise have governed it.

---

* Allgemeine und specielle Pathologie und Therapie. Vierter Theil, 1839, pp. 89-90.

"Nature's healing force" is no autocratic power, which subverts the ordinary processes; it is simply a result of the constant activity of those processes. When a foreign body is in the system, either introduced from without, or retained from a failure of excretion, it is necessarily involved in the never-ceasing mutations of the vital laboratory, which for a time it modifies or obstructs, until, like any other particle better adapted for nutrition, it finishes its series of changes, and its disturbing effects gradually cease.

In the treatment of asthma, Dr. Day alludes favorably to the "calmative" effect of stramonium. We presume that he refers chiefly to the true spasmodic asthma, or to the varieties dependent on cardiac disease and emphysema, and not to those forms which he terms "cachectic asthma."

The three following chapters present nothing of importance.

In the next chapter Dr. Day discusses the several diseases of the nervous system, most common in old age. We question the wisdom of attempting even a summary of such important subjects, as intra- and sub-arachnoid hemorrhage, hemorrhage into the substance of the brain, or into the lateral ventricles, softening, simple congestion, and meningitis, in the space which the author assigns to these affections. To all these affections he only devotes twenty-two pages, a space utterly inadequate to discuss any single topic. Here, as elsewhere, we are led to regret that the author has not addressed himself either to the profession, or to the public, exclusively. In either case he would have found room for the introduction of much valuable matter, by the omission of what is inappropriate to one or other of the classes whose interests he has attempted to combine in the present publication.

With respect to sanguineous apoplexy, by which term is meant apoplectic symptoms arising from effusion of blood in any situation within the cranium, Dr. Day states that it is essentially a disease of advanced life, and refers to Dr. Burrows's table, in which the liability to attack increases with every decennial period from the 20th to the 80th year. We think, however, that Dr. Quain* has adduced facts sufficient to make us doubt the correctness of this opinion. Taking only the cases of apoplexy occurring between the ages of 20 and 80, Dr. Quain's observations go to show that the greatest number of cases of apoplexy occur in males between the ages of 40 and 50, and in females between the ages of 50 and 60. Taking both sexes together, the cases of apoplexy are to a reduced scale of the population as follows: between the ages of 20 and 30, as 1 to 66; between 30 and 40, as 1 to 25; between 40 and 50, as 1 to 16; between 50 and 60, as 1 to 10, at which point the disease reaches its acme of frequency and then again diminishes, being between 60 and 70, as 1 to 18, and between 70 and 80, as 1 to 16. Dr. Quain concludes, and with apparent justice, "that apoplexy is not, as many suppose, chiefly a disease of old age; neither does it uniformly increase in the frequency of its occurrence as life advances; but that the liability to its attacks, considerable between 30 and 40, is greatest between the ages of 40 and 60." We may remark also, that after all, the determination of the mere frequency of intracranial hemorrhage, without reference to the causes which produce it, is only a portion of the inquiry. The hemorrhage is a mere symptom, and may arise from diseased vessels, diseased cerebral substance or membranes, vitiated blood, &c. &c., or may be due to some cause altogether external to the cranium, which may impel blood too forcibly to the brain, or may prevent its ready egress therefrom. What we should seek to determine ought to be the relative frequency with which the several conditions occur, or are brought together, from which the epiphenomenon, which we term hemorrhagic apoplexy, results.

Passing over the next chapter on the Mental Diseases of Old Age, we arrive at a section on Neuralgia. From a table in part derived from Valleix, and in part collected from original sources, Dr. Day concludes that neuralgia is relatively most common between the ages of 50 and 60; it may, however, occur at any age.—After describing the usual modes of treatment, Dr. Day states that if anodynes are requisite, he prefers the cannabis indica to opium. He speaks very highly of the "thermic treatment," to an exposition of which the appendix of the work is devoted. The thermic treatment, which has lately been recommended by Dr. Corrigan, consists in the momentary repeated application to the skin of a flat iron button, which is gently heated in a spirit lamp. In this way, according to the heat, the effect of a mustard poultice, of a blister, or even of a moxa, may be produced. The iron button is about half an inch in diameter, and a quarter of an inch in thickness, and is connected with a wooden handle by a curved shank. With this heated iron Dr. Corrigan taps the part to be affected, but Dr. Day prefers drawing it lightly over the surface. This mode of applying counter-irritation has been found useful not only in neuralgia, but in chronic rheumatism, and some forms of paralysis.

---

* London Journal of Medicine, January, 1849.

Having discussed the diseases of the nervous system, Dr. Day proceeds to those of the Digestive Tube and its appendages. A good practical summary is given of these affections, in which, however, we find nothing which need detain us.

The chapter on Diseases of the Circulating Organs only occupies twelve pages. The changes which occur in the heart and arteries, with the progress of age, are summed up in a quotation from Hasse, respecting the enlargement of the auriculo-ventricular and of the aortic orifices. There is no mention of atheroma, calcification, fatty degeneration, and organic diseases of the valves, coming on in consequence of an impaired nutrition, and which are therefore naturally in place in a treatise on the Diseases of Old Age. In fact, we think Dr. Day has decidedly not done justice to this subject, nor was it indeed possible that he could do justice to it in the space to which he has chosen to confine himself.

In the section on Diseases of the Kidney, we observe that the author deprecates the administration of large doses of the alkalies or their carbonates, with the view of dissolving concretions existing in the kidney or bladder. Referring to the possible solution of stone in the bladder, Dr. Day states that there can be little doubt that calculi of uric acid, urate of ammonia, and mixed and triple phosphates may, under favorable circumstances, be dissolved or disintegrated in the bladder by the free imbibition of natural or artificial water containing alkaline bicarbonates. He informs us that, at some future period, it is his intention to publish the evidence he has collected on this subject.

In the following section on Diseases of the Skin, we may extract the following directions respecting the treatment of prurigo senilis:

> "The causes of prurigo are much the same as those of the preceding skin disease —bad nourishment, want of cleanliness, impurity of the blood. I need not revert to the constitutional treatment required in these cases, further than to remind the reader that this affection is often accompanied, if not preceded, by a diminished secretion of urine, and that this circumstance may furnish us with a hint in reference to treatment. In addition to regulating the biliary secretion, we must try the effects of diuretics. A great deal may sometimes be effected by the use of the alkaline salts with a decoction of juniper berries or broom by squills, or by turpentine and cantharides. To improve the general tone of the system we may give the decoction of sarsaparilla and nitric acid, and allow a mild nutritious diet. With regard to external applications, I can lay down no general rules. A lather of soap and hot water rubbed over the affected parts with a sponge or soft brush, night and morning, is often productive of much comfort. Dr. Graves recommends the application of a lotion composed of a pint of whiskey and a drachm of laudanum, applied hot with a sponge at bedtime. Dr. Bellingham has recently advised the local application of creasote." (p. 291.)

In the next chapter on Senile Gangrene, Dr. Day advocates the plan of treatment adopted by Mr. Syme, viz.: a rigid vegetable and milk diet, abstinence from every sort of stimulant, the free use of opiates as long as the pain continues, the maintenance of complete rest, and the application of a poultice, or of cotton to the part. Dr. Day considers that this plan is attended with far greater success than the usual tonic and stimulating treatment.

The last chapter in the book gives a useful practical account of gout and chronic rheumatism, but our limits do not permit us to enlarge upon these points.

In concluding our brief analysis of this work, we may remark that it bears evidence of the practical skill of the writer, and that the directions for treatment will doubtless be useful to the practitioner who needs a *refresher* to his memory, but who has not time to undertake the perusal of any lengthy work. Yet we must candidly state that Dr. Day would, in our opinion, have done more justice to his subject and tc himself, if he had endeavoured to make his work a comprehensive exposition of everything relating to the physiology and pathology of old age. It is true that much of our knowledge has not received its application in practice, but still it *is* knowledge, and may at any time become pregnant with important indications for treatment. The hygiène and domestic management of Old Age might well form a separate treatise, adapted for the general reader, upon the plan of Dr. A. Combe's valuable work on the Domestic Management of Infancy.

Art. XV.

The Poetry of Science, or Studies of the Physical Phenomena of Nature. By Robert Hunt, Author of 'Researches on Light,' Keeper of Mining Records in the Museum of Practical Geology.—*London.* 1848. 8vo, pp. 464.

It is too frequently supposed that the close examination of material realities, and the application of the intellectual faculties to the search for truth in the discovery of their relations and the development of their laws, are antithetically opposed to the exercise of the imaginative powers, and to the enjoyment of those pleasures which are derived from the simple contemplation of beauty. If we look deeply, however, into the tendency of philosophy, we shall perceive that it has much in common with that of poetry. Both are expressions of the desire of the human mind for something more pure, more harmonious, and more complete, than the world around us seems to afford; and this desire may manifest itself in a variety of ways.—In its highest form it is the source of that longing after immortality which we believe to be peculiar to man, and to be one of those points by which his psychical character may be distinguished from that of brutes. Again, it excites that desire for beauty and harmony, which gives rise to the exercise of the imagination in the creation of poetic fictions; which are fictions, as it has been justly observed, only in the *new combination* of the ideas which, separately considered, are true; just as the statue, modelled by the Grecian sculptor from the finest parts of many distinct figures, and combining their several excellencies into one beautiful and harmonious representation, may be said to embody the poetry of the human form. But this same tendency may manifest itself in another and very different direction. It may stimulate the intellectual powers to the consideration of the phenomena of Nature, in such a manner as to discover, in their *existing combinations*, the real uniformity of plan which is concealed amidst the countless diversity of result; and thus to display an order, a beauty, a harmony, a majesty, more glorious than anything which the fictions of poetry can produce, because *real*. In this point of view, then, to cultivate with the physical philosopher the higher domains of science, is really a nobler employment than to soar with the poet through the elevated flights of his imagination. With the former, we abstract ourselves from earth in proportion as we rise; the grander are the generalizations we attain, the nearer are we led towards a glimpse of the workings of the Infinite Spirit, and the less have we to do with material realities; with the latter, we are always in a degree bound down to earth, however lofty may be the point from which we survey it. The poet attempts to produce a new harmony, by an arrangement of elements which is really discordant because unnatural; whilst the philosopher discovers a secret harmony where all appeared discordant, order where everything seemed confusion, law where all might be supposed to be anarchy.

It is recorded of one of the kings of Portugal, that, when instructed in the Ptolemaic system of astronomy, with all its cumbrous fictions of cycles and epicycles, which had become necessary to reconcile this erroneous theory with the results of observation, he said with an oath—"If I had been the Creator, I would have done the work better." Some such tendency must surely exist in the mind of the poet, when he endeavours to combine distinct images of individual beauty into ideal perfection. Is it not as if he were to take the solar system as at present known to us, strike off from it those perturbations which appear to the uninstructed mind to be irregularities, and then say?—"See how much better I have made your performance than it was before! The planets now revolve regularly round the sun, and carry their satellites with them in uninterrupted curves, without the slightest disturbance." Such is the view of the state of the external world taken by certain poets, and such the mode in which they would improve it. What is the philosopher's? He observes the conditions of these irregularities and perturbations; and tracing them backwards to their source, he finds it in the very cause of the motions themselves, and proves the impossibility of the performance of these motions without the existence of their seeming irregularities. And going still higher, he arrives at the grand principle of *compensation*, by which these perturbations are made to restrain and balance each other; and shows that the simple law of mutual attraction operates not merely in keeping the planets in their destined paths, nor yet only in occasioning slight deviations from these, but also in maintaining the stability of the whole system, by antagonising and restraining every deviation which its own agency produces. Whose notions, then, of beauty and harmony are the most exalted,—those of the poet, or those of the philosopher?

But it may be said that we have placed the functions of the poet and of the philosopher in an unnecessary antagonism; the real office of the poet not being to create an ideal world "with all the newest improvements," but to disclose the soul of beauty which lies hid within the existing material system, to invest the real world with a brightness and a glory which the unimaginative mind cannot of itself discover in it. If this be his idea of his mission, he may pursue it in the fullest harmony with the philosopher; each aiding the other in the discovery of truth and in the development of beauty; and each, by the exercise of the faculties with which he has been endowed, thus taking his share in the work of rendering mankind happier as well as wiser.

Such are the views which we have long since entertained, and taken occasion to express; and with such views, we are prepared to give a hearty welcome to a work conceived in the same spirit, and executed by a writer having many qualifications for the task he has undertaken.

> "An attempt has been made in this volume," says the author in his Preface, "to link together those scientific facts which bear directly and visibly upon natural phenomena, and to show that they have a value superior to their merely economic applications, in their power of exalting the mind to the contemplation of the universe. . . . . For the purpose of exhibiting the great facts of science in their most attractive aspects, the imagination has been occasionally taxed, but it has never been allowed to interfere with the stern reality of truth; and throughout the following pages it is hoped there will be discovered—however illogical to some, certain speculations may appear—evidences of a constant desire to think honestly, and to give to every discovery its full value in the generalizations attempted."

From the following enumeration of the headings of the chapters, our readers may derive a general idea of the contents of the work: I. General Conditions of Matter. II. Motion. III. Gravitation. IV. Molecular Forces. V. Crystallogenic Forces. VI. Heat, Solar and Terrestrial. VII. Light. VIII. Actinism, Chemical Radiations. IX. Electricity. X. Magnetism. XI. Chemical Forces. XII. Chemical Phenomena. XIII. Time, Geological Phenomena. XIV. Phenomena of Vegetable Life. XV. Phenomena of Animal Life. XVI. General Conclusions.

As a specimen of the manner in which these subjects are treated, we may extract the following passage from the chapter on Electricity:

> "It is sufficiently evident from the results which have been examined, that all matter, whatever may be its form or condition, is for ever under the operations of the physical forces, in a state of disturbance. From the centre to the surface all is in an active condition; a state of mutation prevails with every created thing; and science clearly shows that influences are constantly in action which prevent the possibility of absolute repose.
>
> "Under the excitement of the several agencies of the solar beams, motion is given to all bodies by the circulation of caloric, and a full flow of electricity is sent round the earth to perform its wondrous works. The solar influences, which we regard as light, heat, actinism, and electricity, are active in effecting an actual change of state in matter, and in all probability in influencing the great magnetic phenomena of the world. The sunbeam of the morning falls upon the solid earth, and its influence is felt to the very centre. The mountain top catches the first ray of light, and its base, still wrapped in mists and darkness, is disturbed by the irradiating power. The crystalline gems, hidden in the darkness of the solid rock, are dependent for that form which makes them valued by the proud on the influence of those radiations which they are one day to refract in beauty. The metals locked in the chasms of the rifted rocks are, for all their physical peculiarities, as dependent upon solar influence, as is the flower which lifts its head to the morning sun, or the bird which sings 'at heaven's high gate.'" (p. 183.)

Mr. Hunt is not quite as well versed in physiological as he is in physical science; and we should recommend him, whenever he has an opportunity of revising his book, to obtain the assistance of some well-informed professional friend, who would correct some inaccuracies, and perhaps give him some valuable suggestions. For instance, we find him referring (p. 185) to the researches of Phillips on the action of the organs of digestion, as indicating the agency of electricity in the living body. By turning to the authority referred to at the end of the work, we find, as we anticipated, that "Phillips" means Dr. Wilson Philip; whose experiments on this subject have been long since set aside as valueless in the eyes of the physiologist, by the more accurate researches of Dr. Reid and others; but who is more quoted, perhaps, by those who venture from the physical side into the border ground between the two domains, than any other author.

We should gladly multiply our extracts from this work, did our space permit; but we must content ourselves with recommending it to the attention of our readers, as one from which we are sure that they will derive both gratification and instruction.

Art. XVI.

The Diagnosis and Treatment of Eruptive Diseases of the Scalp. By J. Moore Neligan, m.d., Physician to Jervis Street Hospital, &c.—*Dublin*, 1848. 12mo, pp. 56.

Of a truth, Diseases of the Scalp seem doomed to endless confusion. In a late Number we have had occasion to correct a very serious mistake on the part of Mr. Erasmus Wilson, who unhesitatingly stated, in his 'Essay on Porrigo,' that the common ringworm is not contagious! whereas every hospital nurse, nay, every old wife in the kingdom, knows the reverse to be the fact. It is now our disagreeable duty to point out an equally false statement in the monograph on the same subject before us. Dr. Neligan asserts, with unlimited confidence, that—

"Herpes capitis *is the true ringworm of the scalp.*"

The italics are the author's, and are, of course, intended to mark the intensity of his belief in the doctrine he enunciates. Nevertheless, we have no hesitation in saying that it is altogether wrong. Herpes is not ringworm; nor is it an idiopathic disease of the scalp in any shape or form. That herpetic vesicles are to be seen on the forehead and temples, sometimes extending from the cheek in the form of herpes circinatus, is quite true; but they never extend beyond the margin of the hair, and are commonly the result of local irritation, or, in other words, are purely accidental. We believe there is not a practitioner in the United Kingdom, possessed of any experience in the treatment of the eruptive disorders of the scalp, who will coincide with Dr. Neligan's very singular idea as to the elementary nature of ringworm. The fact is, a great part of his description of "herpes capitis" is so like that of eczema of the scalp, that it is evident he has mistaken a variety of the latter eruption for herpes. No form of herpes ever terminates in a true scale, but rather in a kind of thin film. This is not the case with eczema, especially that variety which appears on the scalp; for here the secondary product very nearly approximates to that of a scaly disease. Moreover, it has never yet been shown that any vesicular eruption, occurring on the human skin, is contagious. The vesicles which appear in scabies, like the pustules with which they are complicated, are purely accidental, being the result of the irritation produced by the acarus in his burrow; and it is solely by means of the insect, and not by that of the contents of either vesicle or pustule, that the disease spreads, and is propagated from one individual to another. This fact has been clearly demonstrated by the experimental researches of MM. Renucci, Gras, Raspail, and Hebra.

Dr. Neligan quotes a recent work* by M. Cazenave in support of his view, in which the French physician gives an account of a "new cutaneous disease of the scalp." This disease M. Cazenave proposes to call "herpes tonsurans," and he describes it as a contagious vesicular eruption of the head; but he admits that he has seen the complaint only once or twice, in a public school, and that quite recently. We have no doubt that a more extended acquaintance with the so-called "herpes tonsurans" will enable the distinguished physician of St. Louis to modify, if not to alter, his view as to the nature of this disease, which by his own showing has been hastily formed, and upon very insufficient data. The senior medical officers of the Royal Infirmary for Children in London, who have been quite as long attached to that institution as M. Cazenave has been to the Hospital of St. Louis, have not seen a single case of herpes occurring as an idiopathic disease of the scalp.

At page 15 Dr. Neligan says, that "impetigo is the only pustular disease which is met with on the scalp," a statement equally erroneous with that relating to ringworm; for ecthyma, another pustular eruption, is by no means unfrequently met with there; and singularly enough, the author seems to have been, at one time, aware of that fact; for at page 5 we find him apologising for not including this very disease amongst eruptions of the scalp, and alleging as his reason for so doing, "that it is rarely met with there unless in connexion with its existence on the skin generally, and its presence on the scalp not requiring any special plan of treatment." From this also we must dissent, for when ecthyma occurs on the hairy scalp, it will require the same "special" treatment as any other pustular eruption of that part; and the reason which the author gives for excluding it from his classification would apply with equal force to impetigo of the head, which very frequently coexists with impetigo figurata of the face.

Dr. Neligan divides diseases of the scalp into inflammatory and non-inflammatory. In the first division he places herpes, eczema, impetigo, pityriasis. In the second, porrigo capitis. Does the author maintain that there is no kind of inflammation present during the early stage of porrigo favosa? Never was there a greater mistake. Porrigo capitis is a chronic inflammation of the hair follicles, associated with a peculiar yellow formation, in which the vegetable parasite is subsequently produced. There is no disease of the scalp unattended by inflammation in the early stage. The author might, with an equal amount of accuracy, have placed in his non-inflammatory group pityriasis, which, by the by, he seems to look upon as a sort of mongrel, or half-and-half inflammatory disease. A division of diseases of the scalp into acute and chronic, if the author is bent upon a new arrangement, would be equally simple, and free from the pathological contradictions involved in his own.

---

* Leçons sur les Maladies de la Peau. Paris, 1847.

Of the four diseases included in the inflammatory group, the author has met with eczema most frequently; next, impetigo; then herpes; and pityriasis least frequently. With reference to their prognosis, as to curability, his experience leads him to the following conclusions: 1st, the scattered form of impetigo; 2d, pityriasis; 3d, the moist form of eczema; 4th, confluent impetigo; 5th, herpes (the pseudo-ringworm); 6th, the dry form of eczema.

Dr. Neligan informs us, that two principles must be laid down to enable the treatment of the preceding eruptions to be based "on rational, and not on empirical principles: first, that they are inflammatory; and, secondly, that they are constitutional affections." And further, he is "convinced, that to the neglect of these two principles the difficulty of treating scalp diseases, as so generally acknowledged, must be ascribed." (p. 23.) We freely subscribe to the accuracy of the postulate in the preceding quotation; but, at the same time, it appears to us that the author is either labouring under a delusion, or else he libels the profession, when he declares, that it is owing to the neglect of his "two principles" that scalp eruptions are still so imperfectly understood. We venture to say, there is not a student of two years' standing, of ordinary industry, who does not know that the majority of skin diseases, not only of the scalp, but of the entire body, are both inflammatory and constitutional affections. It is a truism of much longer standing and wider circulation than Dr. Neligan seems to have any idea of; and with regard to the author's belief, that the general acceptation of the "two principles" is the only thing requisite for establishing a treatment "on rational, and not on empirical principles," we fear that he is doomed to disappointment. What is the system of therapeutics, as it at present stands, but a rational empiricism? And the author need not go farther than his own excellent work on the uses of medicines, to find that all that is positive in the system is, that sulphur cures the itch, and bark the ague. Until such time as medicine is reduced in reality to a science of observation, instead of one of conjecture, it is but idle vanity to look for a system of therapeutics that will abide.

We find nothing in the shape of novelty in the plan of treatment recommended, which consists of the usual measures, of cutting the hair close, applying fomentations and poultices to remove the incrustations, and subsequently alkaline lotions and ointments alternately. The only internal remedy mentioned is an occasional alterative compound of the protoiodide of mercury, hydrargyrum cum cretâ, and aromatic powder. A milk diet is strictly enjoined during the treatment. Such are the author's views as to the nature and treatment of his first, or inflammatory, group of eruptive diseases of the scalp.

Porrigo capitis is the alpha and omega of Dr. Neligan's second and non-inflammatory "group." (?) He is a thorough believer in the modern theory of the vegetable nature of this disease; and as he commenced the first division of his essay with the startling and original announcement that herpes is the true ringworm, so he must needs usher in the second part with an equally unique dogma, supported, as in the former instance, by all the force that italics can give it. Here it is:

"*Porrigo favosa is the true scald-head*"!

Dr. Neligan says, that, although the vegetable nature of porrigo capitis is denied by Mr. Wilson, he has been able to convince himself of the accuracy of that view by personal examination of the diseased product in *two* instances. From this it would appear that the author is not difficult to be convinced. Mr. Wilson is perfectly right in denying that the anatomical element of this disease is a vegetable, whatever its secondary product may be. But Mr. Wilson is not the only writer on cutaneous pathology who denies it. M. Cazenave is still more sceptical on that point. Mr. Erichsen, in his 'Treatise on the Scalp,' is equally opposed to the vegetable theory, so far as the *elementary nature* of the disease is concerned; and Dr. Burgess, in a recent work on 'Diseases of the Skin,' entertains a similar view. But as the author's "rational principles" of treatment are founded on his theory of the nature of the disease, we shall give the latter in his own words: "Porrigo capitis is a vegetable production, which grows and is reproduced on the cuticular surface of individuals whose system is in a peculiar cachectic state; and, consequently, it is a constitutional affection." Mark the postulate and its corollary in this paragraph:—porrigo capitis is a vegetable production; consequently, it is a constitutional disease. As for the "peculiar cachectic state," it is one of those loose, vague methods of expression, which means anything or nothing, so happily devised for the protection of the bold and inventive Dædaluses of physic, as well as for the mystification of ingenuous youth. Surely, the author's logic in this passage would bear revision, if not correction; unless he adopts for his text-book the celebrated "Bob's Logic," which undertakes to prove to a demonstration that two and two make five.

After laying down the "principles" upon which his treatment is founded, the author proceeds: "The object is, then, to destroy the vitality of the fungus; and, by altering the nature of the soil on which it flourished, to prevent its reproduction." Now, it appears to us that these principles of treatment are anything but "rational." If the proposition to commence the treatment of a disease pronounced to be a *constitutional* one, by the destruction *externally* of a fungous growth, be not empiricism, and irrational empiricism too, we know not what is. Dr. Neligan proposes to destroy the vegetable on the scalp by an ointment composed of the iodide of lead, which he erroneously supposes has been first employed by himself in scalp diseases,—M. Hebra, of Vienna, having employed it for years,—and then "altering the nature of the soil," by the administration internally of the iodide of arsenic. This is, in fact, the sum and substance of the author's treatment for porrigo capitis. The rational treatment, we presume, would be, to endeavour to remove the vicious state of constitution, by which the seeds of the disease were generated, before having recourse to any local measures, other than those of cleanliness. For where is the wisdom or utility in destroying the branches of the fungus externally, until the source whence it springs, according to the author's own showing, be first sapped by constitutional remedies? Every practitioner knows how easy it is to remove from the scalp the most extensive incrustations of porrigo favosa, by poultices or fomentations and detergent lotions, so as to make the diseased surface look clean and almost healthy; but it is quite another matter to prevent the reappearance of the disease, and perhaps in a more aggravated form.

In conclusion, we have merely to say, that Dr. Neligan's essay on Diseases of the Scalp will form a fitting companion for that of Mr. Wilson on Ringworm, reviewed in a late Number. Both have their merits as well as their faults; and, notwithstanding their errors, neither is altogether devoid of practical utility.

ART. XVII.

Chemical Reports and Memoirs. Edited by THOMAS GRAHAM, V.P.R.S., Corresponding Member of the Institute of France, Professor of Chemistry in University College, London, &c. &c. Published by the Cavendish Society.—*London*, 1848. 8vo, pp. 370.

WE gladly avail ourselves of the opportunity afforded us by the appearance of this volume, to bring under the notice of our readers the plan and objects of the Cavendish Society. Like the *Sydenham*, the *Ray*, the *Palæontographical*, and other societies formed for the publication of works on special departments of science or literature, which would not otherwise be likely to find a remunerating sale, the *Cavendish* has for its object "the promotion of chemistry and its allied sciences by the diffusion of the literature of these subjects;" and this is to be effected "by the translation of recent works and papers of merit; by the publication of valuable original works which would not otherwise be printed, from the slender chance of their meeting with a remunerative sale; and by the occasional republication or translation of such ancient or earlier modern works as may be considered interesting or useful to the Members of this Society."

The utility of such societies, when judiciously managed, has been now so fully demonstrated, that it is not requisite to say anything in their behalf; and no one can affirm that Chemistry is less worthy of a separate society for the promotion of its objects in this mode, than are Medicine, Natural History, and Palæontology. In one respect, indeed, there would seem a peculiar demand for a Chemical publication Society; the accessions to the science by the labours of foreign chemists being so rapid and extensive, that a judicious selection from the results of their labours, cannot but be of the greatest value to those of Britain, too many of whom are but very imperfectly acquainted even with the best text-books, much less with the contents of the numerous chemical journals, published on the Continent.

The Society has exercised, we think, a wise discretion in commencing its issues with a volume of isolated Reports and Memoirs; and in following this up by a work of such standard value as Gmelin's Chemistry, which last is now in course of publication. The 'Reports on Atomic Volume and Isomorphism,' by Professor Otto of Brunswick, and the 'Memoir on the Connexion existing between the Atomic Weights, Crystalline Form, and Density of Bodies,' by M. Filhol, have reference to an investigation of great interest, which is now in progress on the continent, and which promises to throw great light on the laws of the molecular constitution of bodies. The chief principle enunciated by Kopp, now on its trial for reception as a scientific truth, is as follows:—that if the atomic weights or combining equivalents of bodies be divided by their specific gravities, we shall have a series of numbers that may be said to represent their *equivalent volumes;* and that these equivalent volumes are *the same* for the several bodies of the different isomorphous groups, or have a very simple numerical relation to each other. Thus, in the following table, the first column represents the atomic weights, and the second the specific gravities, of the bodies forming two isomorphous groups; whilst the third column gives the quotient obtained by dividing the first by the second, which represents the equivalent volumes of these bodies.

	Atomic Weight.	Specific Gravity.	Equiv. Volume.
Iridium	1232	21·60	57
Osmium	1242·5	21·80	57
Palladium	665·5	11·70	57
Platinum	1232	21·60	57
Titanium	301·5	5·33	56
Zinc	406	6·95	58
Bromine	1000	3·06	326
Chlorine	443·2	1·38	320
Cyanogen	325	1·03	315
Iodine	1585·5	4·93	320

It will be seen that the correspondence is not exact; but it is sufficiently close to indicate a relationship; and numerous sources of error may be found in the inexact determination of the atomic weights and of the specific gravities of the several bodies, and in the inequality of their respective distances from the point of fusion, which must obviously affect the accuracy of the determination of their equivalent volumes from specific gravities all taken at a fixed temperature. A number of subordinate questions are connected with this general principle, such as the relation between the volume of a compound and those of its components; and to these we doubt not that the industry and sagacity of chemists will ere long yield a satisfactory solution. These Memoirs cannot be read without great interest by all who are desirous of following the advance of the general principles of chemistry.

With the "Report on the Laws according to which the Mixing of Fluids and their Penetration into Permeable Substances occur, with special reference to the Processes in the Human and Animal Organization," by Dr. Julius Vogel, we must confess ourselves to be somewhat disappointed; as we do not see in what essential particular it adds to our knowledge of the subject, whilst it leaves many important points untouched.—The Memoir entitled, "*Physical* Investigations on Dyeing," by M. Chevreul, might, with as much propriety, have been designated "*Physiological* Investigations," for it is entirely devoted to an investigation of "the influence that two colours may exercise upon each other when seen simultaneously," which influence must entirely result from the mode in which the organ of vision is affected by them. By this investigation many interesting facts have been brought out. The author shows that if we look simultaneously at two narrow bands of different colours, placed side by side, the colours will be more or less modified. This may be easily demonstrated by taking four strips of some coloured fabric, or of paper, two of each colour, as, for example, two of blue, which we may call B and B′, and two of red, which we may call R and R′, and laying them in the order thus represented, B, B′, R′, R; then, on looking at the group for a little time, and from a certain distance, we shall almost certainly see four differently-coloured bands, the contiguous inner bands B′ and R′ having their hues modified by proximity to each other, whilst the outer bands B and R retain their original aspect. In this case the change will be the approximation of the red towards orange, and of the blue towards green. The same thing will happen when the zones are not in close contiguity with each other, a distance of half an inch being left between the two central bands, and of a quarter of an inch between those on either side.

After making numerous experiments with a variety of colours, and with other eyes besides his own, M. Chevreul arrived at this simple expression of the alterations which they underwent by contiguity,—"that in the case of the eye seeing at the same time two colours which are in contact, they will appear as dissimilar as possible," an expression, however, which seems to us to require considerable softening down to make it accordant with the real fact, which is, that each colour will *tend to* approach the complementary colour of the other. Thus, in the case just cited, the complement of red being green, the blue will present a greenish tinge, as if yellow had been mixed with it; whilst the red will have a tendency to pass towards orange, the complement of blue, as if yellow had been mixed also with it. When the colours are complementary, being already as dissimilar as they can be, they will undergo no change by being placed in contiguity; but if they are not perfectly so, a change will be apparent,—thus a red will be differently affected by a diversity of shades of green, being made to incline towards violet or towards orange, according as yellow or blue are predominant in the green. The tendency to dissimilarity will show itself in a somewhat different mode when both colours are compound; for the colour which is common to each will tend to disappear. Thus, when orange and green are contrasted, the two colours, which have yellow as a common element, lose it by juxtaposition, the orange appearing more red, and the green more blue. Or if green and violet be contrasted, the blue common to both will be lost sight of, and the green will seem more yellow, and the violet more red. A little reflection will show that these results are accordant with the general law of complementary colours; for just as, when the eye is strongly impressed with a bright orange, a contiguous white will appear bluish,—because the eye, powerfully affected with the orange, will not discern that colour in the fainter white, but will only see the blue,—so it will always tend to approximate each of two dissimilar colours seen together to the complementary colour of the other.

The application of these principles to the heightening of the results of contrasts of colour in the various decorative arts, are very numerous. They do not so much affect the painter, as the weaver of textile fabrics; for the following simple reason:

"A painter may, to a certain extent, succeed without a knowledge of the law of simultaneous contrast; for if he produce an effect with which he is not satisfied, depending upon this phenomenon, his palette furnishes him with the means of destroying or increasing it. The weaver has not the same resources at his command. He cannot replace his colours without undoing his work, and to avoid this necessity he ought to be sufficiently well acquainted with the law of contrast to judge of the effect that will be produced by the vicinity of other colours upon the coloured threads he employs in the imitation of a certain part of his pattern; otherwise it will be impossible for him to reproduce a faithful copy of the painting before him." (p. 199.)

Having explained the physiological law, we must refer those of our readers who desire to see its various practical bearings, to the memoir itself, which presents many points of great interest.

A valuable Memoir follows, by M. Regnault, "on the Latent Heat of Steam at different pressures." This is a portion of a series of investigations undertaken by that chemist, at the instance of the French government, to elucidate the theory of the steam-engine; and it is an excellent example of the value of theoretical investigations, in the hands of one who thoroughly understands their practical bearing. As its subject, however, does not fall within our range, we shall pass it over, to notice briefly the report by Professor Kopp on "the Artificial Formation of Alkaloids;" a subject whose importance will be evident from the following extract:

> "There are few questions more deserving of the attention of chemists and pharmaceutists, and of being made the object of their researches, than the formation of alkaloids. In a chemical point of view, the alkaloids are remarkable for their composition, for their special properties both physical and chemical, and for the interesting reactions to which many of them give rise, when exposed to the influence of different agents. Considered medically, the organic bases are distinguished by their energetic properties. They constitute, at the same time, the most violent and sudden poisons, and the most valuable and heroic remedies. Hence their use in medicine is very extensive, while their price is often exorbitant. It is more than probable that, by studying the mode of their formation, we may in course of time be led to prepare artificially, and at little expense, morphine, quinine, strychnine, &c., so as to render these medicines available to the poor, and to free ourselves from the necessity of heavy payments to foreign countries." (p. 297.)

However sanguine this speculation may at first sight appear, it derives a certain justification from the fact that chemists have already formed a tolerably large number of artificial organic bases, similar in all respects to the natural alkaloids; from which it may be justly concluded, that the methods adopted in our laboratories must be analogous to those employed by Nature herself in their production, as a part of the operations of vegetable growth. And although to the minds of some, the idea of obtaining by artificial means the products of what they choose to distinguish as "vital" chemistry may appear altogether chimerical, yet it must not be forgotten that in a considerable number of cases such products have been obtained; sometimes (as in the case of urea) by the union of simpler compounds, in other cases by the conversion of one organic compound into another. We shall cite one or two examples of the latter kind from the Lectures of Dr. Golding Bird, to which we have referred in a previous article. The fragrant essential oil of *Spiræa Ulmaria*, or "queen of the meadows," may be produced by treating salicine, the alkaline base of willow bark, with bichromate of potass and sulphuric acid, whereby it receives two additional equivalents of oxygen, and is resolved into Oil of Spiræa and grape sugar. The latter product, by a further oxidation, is converted into formic acid (naturally secreted by the red ant as a defensive fluid), carbonic acid, and water.—So, again, valerianic acid, to which Valerian-root owes its pungent fetid odour, may be obtained by the action of caustic potass on the hydrated oxyde of amyle, which is an analogue of alcohol, obtained during the distillation of spirit from fermented grain or potatoes. The essential oil of Mustard may be converted into that of Garlic, by dropping into the former a piece of potassium, which takes from it two atoms of carbon, one of nitrogen, and one of sulphur, so as to become sulphocyanide of potassium, the residuary atoms being precisely those which enter into the composition of the oil of garlic, whose presence is at once made known by its odour. And lastly, the essential oil of the *Gualtheria procumbens*, a beautiful ericaceous plant of New Jersey, which oil has become an article of commerce from its beautiful perfume, combining the odours of hyacinth and cinnamon, may be obtained by distilling salicylic acid (which may be procured by heating indigo, or salicine, or the oil of spiræa, with caustic potass), with pyroxylic spirit or wood naphtha, and a little sulphuric acid.

Such transformations are fully as remarkable as would be the production of quinine, strychnine, or morphine from other organic compounds; and we do not see, therefore, anything visionary in the search for the means of effecting this. Two methods, as Professor Kopp points out, may be adopted in this search; either the study of the modes by which these compounds are produced in nature, or an experimental investigation on the artificial production of alkaloids. As to the first of these, it is justly remarked, that nothing has as yet been effected, and that we are ignorant of even the first elements of the investigation. The path, however, is clear for the inquirer. The composition of plants, especially of those yielding these remarkable principles, should be carefully examined during the different phases of their vegetative life; it should be ascertained what proximate principles are first formed, what afterwards disappear, and what new ones result from them; and thus a clue should be sought for, connecting the first and the last. An investigation of this nature, extending to all the most important plants, would undoubtedly be rich in new and interesting results.

> "Such a work would fall within the domain of the pharmaceutist, who, being perfectly familiar with the ordinary modes of their extraction, could, without any great difficulty, isolate pure and frequently crystallizable principles; if their elementary analysis and their exact description were beyond the ordinary means at his disposal, any chemist would willingly carry on the examination of the substances thus isolated, and in this manner contribute his share to the advancement of his own science." (p. 299.)

In regard to the study of the modes of preparation of artificial alkaloids, Professor Kopp justly remarks that "attention should be especially paid to those methods which most nearly approximate to those employed by nature, and do not require a high temperature, or the influence of energetic reagents;" and the same remark is repeated in reference to the study of the products of decomposition of the natural alkaloids, under the influence of different reagents, and under different circumstances.

> "It is a recognised fact in chemistry, that the manner in which a substance undergoes decomposition, and the products it yields, frequently enable us to draw conclusions regarding the mode of its formation, and the course to be pursued in forming it. But the decomposition must not be accompanied with much violence,

if we wish to establish the relations existing between the primary body and the derived products, and the equation illustrating the change that occurs. It unfortunately happens that most of the investigations that have been undertaken in relation to the changes which the alkaloids undergo, have in no way come up to these conditions, and consequently possess only a moderate degree of interest." (p. 302.)

Of the bases produced by artificial means, one of the most remarkable is *furfurine;* for the discovery of which we are indebted to Dr. G. Fownes, whose early death must be regretted by every lover of chemistry, as well as by all those who had the privilege of personal intercourse with one who presented, under the most unassuming exterior, a rare combination of the best qualities of the scientific man. This substance in its composition and properties approximates the most to the well-characterised natural alkaloids; and Professor Kopp does not fear to affirm that we shall some day or other be indebted to it for the discovery of several new alkaloids. The following brief account of it is derived from Dr. Fownes's paper on the subject in the 'Philosophical Transactions' (1845), for which he was rewarded with the royal medal. When bran is treated with sulphuric acid, an oily substance may be distilled over, which has an odour resembling that of a mixture of bitter almond oil and oil of cassia; this substance appears to be identical with an oil first noticed by Dobereiner as appearing in very small quantity in the preparation of artificial formic acid; and afterwards studied by Dr. Stenhouse. The composition of this substance, termed *furfurol,* is represented by the formula $C_{15}\ H_6\ O_6$. When brought into contact with ammonia, furfurol becomes transformed into a solid slightly crystalline substance, insoluble in water; the change it undergoes being such as would be produced by its union with one equiv. of ammonia, and the separation of three equiv. of water. This new substance obviously belongs to the class of *amides* or compounds of ammonia; and much resembles the *hydrobenzamide* of M. Laurent. It is immediately decomposed by acids, which form an ammonia-salt, and set free the original oil; and its decomposition into free ammonia and volatile oil is even slowly effected by boiling water, more slowly still by the influence of atmospheric moisture at ordinary temperatures. But when boiled with weak caustic potash, it dissolves without the least evolution of ammonia; and the liquid deposits, on cooling, small white, silky needles of a new substance, having *the same composition* as the amide itself (namely, $C_{15}\ H_6\ N\ O_3$), but all the properties of a stable and exceedingly energetic organic base, possessing alkalinity, and having the power of forming with acids an extensive series of well-defined, and, for the most part, crystallizable salts of great beauty. So powerful is its alkaline reaction, that it will decompose sal-ammonia, appropriating to itself the hydrochloric acids and setting free the ammonia. Of the extraordinary change produced in the characters of this amide by the presence of the alkali, its ultimate composition remaining precisely the same, Dr. Fownes offers the hypothesis that it consists in a new arrangement of the atoms, involving a *duplication* of their equivalents: the proportion of the components of the alkali required to form a perfectly neutral compound with an acid, being expressed by the formula $C_{30}\ H_{12}\ N_2\ O_6$, which is double that of the amide. The combining equivalent of the latter, however, is uncertain; so that this explanation cannot be regarded as anything else than hypothetical.

With so remarkable a case before us of the artificial production of an alkaloid base of such complex constitution from materials so unpromising (a saccharine or starchy substance appearing to be a sufficient foundation for the whole process), we think that there is adequate ground for the hope that the industry and sagacity of chemists will ere long be successful in obtaining analogous results of more practical importance. The "Report" of Professor Kopp, by bringing together all that has been hitherto done on the subject, constitutes the best preparation for its further investigation. He refers especially to a memoir (still unpublished), by MM. Redtenbacher, Rochleder, and Wertheim, as promising more important results on the nature and composition of the alkalies, than any previously made known. These chemists have announced, that Piperine should be regarded as a neutral combination of a nitrogenous acid with an alkaline base, termed Aniline, which may be obtained from a great variety of sources; they have even succeeded in reproducing Piperine synthetically, by combining the acid and the base. In like manner, they find that Narcotine may be separated into a non-nitrogenous acid, combined with a peculiar base. These compounds are constituted with perfect regularity; and it seems very likely that most of the alkaloids having high equivalents thus admit of separation into more simple elements, which, in all probability, will be common to many of them. The confirmation of this fact would be an important step in the true progress towards our knowledge of the artificial preparation of the alkaloids in general.

The last paper in the volume is a Memoir by Professor Bunsen of Marburg, on the Pseudo-volcanic Phenomena of Iceland; containing a new theory of the irruptions of the Geyser springs, with many interesting observations on the chemical changes which are observed in course of progress at this focus of volcanic activity. As the subject of this paper is altogether beyond our province, we shall only in conclusion again commend the Cavendish Society to the attention of our readers, many of whom, we trust, may be disposed to lend their aid in the prosecution of its important objects.

PART SECOND.

Bibliographical Notices.

ART. I.—*An Inquiry into the Degree of Certainty in Medicine; and into the Nature and Extent of its Power over Disease.* By ELISHA BARTLETT, M.D., Professor of the Theory and Practice of Medicine in Transylvania University.—*Philadelphia*, 1848. Small 8vo, pp. 84.

THIS essay is designed to counteract, as far as may be, the tendency to scepticism as to the power of Medicine, which seems to have taken a stronger and more extensive hold of the public mind in the United States than in this country. It is an earnest vindication of the ability of the medical practitioner to save life, to diminish suffering, or to prevent disease; not, of course, asserting that his powers extend to every case, but claiming for him some beneficial influence in a large proportion.

We have ourselves an earnest and settled conviction of the benefits which Medicine is capable, even in its present imperfect condition, of exerting upon mankind; and our faith is strong, that, as our Science must be regarded as having only recently taken a definite direction, but as now rapidly tending towards a far higher elevation than it has ever before attained, so our Art will speedily improve in the precision of its rules and the certainty of its results. The most intelligent minds in our body are now quite sufficiently alive to the necessity of taking nothing for granted, of scrutinizing the claims of even the most approved systems, of prosecuting the inquiry into the relations of cause and effect far more closely and deeply than it has yet been carried, and of applying to medical reasoning the severe methods which alone are admissible in other departments of knowledge. And we think it much more becoming our own position to do what in us lies to promote these objects, than to discuss how far our present imperfect methods, especially when applied by half-educated and undiscriminating practitioners, are beneficial to mankind in the mass.

To those, however, who wish to have their faith strengthened in the present powers of Medicine, we may recommend the Essay of Dr. Bartlett; warning them, however, that they must take some of his arguments and assertions with a little of that reservation with which we must generally receive the statements of an advocate. For example, our readers know better, we trust, than to accept the following as a correct description of the usual results of syphilitic infection. "Its poison, when received into the system, and not counteracted by appropriate treatment, extends and multiplies itself, till the entire economy becomes its prey, till every fibre and every fluid of the body is involved in its foul contamination, and this fair and goodly fabric, 'the cunning'st pattern of excelling nature,' is changed to a reeking mass of loathsomeness and abomination." No fact is better established in the natural history of syphilis, than that, in a considerable proportion of cases of infection, there is a tendency to a spontaneous cure under favorable hygienic conditions; and the question is, what method of treatment tends most to increase this proportion, and thereby to render the cure more certain. If, as has sometimes happened, the remedy proves as injurious as the disease, such a result must of course be taken into account on the other side, when an estimate is to be formed of the benefits of medical interference.—So, again, we think that the picture which our author has drawn of the ravages of smallpox, previously to the introduction of vaccination, is somewhat overcharged,—when, for example, he speaks of it as, "where it failed to destroy, leaving behind it its disfiguring traces;" and he does not sufficiently distinguish between the results of the disease itself, and those of the anti-hygienic conditions and injurious medical treatment, which undoubtedly contributed largely in former times to the fatal issue.

In several parts of the essay we notice faults of this kind, which considerably diminish its value as a sober "Inquiry;" and the style is sometimes so exaggerated and hyperbolical, as to border on the ludicrous. The following apology, extracted from the Preface, seems to us only to make matters worse:

> "If the drapery of our philosophic muse wears a somewhat warmer and livelier hue than the sober colouring which appropriately belongs to it, it has been caught from the sunny fields through which her pathway has lain. If the axle of our car has now and then waxed fervid, and if an occasional gleam of light has flashed from its flying wheels, let us at least plead in extenuation of the weakness, that it is not from the careless rein, or any perilous speed with which our coursers have been driven, but from the glowing and radiant atmosphere through which we have been carried along."

And again, after saying,—"I speak for the art and science, not for myself. This art and science have been violently, and I think blindly and unjustly assailed, by parties who understand neither their own strength and position, nor ours: I have endeavoured to make fair and manly stand against them, and I have done nothing more,"—he continues: "When crowds of epauletted and bedizened coxcombs, who have never smelt gunpowder, make the air clamorous with their noisy boastings, the war-worn and scar-covered veteran may, at least, point to the trophies that he has brought from a hundred battle-fields." We should not have expected such specimens of bad taste from the author of the excellent treatise on the 'Fevers of the United States;' and are disposed to inquire whether there be anything in the soil or atmosphere of Transylvania, which specially tends to develope and encourage this kind of style amongst its medical authors, as we recollect to have seen examples of it from the same locality on former occasions.

ART. II.—*Surgical Anatomy.* By JOSEPH MACLISE. Fascic. II. Folio. Four Plates.—*London,* 1849.

THE success of this work, which has, we believe, much surpassed the expectations of its author and publisher, fully confirms the justice of the verdict which we had the pleasure of passing on its first part. And we have much satisfaction in assuring our readers that there is no falling off in the present Fasciculus, either as to the character of the drawings, or the judgment displayed in the dissections from which they have been taken.

We wish we could add that Mr. Maclise had profited by our admonition in regard to the style of his accompanying commentaries, which continues to present faults similar to those on which we formerly animadverted.

The first plate of this Fasciculus gives the Surgical Dissection of the Sterno-clavicular or Tracheal Region; and as there is here an unsymmetrical arrangement of the great arterial trunks, two figures are given, one illustrating the anatomy of the right side, and the other that of the left. This circumstance gives two or three occasions to Mr. Maclise to indulge in his peculiar phraseology. Thus in the first paragraph he tells us that "the general median line of the body is characterised as the point of fusion of the two sides; and all structures or organs which range this common centre are either symmetrically azygos, or symmetrically duplex. The azygos organ presents as a symmetrical unity, and the duplex organ as a symmetrical duality." Again, after telling us that it would be out of place in a work on regional anatomy, to enter upon an extended inquiry into the rationale of the want of symmetry peculiar to the branches of the aortic arch, he "cannot but regard this exceptional fact of a symmetry as in itself expressing a question by no means foreign to the interests of the practical." And when speaking of the occasional origin of the left carotid and subclavian from a common trunk, he says, "On the other hand, I observe that the peculiarities to the normal separate condition of A and B, the carotid and subclavian arteries of fig. 2, display in the relationary aggregate, a phasial gradation of A and B joining into a common trunk union, in which state we then find the aorta giving origin to a right and left innominate artery." We presume that there is some idiosyncrasy in Mr. Maclise's mind which renders this style natural to him, for we should be unwilling to regard it as a piece of affectation; however it be, he would consult the convenience of his readers, as well as his own reputation, by putting his meaning into ordinary English, which, we can assure him, he need not find any difficulty in doing.

The second plate, giving the Surgical Dissection of the Axillary and Brachial Regions is a most excellent delineation; and the commentary is judicious throughout. The third gives comparative views of the Surgical form of the Male and Female Axillæ; the differences between which, depending chiefly upon the superposition of the mammary gland upon the pectoral muscle, and the greater number of lymphatic glands, in the female axilla, are quite worthy of this special notice. The old leaven thus manifests itself in the introduction to the Commentary on this plate.

> "Certain characteristic features mark those differences which are to be found in all characteristic regions of both sexes. Though the male and female bodies, in all their regions, are anatomically homologous or similar at basis, yet the constituent and corresponding organs of each are gently diversified by the plus or minus condition, the more or less, which the development of certain organs exhibits; and this diversity, viewed in the aggregate, constitutes the sexual difference. That diversity which defines the sexual character of beings of the same species is but a link in that extended chain of differential gradation which marks its progress through the whole animal kingdom. The female breast is a plus glandular organ, situated, pendent, in that very position where, in the male body, the unevolved mamma is still rudimentarily manifested."

Surely the very familiar idea, which is here mystified into the aspect of some portentous novelty, needed no such magniloquence for its enunciation.

The fourth plate gives the Surgical Dissection of the bend of the Elbow and of the Fore-arm, in two excellent figures, one showing the superficial and the other the deeper vessels, nerves, &c. As we have quoted from Mr. Maclise's text to his disadvantage, we shall do him the justice to cite the opening paragraph of his commentary on this plate, which will show that he can give happy and forcible expression to a good idea, when he is so minded.

> "The farther the surgical region happens to be removed from the centre of the body, the less likely is it that all accidents and operations which involve such regions will concern the life immediately. The limbs undergo all kinds of mutilation, both by accident and intention, and yet the patient survives; but when the like happens at any region of the trunk of the body, the life will be directly and seriously threatened. It seems, therefore, that in the same degree as the living principle diverges from the body's centre into the outstanding members, in that degree is the light weakened in intensity; and just as, according to physical laws, the ray of light becomes less and less by the square of the distance from the central source, so the vital ray, or vis, loses momentum in the same ratio as it diverges from the common central line to the periphery."

We should not be fairly representing Mr. Maclise if we did not say that his general style of exposition is more akin to the last of these specimens, than it is to the former. In fact we have no fault to find with his purely anatomical descriptions; and it is only when he gets upon his philosophical hobby, that this style and phraseology present the objectionable form of which we have given examples. We do not intend to return to this topic, unless Mr. Maclise should astonish us by attending to our admonitions, in which case we shall think it right to give our readers the benefit of the result. Our hearty good wishes attend this work, which promises to supply, when complete, a far better series of delineations of surgical regions than has been yet given, and at a price as low as that of the most ordinary series of illustrations.

ART. III.—1. *Demonstrations of Anatomy: being a Guide to the Knowledge of the Human Body by Dissection.* By GEORGE VINER ELLIS, Junior Professor of Anatomy in University College, London. Second Edition, rewritten. Part I.—*London,* 1849. Fcap. 8vo, pp. 464.

2. *A Manual of the Dissection of the Human Body.* By LUTHER HOLDEN, F.R.C.S., Demonstrator of Anatomy at St. Bartholomew's Hospital. Part I.—*London,* 1849. Fcap. 8vo, pp. 106.

WE simply announce the fact of the publication of these portions of two Dissecting Manuals; postponing any further account of their contents, for reasons which the readers of our Thirteenth Article will be at no loss to discover, until they shall be completed. The second and concluding part of Mr. Ellis's Demonstrations is promised for next October; and on the strength of this promise, the purchasers of the first part are expected to pay down the price of the whole work. Mr. Holden's work is to appear in four parts, comprising the Dissection of the Arm, that of the Head and Neck, that of the Abdomen and Pelvis, and that of the Leg, respectively. With which of these the dissection of the thoracic viscera is to be incorporated, the author does not tell us. The first part, now before us, includes the dissection of the external anatomy of the front of the chest, and that of the back as connected with the arm.

ART. IV.—*The Animal Kingdom, arranged after its Organization, forming a Natural History of Animals, and an Introduction to Comparative Anatomy.* By the late Baron GEORGES CUVIER. Translated and adapted to the present State of Science. A New Edition, with Additions by W. B. CARPENTER, M.D., F.R.S., and J. O. WESTWOOD, F.L.S. Illustrated by Three Hundred Engravings on Wood, and Thirty-four on Steel.—*London,* 1849. Royal 8vo, pp. 718.

THE 'Règne Animal' of Cuvier must maintain its ground as the only complete systematic treatise on Zoology, until some naturalist, equally competent to the task, shall be bold enough to undertake the production of a similar work, adapted to the present condition of our knowledge. And this, we think, is not likely soon to be the case. For the vast accumulation of materials, which has been brought together since Cuvier reduced all that was then known into a systematic form, is sufficient to alarm the most enterprising, to confound the most self-confident. We seldom now hear of zoologists attempting to grapple with every department of their science. Those, for instance, who aim to acquire a comprehensive acquaintance with the structure and classification of animals as a whole, are fain to leave the subordinate questions of genera and species to be worked out by others, whose cast of mind leads them to feel an especial interest in such matters. But we usually now find zoologists devoting themselves specially to some one division,—whether mammals, birds, reptiles, or fishes, insects or worms, molluscs or zoophytes; and contenting themselves with but a scanty knowledge of the remainder. And it is one evil of this system, that the classifications which such men propose for their own particular groups are founded on principles which do not hold good in other departments of zoology; so that a new 'Règne Animal,' in which each class should be arranged according to the ideas of some naturalist who had made it a special object of study, would be a most heterogeneous farrago. Until, therefore, one of those giant minds arises, which illuminate the world from time to time by their combination of vast solidity of acquirement with the brilliancy of original genius, we expect that the great work of Cuvier will maintain its ground, notwithstanding that in no one department would it now be adopted as a guide by the scientific zoologist.

The translation of this great work prepared some years ago by Messrs. E. Blyth, G. Johnston, J. O. Westwood, and R. Mudie, was comprised in a single octavo volume of by no means inconvenient size; and was enriched with many valuable additions by the translators, who were for the most part well acquainted with the departments they respectively undertook; besides being illustrated with a considerable number of woodcuts. A new edition of this translation being called for, the publisher has still further added to its value, by procuring from Dr. Carpenter and Mr. Westwood such further additions as might give the best account of the recent progress of those departments of zoology, with respect to which the original is now most incomplete. Thus Dr. Carpenter has introduced a view of Professor Agassiz's classification of *Fishes,* as modified by Professor Müller and Owen. In like manner, he has given an account of the arrangement of the *Cephalopoda* proposed by Professor Owen, the correctness of whose principles of classification has been recently confirmed by the dissection of Spirula, an animal possessing a chambered shell, but having the dibranchiate type of organization; and he has also furnished a summary of the researches on the *Tunicata* which have been so successfully prosecuted by Professor Milne Edwards and others. The *Articulated* series, originally treated of by M. Latreille, has had the advantage of the revision of Mr. Westwood; who has contributed a most laborious summary of the recent labours of entomologists, carcinologists, &c., with copious references to their works, which must be of great value to the student. Of the *Radiated* classes, which were left in the greatest confusion by Cuvier, Dr. Carpenter has given a general account; not attempting to enter into the minutiæ of classification, nor even to enumerate the multitude of additions which have been made to their known forms; but confining himself to a comprehensive sketch of the structure of each class, and of the principles on which it ought to be subdivided.

The illustrations of the present edition have also received the addition of numerous woodcuts illustrative of the new matter, and of a number of steel-plates, etched by Thomas Landseer, representing numerous species of mammals and birds with that remarkable felicity for which that artist is scarcely less celebrated as an engraver, than is his brother Edwin as a painter. We cannot doubt, therefore, that this new edition will be very acceptable to every student of zoology.

ART. V.—*An Essay on the Cerebral Affections occurring most commonly in Infancy and Childhood, including Notices of their History, Causes, Diagnosis, Prognosis, and Treatment. Being the Council Prize Essay awarded at the Annual Meeting of the Provincial Medical and Surgical Association, held at Bath, on Wednesday and Thursday, August* 16*th and* 17*th,* 1848. By VALENTINE DUKE, M.D., Fellow of the Royal College of Surgeons in Ireland, &c. &c.—*Dublin,* 1849. 8vo, pp. 90.

DR. DUKE'S essay reached us at too late a period to be included in our review of the 'Diseases of Children' (Art. IX). We regret this the less, since it offers no pretensions for analytical remark, and we must confess does not appear to us quite to attain even that standard of value by which we assume that we have a right to judge works of its own class.

After making all allowances for the usual mode of manufacture adopted in the production of such *forced* articles of literary and professional ingenuity as are prize essays, we are yet of opinion that Dr. Duke has proceeded beyond the mark in availing himself so much of the labours of others, whilst at the same time he offers us so little proceeding from himself. Not that our author is to be charged with the crime of passing off such labours as the results of his own reflections and researches, for he candidly acknowledges the various sources from which he draws; but he should have given more satisfactory proofs of original investigations of his own making. To demand this of Dr. Duke would surely not be imposing a very great tax on his professional experience, considering the scope of the subject upon which he has ventured to touch, and the limitation of his essay on it to ninety pages. We are of opinion also that our author has most unfairly favoured one disease—*tubercular meningitis*—in comparison with the consideration he has bestowed upon other affections. No less than fifty-six pages of the essay are occupied with an account of it, whilst such important disorders as *congestion of the brain, irritation or erethism of the brain, hydrocephaloid disease,* and *convulsions,* are all disposed of in less than ten! It is true that tubercular meningitis in itself quite merits all the consideration that Dr. Duke has given it, and we may say even much more; but not that *proportionate* amount which it is made to bear in a work having such a title-page as that borne by the prize essay of Dr. Duke.

Art. VI.—*An Introduction to Botany.* By John Lindley, ph.d., f.r.s., Professor of Botany in University College, London, &c. With Six Copperplates and numerous Wood-engravings. Fourth Edition, with Corrections and numerous Additions. In Two Volumes.—*London,* 1848. 8vo, pp. 834.

Dr. Lindley's 'Introduction to Botany' had the merit of being the first English work which gave any idea of the results of modern researches in vegetable anatomy and physiology; and notwithstanding numerous imperfections both as to substance and arrangement, it has long maintained a very creditable position, and has been of great service in the training of botanical students. A considerable time, however, has elapsed since the appearance of the preceding edition; and during that period the industry of continental botanists has added largely to the materials from which a treatise like the present ought to be constructed. Comparatively little, we regret to say, has been done in this country; yet some contributions of great importance have been furnished, especially by Berkeley, Thwaites, Henfrey, and Ralfs. Under these circumstances we looked with considerable anxiety at the present edition; for nothing short of an entire reconstruction of the preceding could, in our apprehension, render it an adequate representation of the present aspect of this department of science. Our anxiety was increased by the apprehension that Dr. Lindley's numerous engagements might have prevented him from devoting that amount of time and labour to the personal investigation of the various novelties brought forward by others, which alone could render him a competent judge of their value.

We are sorry to find our fears confirmed by the result. The work has undergone considerable enlargement, but in great part at the expense of materials supplied by the 'Annals of Natural History;' and these materials are simply inserted as extracts in the parts to which they seem most properly to belong, so that a large proportion of the treatise now consists of a mere compilation of selections from a variety of original memoirs, brought together with very little regard to the completeness of the work, and with very little indication of a practical acquaintance with the matters of which these extracts treat. In fact, we must either imagine that the preparation of this edition has been intrusted to some very incompetent hand, or that Dr. Lindley has found himself unable, from want of time, to do more than thus collect a *rudis indigestaque moles,* leaving it to his readers to make a profitable use of the materials with which he furnishes them. Either way, however, he has seriously hazarded the reputation of his work; for although it contains a large amount of valuable information, and is more useful to some readers from its presenting this information in the words of the authors from whom it is derived, yet it cannot be any longer regarded as an appropriate text-book for the botanical student, nor is the collection of recent materials by any means so complete as to suffice for the wants of the scientific inquirer. We must animadvert, too, upon the fact that the old plates are reprinted without alteration, although many of the figures are extremely inaccurate; and that no additional wood-engravings have been introduced, notwithstanding that they are urgently required to illustrate the new matter, much of which is almost unintelligible without them.

We had intended to have taken this opportunity of bringing under the notice of our readers some of those important additions recently made to our knowledge of Vegetable anatomy and physiology, which have the most intimate bearing on our own science; but as we expect to have a more favorable opportunity of doing this ere long, we shall for the present postpone the subject.

ART. VII.—*Practical Pharmacy: the Arrangements, Apparatus, and Manipulations of the Pharmaceutical Shop and Laboratory.* By FRANCIS MÖHR, PH.D., Assessor Pharmaciæ of the Royal Prussian College of Medicine, Coblentz; and THEOPHILUS REDWOOD, Professor of Chemistry and Pharmacy to the Pharmaceutical Society of Great Britain. Illustrated by Four Hundred Engravings on Wood.—*London*, 1849. 8vo, pp. 390.

THE original design of this volume was furnished by Dr. Möhr's 'Manual of Pharmaceutical Technology,' of which Mr. Redwood undertook to edit a translation adapted to the requirements of English Pharmaceutical Chemists. In proceeding with his undertaking, however, he found it necessary to make much more extensive changes, in the way both of addition and alteration, than he had contemplated; and the work at last became at least as much Mr. Redwood's as Dr. Möhr's,—two thirds of the letter-press, and two fifths of the illustrations being the contribution of the former. Under these circumstances we almost think that Mr. Redwood might have advantageously taken the entire work upon himself; as there are many objections to the union of the products of two brains, and to the junction of two names on the title-page, when, as in the present case, the product is a mixture of the materials furnished by the two, and the original author has been allowed no voice in the alterations made by his annotator. We would not be understood, however, as implying that the present treatise is any the worse, either in matter or style, for its double authorship; since no want of continuity, or difference of manner would, we think, be detected by the reader; who is only reminded of the circumstance by meeting with the brackets within which Mr. Redwood's additions are included. It has rather an odd effect, however, to find two thirds of the work thus separated; and as we doubt not that, in future additions, a still larger proportion will be Mr. Redwood's, we would suggest to him the propriety of transferring the brackets to the original matter, as Dr. Elliotson has done with that of Blumenbach in the last edition of his 'Physiology.'

The work is divided into three principal sections:—the first, occupying 24 pages, treats of the General Arrangements most desirable in a

Pharmaceutical Establishment;—the second, extending over 300 pages, embraces the description of the Special Arrangements, Apparatus, and Operations, and is, in fact, a general treatise on Chemical Manipulation, in which "the shop" figures very little;—whilst in the third, on the Dispensing of Medicines, which fills up the remaining 60 pages, the special design again manifests itself. Thus we find that two-thirds of the work consists of matter which is adapted to the use of any one engaged in the study or in the labours of Practical Chemistry; its value not being by any means restricted to the class for which it is especially designed. It occupies, in fact, a very advantageous medium position between the 'Chemical Manipulation' of Faraday, which is too bulky and too minute in its details for the generality of students, and the general and concise directions which constitute all that the learner finds for his guidance in the ordinary treatises on Practical Chemistry.

After a careful examination of this treatise, we have no hesitation in giving it our strongest recommendation, as excellent both in matter and manner. The whole of it is marked by a clear sense of the objects to be gained, by ingenuity in devising means for their accomplishment, and by a facility in explanation which renders every description at once intelligible. The wood-engravings are of first-rate execution, and are so profusely scattered through the volume as to leave nothing to be desired in the way of illustration. As we do not address ourselves to the mere pharmaceutist, we shall not speak of the special adaptation of the work to his wants; but shall simply say that every Medical Practitioner who has anything to do with chemical processes, whether in the study of chemical pathology, or in the preparation and dispensing of medicines, will find in it a number of valuable hints, conducive alike to the saving of labour, and to the more effectual performance of his operations. Nearly the whole of the latter division has been supplied by Mr. Redwood; and it thus possesses the advantage of being in every way adapted to the requirements of the English dispenser, whilst it furnishes the results of a much more extensive practical acquaintance with the subject, than our professional writers on Materia Medica and Pharmacology can be expected to possess.

Art. VIII.—*Lexicon Medicum; or Medical Dictionary.* By the late Robert Hooper, M.D., F.L.S. Eighth Edition, revised, corrected, and improved by Klein Grant, M.D., late Senior Physician to the Royal General Dispensary, and Lecturer on the Practice of Physic at the Aldersgate Street School of Medicine, &c. &c.—*London*, 1848. 8vo, pp. 1392.

Dr. Hooper's 'Medical Dictionary' is so generally known and esteemed among the members of our profession, that it is quite unnecessary for us to give any account of its distinctive features. The editor has shown great industry and skill in carrying out the original design, in such a manner as to embody the results of recent researches in most of the departments of medical science; whilst he has also added greatly to the lexicographical matter, which was originally the most defective portion of the work. We are sorry to be obliged to add, however, that in certain departments in which improvement was particularly needed, there has been no essential change; and that in these the work is consequently very far from exhibiting the present aspect of science. We refer especially to the articles on General Anatomy, Physiology, Animal Chemistry, and General Pathology. Thus under the head of *Texture,* in which we might expect to find some account of recent microscopical and chemical inquiries into the structure and composition of the elementary tissues, reference being continually made to it under their respective designations, we have nothing else than an enumeration of the textures according to Bichat, and a table appended by Dr. Milligan to his translation of Magendie; both of which are now utterly valueless, except as indications of the ignorance of the last generation upon these topics.—Turning to the article *Sanguis,* to which we are referred from *blood,* we find scarcely any information which is not at least fifteen years old. The corpuscles are described, according to Raspail, Prevost and Dumas, and Bauer; whilst of the composition of the fluid in health, nothing is given later than the analysis of Lecanu; and of the changes it undergoes in disease, we scarcely find a word.—So under the article *Tubercle,* Laennec and Carswell are still the great authorities; and not a word is hinted of the real nature of tubercular deposits and of their relations to healthy structure.—So, again, the editor has left *Cancer* at the point at which our knowledge of it had been brought at the time when Dr. Copland wrote upon it in his 'Medical Dictionary,' when as yet the microscope had not been applied to the elucidation of its nature.—The account of *Muscular Fibre* will be a fair sample of the condition in which the editor has left the greater part of the articles on the topics we have named.

> "The most minute fibre we are able to trace seems to be somewhat plaited; these plaits, disappearing when the fibre is put upon the stretch, seem evidently to be the effect of contraction, and have probably induced some writers to assert, that the muscular fibre is twisted or spiral. Various have been the opinions concerning the structure of these fibres, their form, size, position, and the nature of the atoms that compose them. A fibre is essentially composed of *fibrine* and ozmazome, receives a great deal of blood, and, at least, one nervous filament. All other suppositions are founded only on conjecture, and therefore we shall mention only the principal ones, and this with a view rather to gratify the curiosity of the reader, than to afford him information. Borelli supposes them to be so many hollow cylinders, filled with a spongy medullary substance, which he compares to the pith of elder, *spongiosa ad instar sambuci.* These cylinders, he contends, are intersected by circular fibres, which form a chain of very minute bladders. This hypothesis has since been adopted by a great number of writers, with certain variations. Thus, for instance, Bellini supposes the vesicles to be of a rhomboidal shape; whereas Bernouilli contends that they are oval. Cooper went so far as to persuade himself that he had filled these cells with mercury; a mistake, no doubt which arose from its insinuating itself into some of the lymphatics. It is observable, however, that Leuwenhoeck says nothing of any such vesicles. Here, as well as in many other of her works, Nature seems to have drawn a boundary to our inquiries, beyond which no human penetration will probably ever extend."

Such stuff as this is considered by Dr. K. Grant as adequate to "furnish the medical student and practitioner with a concise but accurate survey of the present state of medical knowledge!" We are far from wishing to depreciate the value of the editorial labour which has been bestowed upon the more practical portions of the work; but we feel called upon to remark that even here there are many deficiencies. Thus we do not find any explanation of terms now in such current use among medical authors, as

Colloid and *Stomatitis;* and we look in vain for *Cephalhæmatoma, Atelectasis, Angioleucitis, Scleroma,* and many other words which certainly ought to have a place in a Medical Dictionary.—We would suggest to the publishers that, when a reprint of this work is called for, the editorial labour be subdivided, so that two or more competent heads be applied to its revision. It is too much to expect that one individual, however great his acquirements, can keep up with the progress of every department of medical science and practice.

ART. IX.—1. *Bibliothèque des Médecins Grecs et Latins, publiée avec le concours de Médecins Erudits de la France et d'Etranger.* Par le Docteur CH. DAREMBERG. *Prospectus et Spécimen.—Paris,* 1847. Large 8vo, pp. 70.

2. *Résumé d'un Voyage Médico-Littéraire en Angleterre : Lu à l'Académie des Inscriptions et Belles-Lettres, dans la Séance du six Octobre,* 1848. Par le Docteur CH. DAREMBERG.—8vo, pp. 18.

IN 1844 Dr. Daremberg, librarian at the Ecole de Médecine, who was even then well known to the more learned portion of the medical profession, was sent by the French Minister of Public Instruction to examine the ancient medical MSS. contained in the principal libraries of Belgium and the north of Germany; and of this expedition he gave a short and interesting bibliographical account, which was noticed in the 'Provincial Medical and Surgical Journal' for Nov. 12, 1845. In the autumn of 1847 he was sent for the same purpose to this country, where, by his intelligence and pleasing manners, he gained the esteem of all who became acquainted with him. It appears from the few pages in which he gives an account of his tour, that he visited the libraries in London and Oxford, and also the noble collection of Sir Thomas Phillipps at Middlehill; and that of all he saw in this country, he was most pleased with Oxford, where he was charmed with the beauty of the place, and also with the excellence of the Bodleian Library, and the extreme complaisance of its librarians.

Dr. Daremberg does not notice all the medical MSS. that came before him, but only the most interesting. Among these is a Greek MS. in the Bodleian of the "Viaticum Peregrinantis," a work which he believes to belong originally to the Arabic physician named Ibnu-l-Jezzár; and this statement agrees with the conclusion arrived at by the writer of the article "Synesius" in Smith's 'Dictionary of Greek and Roman Biography,' (which Dr. Daremberg has not had an opportunity of consulting,) where the whole question of the authorship of the work is discussed at some length. But of all the Greek MSS. which he found in England, the most valuable is that of Oribasius in the library of the London Medical Society, transcribed from one still in existence in the library of St. John's College at Cambridge, and containing a text superior to that of any of the MSS. at Paris.

But it was not merely to collect bibliographical information, irrespective of any ulterior object, that Dr. Daremberg was sent on these two expeditions. They were, in fact, connected with an extensive scheme entertained by Louis Philippe's government, of which Dr. Daremberg gives a detailed account in the "Prospectus" mentioned above, but which (like many other undertakings of greater importance,) has been suspended at least, if not positively abandoned, in consequence of the late revolution. This plan we will now lay before our readers as briefly as possible, after first directing their attention to the admirable manner in which these matters are conducted on the Continent. It appears that Dr. Daremberg presents his plan to the Minister of Public Instruction, who, as it relates to ancient medical literature, does not give any decisive answer till he has consulted the Royal Academy of Inscriptions and Belles-Lettres, and the Royal Academy of Medicine. Each of these learned bodies appoints a separate committee to examine the subject, and then upon their recommendation the Minister engages to patronize the undertaking. The pamphlet before us contains Dr. Daremberg's "Projet," the two "Rapports" presented to the Minister, and a specimen of the proposed work. We must content ourselves with stating that Dr. Daremberg proposes (with the co-operation of various medical scholars in different parts of Europe,) to publish a complete and uniform collection of the ancient Greek and Latin medical writers, accompanied with a translation, notes and indices. It is known to many of our readers that Kühn projected and partly executed a similar design, but the prospectus and specimen before us appear to be in every respect far superior to his; and, as it is not proposed that the whole of the vast work should be done by a single individual, (as may be said to have been the case with Kühn,) we may confidently hope, that if, through the aid of the French Government, (for without this it would be hardly possible to undertake so extensive a scheme,) it is allowed to be begun at all, it will be conducted in a creditable and satisfactory manner. The specimen contains a few chapters of the Greek text of Oribasius, with the Latin version underneath, and the various readings, &c. at the bottom of the page; after which follow some excellent Latin annotations relating not so much to the language as to the subject matter of the work. It is one of the most beautiful specimens of typography that we have ever seen, and is altogether so admirably arranged and executed, as to confer the greatest credit on both the editor and the printer. We thought it right to bring the plan before the notice of our readers, as we have reason to believe that it is but little known in this country, though it has been noticed by the public press in Germany. We heartily wish Dr. Daremberg every success, and trust that he will procure a sufficient number of subscribers in different parts of Europe to show that his labours are appreciated by his medical brethren, and also that the new French government (as soon as it is firmly established,) will not be less ready than its predecessor to patronise every deserving literary and scientific work.

ART. X.—*The London and Provincial Medical Directory for* 1849.—12mo, pp. 532.

AGAIN has the scope and size of the above work undergone modification; and in this instance, we think, with advantage in every respect. The Diary, of whose utility we expressed ourselves doubtful in our notice of last year's Directory, has been omitted, and the size and price of the volume thereby materially reduced. On the other hand, a Street List has been added to the London Directory; and in some other departments additional information has been given. We observe that various inaccuracies have been pointed out in this work, some of which have been animadverted on with, we think, unnecessary severity. The editors are fully justified in saying that "those only who have had experience in such a matter can at all conceive the difficulties attending the compilation of a work like the present;" and many of the errors it contains are doubtless owing to the inattention of the parties to which they relate, in not supplying more correct information. At the same time we trust that the editors will take increased pains to prevent their recurrence; and we should suggest, especially that they secure the revision of their "street" and "local" lists by parties acquainted with the professional residents in the respective localities. We must again animadvert upon the omission of the names of a large proportion of the lecturers in the metropolitan schools; those only being mentioned, whose names occur in the general account of the schools or of the hospitals to which they are attached; and we observe that of the Provincial Medical Schools no notice whatever is taken. These omissions appear to us of such importance, that we would strongly urge the editors to supply them in future editions; as we do not know where such information might be more legitimately sought, with a full expectation of its being afforded, than in the pages of a 'Medical Directory.'

ART. XI.—*An Essay on some of the most important Diseases of Women, with a Description of a novel Invention for their Treatment and Relief.* By W. JONES, M.D., Physician to the Free Hospital for Women and Children. Illustrated with Cases and Woodcuts.—*London*, 1848. 12mo, pp. 40.

IT is a great pity that practitioners who have some trifling improvement to suggest in the treatment of disease or the performance of operations, do not avail themselves of the legitimate means of doing so, by forwarding their contributions to the pages of some of the numerous medical periodicals which are open to their reception; instead of building them up into systematic treatises, composed, in every other particular, of threadbare materials; or issuing them in the form of separate pamphlets, which have too much the appearance of being really intended for distribution among the public, with the view of extending the author's sphere of practice. Under this last category the publication before us must be ranked; for what information can the profession be expected to derive from an "Essay on some of the most important Diseases of Women" in forty 12mo pages? We should not, indeed, have noticed the production at all, did it not contain a practical suggestion which we think worthy of being made more extensively known. This consists in the employment of an instrument for the irrigation of the female passages, which the author terms the "syphon douche;" which may be made of gutta percha tubing with box-wood terminations, at a price far below that of a good syringe or caoutchouc-bottle; and will evidently, by supplying a continuous stream of warm or cold water or medicated fluid, for any length of time, exert a more powerful influence on the uterus than ordinary injections can effect. We are inclined to believe that in many cases of irritable and congested uterus, its use will prove extremely beneficial; and we would therefore call the attention of our readers to Dr. Jones's directions for its construction and employment.

ART. XII.—*The Undercliff of the Isle of Wight; its Climate, History, and Natural Productions.* By GEORGE A. MARTIN, M.D. With a Lithographic plate.—*London,* 1849. Post 8vo, pp. 366.

THE portion of "the Island" (as the *natives* usually designate it) which lies between its southern coast and the remarkable range of cliffs that rises at some distance inland, and which has long been known as "the Undercliff," has recently attracted much attention, both with the profession and the public, on account of the remarkable equability of its climate, and its special adaptation to numerous cases of pulmonary disorder; and, as a natural consequence, it has been resorted to by many invalids as a temporary, and by as many more as a permanent residence. In 1838, the then *village* of Ventnor counted a population of about 350 souls, which, at the census of 1841 had advanced to nearly 900. In 1843 it was constituted a *market-town* by Act of Parliament; in 1847, the number of its inhabitants exceeded 2000; and we believe that it has undergone a considerable augmentation since that time,—other villages, also, in the same locality having increased, though in a less proportion.

Dr. Martin's little work is intended to afford to those who may desire it, the opportunity of more fully acquainting themselves with the numerous points of interest which the district presents, than they might otherwise do; and to furnish an accurate account of its climate and medical topography, for the guidance of those who are seeking information as to the choice of a residence for invalids.

The description of the scenery, &c., which is very agreeably written, is followed by an account of the climate, based on observations very carefully conducted by the author for a period of ten years; and the remainder of the volume is occupied by a survey of the Zoology, Botany, and Geology of the district.

We could have wished, however, that this portion of the work had been rendered more complete. The Zoology only includes the vertebrata and insects; no notice being taken of the mollusca, whether terrestrial, fresh-water, or marine; and the zoophytes being also passed by; although both these groups afford peculiar sources of interest both to the collector and to the observer, and are abundantly represented, we believe, in the locality in question. So also, in the chapter on Botany, we do not find any account of the Cryptogamia, with the exception of ferns; the sea-weeds, as well as mosses, lichens, and fungi, being left unnoticed. We do not mention this by way of blame to Dr. Martin, whose professional avocations can leave him but little leisure for pursuits of this kind, and render him chiefly dependent upon the information which his friends may be able to afford him. But we trust that he will endeavour to incite some of them to supply the deficiency, so that his Fauna and Flora may be rendered complete.

The chapters on Climate constitute the most valuable portion of the work; the observations on which they are based having evidently been made with great care, and having been sufficiently prolonged to allow very fair averages to be drawn. We are unwilling to dismiss these with a cursory notice, and shall take an opportunity of returning to the subject in our next Number. Taken as a whole, Dr. Martin's book forms a very complete and well-written description of the district he has undertaken to illustrate; and will no doubt add to the reputation which it has already acquired as a salubrious and agreeable residence.

PART THIRD.

Periscope.

ANATOMY AND PHYSIOLOGY.

An Experimental Inquiry into the Manifestations of Electricity during the organic process of Secretion in living Animals. By H. F. BAXTER, Esq.

THE author designates this paper as "an attempt to apply some of the discoveries of Faraday to Physiology;" his experiments having been made under the guidance of Professor Faraday's definition of the voltaic circuit as "an axis of power having contrary forces, exactly equal in amount, in contrary directions," and being considered by him as tests of the correctness of Professor Faraday's opinions. It is well known that various experimenters have asserted the existence of opposite electric states in secreting organs furnishing acid and alkaline products, as the mucous membrane of the stomach and the liver. Between arterial and venous blood, however, neither Pouillet nor Müller could obtain any manifestation of difference in electrical state. Mr. Baxter's chief object has been to determine whether any such manifestation could be obtained between the secreting surface and the blood flowing from its veins. In order to arrive at correct conclusions, it is necessary to examine carefully into the sources of fallacy resulting from the chemical difference between the solids and fluids in contact with the electrodes; the proposition having been established by Becquerel, that when two heterogeneous fluids are separated from each other by a membrane or other porous diaphragm, that which performs the part of an acid indicates positive electricity, whilst that which acts as an alkali indicates negative electricity. Hence it is quite impossible to say *à priori*, what would be the effect upon the needle when the electrodes are inserted into different parts of a dead animal. "The mere circumstance of placing the electrodes in contact with different portions of the contents of the stomach produces an effect or not upon the needle; the state of the parts as to moisture, and the transudation of the secreted fluids or contents through the walls of the different viscera; the contents of the colon and of the stomach considered as a mass, forming one conducting body; the difference with respect to the extent of surface of the electrodes; each of these circumstances may be readily seen to be influential and unnecessary to particularise. There may be two or three acting points between the electrodes, determining the current in opposite directions, or assisting each other, the action upon the needle being the result of a differential or of a combined current, according to circumstances."

Nevertheless, after making allowance for these sources of fallacy, the experiments of Mr. Baxter have shown a certain constant relation between the electric condition of the mucous membrane of the small intestines, in animals recently killed, and that of the blood flowing from *the veins of the same part.* When one of the pointed electrodes was placed upon the intestinal surface, to the extent of an inch and a half, and the other inserted into the branch of the mesenteric vein proceeding from it, a deflection of the needle to the extent of 4° or 5°, indicating a positive condition of the blood, was produced; and the effect was greater when a plate was used, instead of a point, for the second electrode. No effect was produced when the second electrode was inserted into the artery of the part; nor when it was placed in the vein of the other part, unless the blood of the two veins should become mixed, when a slight effect is observable; nor when it was inserted into the vein of the part, after it had been emptied of its blood. These effects cease soon after the death of the animal, which is not the case with those which proceed from the simply chemical differences between the blood and the secreted product; and it is therefore considered by the author that they may be attributed to those changes which occur during the formation of the secreted product, and which are proper to the organic action of the part.

[We could wish that the author's experiments had been more numerous and decisive, the greater part of his results having been obviously rendered unstable by the sources of fallacy just referred to, and the number of observations upon which his conclusions rest being very small. We trust that he may follow up his researches, under the guidance with which his experience has now furnished him; and that he will be able, ere long, to obtain and publish more satisfactory results upon this interesting subject of inquiry.]

Philosophical Transactions, 1848.

On the Absence of Vomiting in the Horse. By M. FLOURENS.

EVERY one knows that the horse cannot vomit; but, as to the cause of this peculiarity, there has been a difference of opinion. From his experiments and dissections, M. Flourens is led to the conclusion, that the obstacle to the reflux of the contents of the stomach,—which is such as to prevent the exit of a single drop of fluid by the pylorus, when the stomach has been distended with water and subjected to violent pressure,—is to be found partly in the very oblique mode of entrance of the œsophagus, and partly in the peculiar arrangement of the muscular fibres. These are so disposed, that various bands, arising from the coats of the stomach, pass round a portion of the tube, and then return into the muscular wall of the viscus; from which it results that, the more the latter is distended, the greater will be the constriction exercised around the pyloric orifice.—*Annales des Sciences Naturelles*, Sept. 1848.

On the Development and Functions of the Spermatozoa.
By DRS. WAGNER and LEUCKARDT.

THE elaborate inquiries of the authors into the constitution of the seminal fluid of different animals, have led them to some new and valuable results, which tend to modify in several particulars the opinions formerly entertained. The account which they now give of the origin of the spermatozoa is as follows.—1. All spermatozoa originate in 'formative vesicles,' which appear to resemble the secreting cells of glands in being metamorphosed epithelium-cells of the glandular tubuli or follicles. —2. From these formative vesicles, the spermatozoa are produced in one of the three following modes: *a*, by the conversion of the cell-membrane and nucleus of the formative vesicle itself into the spermatozoon, a method in which the change is the least possible, and which is only found among the Nematoid worms; *b*, by the metamorphosis of the nucleus of the formative vesicle into the spermatozoon, a method which is much more common, especially among the lower animals, in many of which (as Chilopoda, Acarina, and Entomostraca) the spermatozoa remain as solid massive corpuscles, resembling the neuclei from which they sprang, instead of having the filiform shape of ordinary spermatozoa; *c*, by the endogenous development of cells originating in the nucleus of the parent cavity, each young cell producing a spermatozoon within it. This last method is that which we find in all the higher animals; but its latter part may take place in two ways. The parent-vesicle may burst and set free the young cells, before the latter have begun to form the spermatozoa, which then evidently issue from them. But it frequently happens that the development of the spermatozoa takes place, whilst the cells within which they are formed are yet within the parent vesicle; and the walls of these cells give way, so that the spermatozoa come to be associated together in bundles within the parent-cells, as formerly described by Wagner, and are finally set free by their rupture.

The authors have come to the conclusion that the spermatozoa are the essential constituents of the semen; having met with cases in which the liquor seminis is altogether absent; and being also greatly influenced by the apparent impossibility of the fertilization of ova by liquor seminis, when there is no copulation, and the semen is diffused through water, as in most fishes.—*Cyclop. of Anat. and Phys.*, Part xxxiv.

ORGANIC CHEMISTRY.

On the Estimation of Urea.

The physiological and pathological value which is attached to the existence of urea in the various fluids of the body, renders its easy estimation a matter of considerable importance, for hitherto the methods which have been employed for its quantitative determination have been both uncertain and tedious. Millon, however, has recently directed attention to a process which he considers is free from both of these objections. It consists in the conversion of urea into carbonic acid and azote by means of an oxydizing agent (nitrate of mercury), and the subsequent estimation of the urea by the amount of the gaseous products. The nitrate of mercury which Millon uses is made by acting upon 125 parts of mercury with 168 of commercial nitric acid, and then, when the action is complete, he adds twice its bulk of water. In this condition the solution is kept, and it is always ready for analysis; to effect which, he takes a glass balloon of a capacity of from 150 to 200 cubic centimetres, and introduces into it, by means of a pipette, from 40 to 45 cubic centimetres of the mercurial solution. On this he pours from 15 to 20 grammes of urine, and washes down that which soils the neck of the balloon with a little distilled water; then he shakes the balloon so as to mix the contents, and immediately adapts a carbonic-acid apparatus containing caustic potash. The reaction of the nitrate of mercury commences even in the cold, but it is assisted and rendered complete by raising the mixture to the boiling point. The weight of the carbonic acid produced is to that of the urea, as 1 is to 1·371. M. Millon states that the process is most constant, and that there are no variations in the results, notwithstanding that uric acid, hippuric, oxalic, acetic, lactic, or butyric acids are present, and that neither albumen, sugar, or colouring matter of the bile will have any influence on the products; proceeding in this way, he has noticed that there is a very important relation between the density of healthy urine and the amount of urea which is present, and that, in fact, the second and third figures from the decimal point express very nearly the quantity of urea which is contained in 1000 parts of healthy urine; for example, a urine having a sp. gr. of 1·011, would have 11·39 of urea in it, and one of a density of 1·026 would contain 25·8 grs. of urea, and so on. This relation, however, does not hold good in disease; for he has found that the urine of patients who were affected with the following diseases presented a great variation in the density and proportion of urea.

	Sp. Gr.	Urea in 100.
With pneumonia of the right lung . . .	1·015	39·75
„ „ . . .	1·025	45·94
„ double pneumonia	1·017	42·90
„ phthisis	1·043	24·25
„ articular rheumatism	1·028	43·11
„ diabetes	1·037	8·25
„ „ accompanied with fever .	1·039	21·50
„ „	1·035	5·51

Comptes Rendus, xxvi, p. 119.

Another process for the estimation of urea has been proposed by Professor

Bunsen. It is founded on the fact that urea is converted into carbonate of ammonia when it is heated in closed tubes to a temperature above 212° F. He takes from 1½ to 2 oz. of urine, and precipitates the uric acid which is present, by means of a concentrated solution of chloride of barium, to which ammonia has been added; this is then filtered to separate the urate of baryta, and about ½ an ounce of the solution is introduced into a strong glass tube containing 50 grs. of chemically pure chloride of barium; the tube is then hermetically sealed, and heated for 3 or 4 hours in an oil bath, the temperature of which is from 428° to 464° F.; by this means the urea is decomposed into carbonate of ammonia, the acid of which unites with the baryta, and forms carbonate of baryta. On allowing the tube to cool, the carbonate of baryta is easily removed and weighed; and every 3·3 grs. of it indicates that 1 of urea was present.

Professor Bunsen says that neither hippuric nor benzoic acids will affect the result, nor will the extractive of the urine or decomposing animal matter. He has observed, however, that uric acid undergoes a similar decomposition, and hence the necessity for its removal from the urine before proceeding with the analysis.—*Annal. der Chem. und Pharm.*, March, 1848.

Analysis of Human Milk.

DR. GRIFFITHS has examined the proportions of the proximate, as well as the ultimate elements of human milk, and with the following results. A milk taken from a woman fourteen days after her parturition, her diet including meat and beer, yielded 25·36 butter, 61·76 sugar and extractive, and 12·68 of caseine. The milk of a poor woman, who was living entirely on vegetables, was examined one month after her parturition: it had a specific gravity of 1030, and contained 34·32 of butter, 52·41 sugar, &c., and 13·27 caseine. The milk of a woman who had been confined one month and six days, and whose diet was a mixed one, had a specific gravity of 1028, and contained 16·90 of butter, 76·60 of sugar, &c., and 6·42 of caseine. The ultimate analyses indicate that the milk is richer in carbon during the earlier periods of lactation; while the amount of nitrogen does not undergo much variation. This, as Dr. Griffiths observes, is not in accordance with the results of Dr. Simon, who found that the caseine (and therefore the nitrogen) was increased during the later periods.—*Chem. Gaz.*, 1848, p. 192.

On the Origin of Sugar in the Animal Economy. By Dr. CHARLES BERNARD.

IT has been generally maintained that sugar exists in the blood of animals, only when they have previously eaten substances containing it, or capable of being converted into it—the economy having, in fact, been most explicitly denied the power of making sugar, its sole province being that of destroying it and causing its disappearance. It is for the purpose of controverting this doctrine, that Dr. Bernard publishes the results of several experiments he has performed. As sugar is found in the blood after the digestion of amylaceous and saccharine substances, it has been hastily assumed that its existence is due to the use of these descriptions of aliment. This is only true as far as it goes; as the author's first series of experiments prove, *that sugar is found in the blood in all alimentary regimens, and even after long abstinence.* But where is the source of sugar in animals fed on meat alone, or submitted to abstinence? The next series of experiments indicate this to be *the liver.* It then becomes mixed with the blood, traversing that organ, and is carried by the supra-hepatic veins and vena cava inferior to the right side of the heart, where it is constantly found. If a portion of this organ, in a normal state, be bruised, and boiled with a little water, the filtered decoction will be found to possess all the characters of a saccharine solution, especially that of producing carbonic acid and alcohol during fermentation. We may concentrate this fluid, but we cannot crystallize the sugar, owing to the presence of fatty and albuminous matters with large proportions of salts. An examination of sugar produced from the blood or liver, proves that it is neither cane-sugar nor sugar of milk. It presents all the chemical characters of grape-sugar or *glucose*—the sugar of diabetes—though differing from it in certain physiological characters, to be detailed in a future paper.

But how is the sugar thus found definitively produced? Does it result directly from a particular transformation of certain elements of the liver, or is it derived from external alimentary substances, only deposited or accumulated in this organ? As it might be said that the animals fed on meat or kept fasting had, prior to the experiments, partaken of food which supplied the sugar in question, examination of the blood and liver was made in those which had been so kept for ten days, and yet the sugar was found; while, on the other hand, this was quite absent in other animals that had been fed on saccharine articles of food, and in which the pneumogastric nerves on both sides were divided. If these last experiments are confirmed, they will show that, though the formation of sugar by the liver is essentially a chemical phenomenon, it is directly influenced by the nervous system [or, more probably, indirectly, by the diminution of the respiration, produced by this operation]. In confirmation of the independence of the formation of sugar by the liver, Dr. Bernard states he finds that it exists in large proportions in the fourth or fifth intra-uterine month of young calves, gradually increasing until the time of birth.

From what has been said, it must not be concluded that, in taking any liver from a dissecting-room, we are to find sugar in it. In several diseases it disappears, as it does from both the urine and liver in the last stage of diabetes. The livers of eighteen persons dying of different diseases, have afforded various proportions, and some none at all; but the results are as yet too few to enable us to speak positively. In animals which have been submitted to long abstinence, the sugar is diminished or disappears. The livers of all animals killed in good condition should contain it, and the author has always found it in those he has purchased at tripe-shops. The quantity varies in different animals; and Dr. Bernard's observations allow him to affirm, that in birds (fowls and pigeons) the proportion is very considerable, as it is also in mammalia (dog, rabbit, pig, ox, horse, &c.) In reptiles (frog and lizard) it is very slight, while in fish (ray and eel) no traces are found. Does this absence of sugar in cold-blooded animals depend upon the inferior energy of their respiratory functions, which, the author will be afterwards enabled to show, bears an intimate relation to its formation in other animals?—*Archives Gén.*, tom. xviii, pp. 303-19.

M. Martin-Solon, in a letter addressed to Dr. Bernard, in reference to the above communication, which he characterises as a very important one, observes that when ill health seized him some time since, he was engaged in a train of investigation concerning diabetes, which led him to conclusions somewhat analogous to those of M. Bernard. He thinks that the complex operations attributed to Nature in the prevalent theories of *glucosuria*, are so contrary to her habitudes, that we must, *à priori*, on that account doubt their validity. His observations have led him to conclude that the liver is more or less affected in this disease, as indicated by the disturbed digestion accompanying constipation, and the dry, altered colour and consistence of the fæces, which so much resemble potter's clay. The return of the normal secretion of bile is of favorable augury; and medicines exerting a special action upon the liver, as calomel, aloes, rhubarb, &c., seem of utility.—*L'Union Médicale*, 1848, No. 141.

On the Pancreatic Fluid. By Dr. CHARLES BERNARD.

DR. BERNARD after describing the mode in which he obtains the pancreatic juice in the living animal, states as the general result of his thirty-four experiments, that when it is desired to obtain the largest portion of this fluid, the experiment must be performed at the commencement of digestion, and conducted with such celerity as to leave the pancreas exposed for as short a time as possible.

In this way the secretion of the juice does not become suspended; the quantity obtainable never, however, exceeding two grammes per hour in a large dog. But a very important circumstance, which has never hitherto been remarked, is, that the secretion of the pancreas becomes considerably increased under the influence of the inflammation consequent on the operation, which sometimes is set up shortly, and in other cases not for a day or two. This increased secretion has, however, become deprived of the physiological properties of the normal fluid; so that under different circumstances the experiment may furnish us with a normal or a morbid juice.

The *normal* fluid is colourless, limpid, and viscous, flowing slowly out in large pearly or syrupy drops, and frothing-up on agitation. It possesses no peculiar odour, and imparts to the tongue a saline taste like that of the serum of the blood. It is invariably alkaline. Exposed to heat, it coagulates into a very white, concrete matter, as completely as does the white of an egg—not a drop of free liquid remaining. It is also precipitated by nitric acid, sulphuric acid, and concentrated muriatic acid. Alkalies redissolve the coagulated matter. We may allow, with Magendie, Tiedemann, and Gmelin, that this fluid possesses the characters of albumen, but in a physiological point of view there is no relation between it and albuminous fluids; and indeed chemical distinctions between the two exist. Thus, when the organic matter of the pancreatic juice has been coagulated by alcohol and dried, it may be easily and entirely redissolved in water, to which it imparts the viscosity and physiological properties of the pancreatic juice.

The *morbid* pancreatic juice is generally colourless, sometimes opalescent, and at others reddish, possesses no viscosity, and produces a nauseous saline taste. It is less dense, and coagulates neither by heat or acids. The transformation of the normal into the morbid juice is so gradual, that various intermediate shades exist, as indicated by the quantity of coagulable matter, upon the amount of which the activity of the juice depends.

No fluid of the economy is so liable to alteration. Exposed to a low temperature (42° to 52°) it may be preserved for several days, increasing in viscosity, and acquiring the condition of a light jelly. Exposed to from 104° to 112° for some hours, its properties, with the exception of the alkaline reaction, become entirely altered. During the heats of summer, or in stormy weather, such changes may be effected in a few moments. The deposition which takes place furnishes to the microscope a peculiar silky aspect, and a large quantity of needle-shaped crystals, resembling those of margarine or margaric acid. These changes in the composition of the fluid explain the discrepancies of authors in respect to its quantity of albumen.

The special action of the pancreatic juice on neutral fatty bodies out of the body.—Exposure of fatty bodies to the juice, at once produces a complete emulsion, and all fatty substances are resolved into glycerine and fatty acid, and in the case of butter, butyric acid. Parallel experiments instituted with other fluids, as bile, saliva, gastric juice, serum of the blood, produce no such effects on fatty bodies. These experiments have been frequently verified in the presence of distinguished men, as Magendie, Berard, Andral, &c. If a *morbid* or *altered* juice be employed, it exerts no action, soon separating from the fatty matter without modifying it. Partial changes are attended with imperfect results.

The pancreatic fluid is indispensable for the absorption of fatty matters, and the formation of chyle. The chyliferous vessels are only found to contain a white, milky, homogeneous fluid, on condition of their having absorbed fatty matters in the intestine; so that a limpid, transparent chyle (improperly called vegetable chyle) is a chyle without fatty matters; and a white, homogeneous chyle (improperly called animal) is a chyle containing fatty matters, emulsioned and modified. That this is effected by the agency of the pancreatic juice, may be demonstrated. If dogs are killed during full digestion, oil will be found unaltered, until it comes in contact with this fluid; and if the pancreatic ducts be tied, all alteration is prevented. In the *rabbit* the pancreatic duct opens into the intestine very low down, 35 centimetres below the choledochus; and if fatty matters be introduced into its stomach, and the animal be killed in three or four hours, they will be found to become emulsioned only at that low distance down, and only after then is it that the white absorbents are seen.

If it be asked how it happens that a fact now so easy to demonstrate, that it is the pancreatic fluid and not the bile which emulsions fatty matters, has been overlooked, the author is disposed to attribute it in a great measure to the prevalent belief in the salivary character of the pancreatic fluid having misled inquirers. Brodie's experiments on *cats*, showing that the chyle was rendered limpid by ligature of the choledochus, were not verified by those of Magendie on *dogs*, who found the fluid milky and homogeneous. But in the *cat* the pancreatic duct anastomoses with the choledochus before opening into the intestine, so that Brodie, unaware of the importance of the pancreatic juice, might easily have tied both vessels; while in the *dog* the choledochus is completely isolated from the two pancreatic ducts of that animal.—*Archives Générales*, tom. xix, pp. 60-81.

M. Dumas, in a Report upon another Memoir by M. Bernard, read at the Académie des Sciences, Feb. 26th, 1849, makes the following observations.

"It is not long since, that alimentary bodies have been classed into four groups, founded upon their chemical characters, viz.: matters soluble of themselves, and therefore susceptible of direct absorption by the veins of the alimentary canal; amylaceous matters fit for conversion into sugar; fibrinous matters requiring a special ferment to be rendered soluble; and fatty matters evidently destined to pass into the chyle, and impart to it its most obvious characters. Recent researches have placed beyond doubt the existence of a ferment fitted to saccharify fecula, in certain of the fluids which are mingled with the alimentary bolus. They have proved, too, that the especial object of the gastric juice is the digestion of nitrogenous matters, which it renders soluble. There remained yet to be discovered, the true agent of the digestion of fatty bodies, i. e. the agent of the formation of the chyle, properly so called. M. Bernard now brings forward his proofs that this remarkable office is due to the pancreatic juice.

"The commission (MM. Magendie, Milne Edwards, and Dumas), without dwelling upon the chemical composition of the pancreatic fluid, which will be the object of future researches, does not hesitate to conclude that the author has completely established its physiological office, that he has completed the general characters of the theory of digestion, and has made known the mechanism of the formation of the fatty substance of the chyle, and that of the digestion of fatty matters."—*Gazette Médicale*, 1849, No. 9.

PATHOLOGY.

On Ozone and Influenza. By Dr. SPENGLER.

BELIEVING Schonbein's recent observations to be of a highly important character, Dr. Spengler calls upon the practitioners in various parts of Europe to put them to the test; and offers, as a small contribution, an account of what he himself has observed at Roggendorf.

At the end of the year 1846, many slight catarrhal affections prevailed, but no ozone whatever was detectible. With the new year, catarrhal affections of the bronchial and Schneiderian membranes and of the fauces became exceedingly frequent. Almost every one was coughing. Influenza, in fact, prevailed over the entire vicinity, and the reagents showed abundance of ozone to be present. Gradually as the disease diminished, so did the indications of the presence of this substance. Strips of paper, smeared with a mixture of iodide of potassium and starch-paste, formed a most delicate ozonometer. At first the paper assumed a slightly yellow colour, then a darker one, and at last a dark brown.—*Henle und Pfeufer's Zeitschrift*, vol. vii, p. 70.

Multiple Fibrous Tumours.

M. **LEBERT** recently communicated to the Biological Society an example of an extremely rare occurrence—the dissemination of a vast number of fibrous tumours in the subcutaneous cellular tissue. The patient, æt. 66, is in good health. One of the tumours which has been removed from the thigh (and the microscopic examination of which leaves no doubt as to its fibrous nature) dates as far back as thirty years; but it has been only in the course of the last twelve years that the others have multiplied by hundreds in the cellular tissue of the head, neck, trunk, and limbs. The skin covering the tumours is not reddened, and over some of them it is in too relaxed a condition. Neither pain or any uneasy sensation has accompanied their production.—*Gazette Médicale*, 1849, No. 6.

On the Influence of Physical Agents in Variola. By M. SERRES.

THE skin is pre-eminently the seat of the variolous eruption, not so much on account of its structure, as its external position, and consequent exposure to the action of the air. Thus those portions of the skin which, by the hair covering them, are somewhat protected from this influence, are those least affected by pustules. And if portions of the internal organs, naturally protected from the influence of the air, become exposed to this, no matter what their structure or functions may be, they also become covered with the pustules. Thus in the same child we may see the pharynx, epiglottis, and sometimes the trachea, exhibiting pustules, while the œsophagus is quite free. In trichiasis, in protrusion of the rectum, in protrusion of the vagina or uterus, the internal surface, now placed in the same conditions as the external, furnishes pustules like it. It is to the exclusion of the air that the ectrotic mode of treating smallpox owes its success.

Seeing that the atmosphere produces so manifest an effect on the development of the pustules, we should expect that changes in its condition would not be without their influence. And thus it is, when we examine into the causes of the mortality of the disease prior to vaccination, we find that *inordinate dryness* of the air was one of the conditions most favorable to its aggravation, and this whether it coexisted with an excess of cold or heat. So, too, the history of the epidemics of this disease shows that they have always proved more fatal in the dry south, than in the more humid north. During 1817-19, M. Serres' smallpox patients were placed in small, ill-ventilated wards, which were also damp; but the cases of confluent smallpox manifested little severity. When, however, these wards were abandoned for spacious ones situated on the fourth story, very dry, and looking north and south, being very hot in summer, and very cold in winter, the mortality became so much greater, as to lead to the speedy resumption of the old, damp wards on the ground floor.

Does the same observation apply to vaccinia; and is this why revaccination has been so much more successful in the north than in the south?—*Gazette Médicale*, 1848, No. 41.

On the Analogy and Differences between Tubercle and Scrofula. By M. LEGRAND.

M. **LEGRAND** terminates an elaborate and valuable series of papers, abounding in illustrative cases, the fruit, he states, of several years' research, with the following positions:

1. There undoubtedly exist analogies, which we may call symptomatic, between tubercle and scrofula, that is to say, one of these two morbid principles; the tubercular may exhibit itself by symptoms which appear to belong to the other. It is not the case with regard to the latter. 2. Tubercle possesses, so to speak, its morbid individuality, its molecular element—the tubercular globule—which is often met with in the scrofulous manifestations of tubercle. 3. Scrofula is always deficient in the morbid molecular element, and its existence is only proved by the constancy of the effects which are attributed to it. 4. The chief, or even the only seat of tubercle is in the

internal organs, and the external manifestations of the morbid principle irradiate from the centre to the circumference. 5. Scrofula comports itself quite otherwise, and manifesting itself on the skin or periosteum, irradiates thence towards the internal organs, which, however, it never disorganizes in the same manner as tubercle. 6. Tubercle, in spite of the impoverishment of the blood it always induces, does not destroy, at least in the early periods, the inflammatory element, the fibrine, which well explains the occurrence of the phlegmasiæ, which so often complicate it, and which always hasten its disorganizing progress. 7. Scrofula likewise impoverishes the blood, but at the same time it seems to annihilate the inflammatory element. Thus inflammations rarely complicate it, and when such complication does exist, it often favours the cure of the disease. 8. All the changes observed in the blood and urine of tuberculous and scrofulous patients, are evidently consecutive; and they cannot be considered as the cause of these two diseases, whose principle is nevertheless very probably contained in the blood. 9. Tubercle is never curable, or at least such cure constitutes a rare exception, while scrofula is almost always curable.—*Revue Médicale*, 1848, tom. iii, p. 367.

On the Absorption of Virus. By M. Renault.

M. **Renault** has recently communicated to the Academy of Sciences the results of certain experiments made at the Veterinary School at Alfort, the object of which was the determination of the period at which the *action of a virus ceases to be local, and becomes general.*

This was endeavoured to be determined by ascertaining how soon a parcel of virus placed under the epidermis becomes absorbed; or, in other words, how long is the period after inoculation within which we may destroy or remove the portion of skin under which such deposition has been made, without modifying the absorption of the virus, so as to prevent or sensibly modify the general effects. In thirteen experiments, the animals were inoculated with the matter of acute glanders, and the actual cautery applied, after previous excision of the congested parts, at periods after the inoculation varying in the different animals from 96 hours to 1 hour. In all the animals became diseased. In another series of experiments in which the virus of the rot of sheep was employed, it resulted that the virus was absorbed in five minutes.—*Gazette Médicale*, 1848, No. 51.

On the Simultaneous Development of Variola and Vaccinia.
By MM. Herard and Bousquet.

M. **Herard** having had at the Children's Hospital of Paris (where children are so wantonly exposed to the danger of contracting smallpox prior to their vaccination) the opportunity of observing 18 cases of the coexistence of the two diseases, he is desirous of expressing his opinion upon the disputed question as to the degree of influence they exert upon each other.

Of these 18 children (from 20 months to 4 years old) 7 died; all these being, however, cachectic or exhausted by prior disease. In them the smallpox was rarely confluent, but it was irregular and ill developed. In the 11 cases which recovered, the eruption was discrete, rapid in its course, and unaccompanied by suppuration of the pustules, or tumefaction of the surrounding skin. The fever was slight, and the convalescence rapid. During this benign progress of these cases, the disease more than once proved very fatal to non-vaccinated children. Had the vaccine acted injuriously in the seven children who died? MM. Rilliet, Barthez, and Legendre answer this question in the affirmative, believing the vaccination hastens the evolution of the variola, and increases the debility—opinions with which M. Herard does not agree. He also, in the 11 successful cases, observed no difference produced in the mode of the development of the vaccine vesicle; but in the seven children who died from various complications, the vaccine was in a very languishing condition. The following are the general conclusions arrived at: 1. When the two eruptions are developed at the same time in a healthy child, the variola is advantageously modified. Its progress is more rapid, and the eruption is more discrete—taking on the character of varioloid. 2. Although vaccination is far from exerting the same effect, and especially in hospitals, on very young and diseased children, being innocent, it should be practised. The danger in these cases is attributable to the smallpox, not to the vaccination, and is produced by complications and bad hygienic conditions. 3. It is not correct to say that the two eruptions exert a reciprocal modification. That which has the priority of invasion (not of infection) influences the other, but is not influenced by it; and as, in almost all cases, the vaccine, to become developed, must precede the variola, we may generally state that in the cases where the diseases exist simultaneously, the vaccinia undergoes no modification.—*L'Union Médicale*, Nos. 108, 109, 110.

M. Bousquet states that an attentive examination of the voluminous correspondence of the Académie reveals the greatest discrepancy of opinion upon this subject. For his own part, he denies that vaccinia modifies the progress of smallpox advantageously; and considers it a false deduction to suppose that because vaccinia prevents variola, it must retain its advantages when the two diseases meet together. According to MM. Rayer and Clerault's statistics of 111 cases of simultaneous existence of the two diseases, 12 died, and M. Legendre states that 9 of 56 died: but smallpox, unaided by vaccination, is not more fatal than this. Of M. Herard's 18 cases, 7 died, and there is no reason to agree with him that the remaining 11 were milder because of the vaccinia. MM. Rilliet and Barthez declare that in very young infants it increases danger; but M. Bousquet agrees with neither party. Those who maintain that smallpox is rendered more discrete by vaccination; take only the successful cases, forgetting that in the natural smallpox the discrete exceed the confluent cases in the proportion of 10 to 1.

M. Bousquet therefore denies that the two eruptions exert any reciprocal reaction, and the nearer they appear together, the more independent are they of each other. Suppose, e. g., that the two could appear at the same hour, then each would pursue its ordinary course, just as if the other were not present. But supposing the one eruption appears after the other, all will depend upon the space of time separating them. If this be only some hours, or even two or three days, all passes on as just stated. The case is different when one of these eruptions is greatly in advance of the other. If this is not to such an extent as to exclude, the eruptions progress together, but not in parallel. The most advanced always keeps its advantage, and finishes at its ordinary epoch, without having undergone any change in form or duration. The other follows it at a distance, but after the variolous capacity of the subject has become exhausted by the first, the second dies away. In these influences there is nothing direct, active, or special, they are the consequences of the faculty possessed by the eruptions of supplying or substituting each other. The vaccinia does not arrest the variola, but it is the variola that stops short in face of the vaccinia; and, conversely, variola does not cut short the course of vaccinia, but this last interrupts its own course in presence of the variola. It is the right of precedence, and the more widely the two eruptions are separated, the more readily do they exclude each other, while the nearer they are together, the more independently do they proceed. Considered in themselves, the vaccine and variolous virus are so little capable of destroying each other's energy, that if they are mixed together, and inoculation performed with the mixture, two perfectly distinct eruptions are produced. Considered as regards their effects, we cannot say that vaccinia *cures* variola, or even, rigorously speaking, that it *prevents* it. It takes its place, stands in its stead, and is neither more nor less than a substitution. Thus so far from explaining the operation of vaccinia by the supposed opposition it offers to variola, we would rather do so by the analogy and reciprocal action of the two diseases.—*Bulletin de Thérapeutique*, tom. xxxv, pp. 342-52.

PRACTICAL MEDICINE.

On Paracentesis in Acute Pleurisy. By M. TROUSSEAU.

M. TROUSSEAU has already published seven cases in which he has resorted to this operation, and more recently he has communicated two others to one of the Parisian Medical Societies. His cases belong to a variety of the disease which he thinks has not sufficiently excited the attention of pathologists. In these there would seem to be a moderate amount of inflammatory action, as indicated by fever, pain, or cough; and yet, in a very few days, an enormous amount of effusion distends the pleura, and menaces life itself by the compression of important organs it induces. Of nine such cases, three, in which no puncture was performed, died, the six operated upon recovering. These six patients could not, however, be regarded as being in a state of immediately imminent danger; and M. Trousseau does not regard the operation as a last resource, but believes we should resort to it at a sufficiently early period to prevent fatal accidents which possibly may occur; and refers to several fatal cases, occurring in the practice of himself or others, in which had the great effusion present been removed, the patients might have recovered. He employs a somewhat shorter trocar than is generally in use, having a stop to regulate the extent to which the blade is to be admitted, and provided with a valve of moistened leather, which prevents the admission of air after the withdrawal of the fluid. Before puncturing the parietes also, he makes a small crescent-shaped incision into the integuments, and passes the trocar beneath its upper lip, so that, on withdrawing the canula, the skin entirely covers the aperture. The puncture gives little pain, the lungs and heart resume their proper position, and free respiration is restored.—*L'Union Méd.*, 1848, No. 153.

On Asthma occurring in a Child. By Dr. TOTT.

A CHILD, æt. 1½ year, had been seized daily, at mid-day, for three months, with asthmatic paroxysms of difficulty of breathing, terminating in cough without expectoration. An intermittent fever prevailing in the vicinity, the author supposed that the disease might partake of its nature, and administered quinine with hyoscyamus, &c., as well as external derivatives; but without any avail. The *tinct. lobel. infl.* (15 drops every 3 hours) was then given in weak tea, and by the third day the paroxysms had become much diminished in intensity and duration, and in fourteen days no traces of the affection remained, the child continuing quite well when seen a year and a half afterwards.—*Neue Zeitschrift für Geburtskunde*, Band xxv, p. 197.

On Ciliary Neuralgia. By M. TAVIGNOT.

M. TAVIGNOT prefers the term neuralgia of the ciliary nerves as less vague than that of ocular neuralgia, under which the symptoms are usually described. It may occur in either an acute or chronic form, in a continuous or an intermittent one. In the *chronic* form important changes may take place in the tissues of the eyeball—changes which the author believes to be the consequence of the neuralgia. This precedes the organic disorders, and the characters of the accompanying pain are quite different from those of ophthalmia. Thus, too, he regards it as an error to consider the oculo-circumorbitar pain in glaucoma as *symptomatic.* In explanation of the possibility of the production of glaucoma from this cause, he refers to the experiments and pathological facts which have exhibited the influence of the fifth pair upon the functions and nutrition of the eye. Taking this view of the case, he has made several attempts at treating glaucomatous affections as essentially nervous diseases, and with considerable success, the particulars of which he intends shortly publishing.—*L'Union Médicale*, 1848, No. 135.

On the Treatment of the Asphyxia of Confluent Variola. By Dr. HERPIN.

DR. HERPIN observes that in most cases of smallpox which have produced death, independently of complications, no other changes are found than traces of the eruption at the upper portion of the respiratory passages. So persons who have watched these cases are aware, that after having manifested a raucous voice and cough, and then aphonia, they die with all the signs of suffocation. These remarks, made by several observers, have not yet sufficiently excited attention as to the means of prevention of the consequence by the removal of the cause. The indication is very similar to that furnished by croup; the eruption in this case, however, having less tendency to affect the divisions of the bronchial tree than have the pseudo-membranes of croup; nor do the softened *débris* of the epithelium present the same tenacity or adherence, or the same tendency to reproduction.

The affection, indeed, should be combated before it reaches the extreme point, when it renders swallowing difficult or impossible; and the author refers to a case in which he successfully resorted to cauterization by the nitrate of silver. In other cases, exciting vomiting by the tartar emetic forms an excellent method; and he suggests that Kermes mineral, or sulphuret of potass, might be useful in the less urgent cases.

An interesting case was the immediate cause of the publication of these remarks. A man, æt. 28, having complete vaccine scars on his arm, took the smallpox in its most virulent form, it becoming very confluent. By the eighth day, when the suppuration had attained its maximum, the breathing became excessively embarrassed (though the respiratory organs exhibited nothing anormal), and deglutition very difficult. The patient lay exceedingly prostrated and somnolent. Tartar emetic was got down with difficulty, but soon produced cough and expectoration, and then vomiting, followed by amendment, so that a few doses removed all impending danger. Desquamation did not occur until the sixteenth day.—*Gazette Médicale*, 1848, No. 49.

On Polydipsia. By M. VIGLA.

M. VIGLA took the occasion of an example of this rare form of disease being in the Hôtel-Dieu to deliver a clinical lecture upon it. It occurred in the person of a shoemaker, æt. 40, who two months before admission suffered from severe frontal neuralgia. Shortly before he came in he was seized with so tormenting a thirst, that he was forced to drink six or seven quarts of water a day, and two or three by night. This state continued for three weeks, during which the neuralgia entirely left him; but a week prior to admission the thirst diminished and the neuralgia returned. Blisters to the head and purgatives relieved this; but the thirst now returned as intensely as ever. On the 2d of November he was found to have passed from eight to ten pints of urine since the prior evening, which was of a very pale citron hue, inodorous, nearly neutral, and of a density (1002) but little above that of water, &c. The tongue was nearly normal, the gums pale, mouth dry, and spitting difficult, saliva slightly acid, and so sparing that he could not swallow two mouthfuls without drinking; appetite gone, having some desire for vegetable food, and a loathing for animal. No pain in the abdomen, and stools are normal. Skin dry, and very susceptible to cold. Some emaciation; little sleep; suspension of sexual desires; pulse 56, and regular.

There are three diseases in which excessive thirst and secretion of urine are prominent symptoms; polydipsia, diabetes mellitus, and diabetes insipidus. *Polydipsia* is distinguished from *diabetes mellitus* by there being no sugar in the urine, and mere congestion or augmentation of volume of the kidney, but no organic change. Although the odour of diabetic urine is slight, it is of a more animalized nature than that of polydipsia; and if the latter be left to itself, it passes into the putrefactive fermentation, while that of diabetes passes into the alcoholic, depositing a whitish substance, which is a true ferment. The difference of *density* sufficiently distinguishes the two urines; for while that of diabetes furnishes a specific gravity of from 1026 to 1044, that of polydipsia furnishes one of but from 1000 to 1004, or at most 1008: the density in the one affection being greater, in the other less, than in any other disease, and forming the two extremes of the scale. The urine of diabetes polarizes light, which that of polydipsia does not. In diabetes the appetite may be much increased, while in polydipsia it is diminished; meat and gelatinous aliment are taken and digested in the former, vegetables in the latter. Nutrition is much more seriously affected in diabetes than in polydipsia; the continual emaciation, in spite of enormous alimentation, observed in the one, not taking place in the other. The diabetic patient easily takes cold, each cold becoming more and more obstinate, and usually terminating in phthisis. All those patients who do not die of a complication of the original disease, die tuberculous, a termination not observed in polydipsia. Arrived at such a period, the diabetes may seem cured; but in fact less sugar is secreted, because the patient now takes less food whence to elaborate it. Towards the end of the case the patient becomes œdematous, which he does not in polydipsia. Polydipsia does not easily make ravages in the constitution, the patient bearing it as well for twenty years as for six months, which is very far from being the case with diabetes. In both affections, the complication of a febrile disease may temporarily suspend their course. In one case seen by M. Vigla, the polydipsia was suspended during an intense reaction excited by blisters, and reappeared when this had subsided.

As to *diabetes insipidus*, M. Vigla is aware of no well-ascertained example of such a disease, which, without sugar in the urine, gives rise to emaciation and eventual phthisis.

The *causes* of polydipsia are unknown. It may occur at any age, in any climate, and in either sex. Generally its access is sudden, and it becomes fully devel̀oped in a few days. M. Vigla regards both it and bulimia as *neuroses*, deranging the health no more, or even less, than other neuroses. It obstinately resists all treatment; the only remedies which are of any occasional avail being antispasmodics.—*Gazette des Hôpitaux*, 1848, No. 130.

Œdema of the Glottis.

M. Baizeau (*Gazette des Hôpitaux*, 1849, No. 20) relates an interesting case of this affection. A soldier complained, on the night of the 8th of February, of slight angina, which next morning was regarded by the surgeon as of little consequence. During the night of the 9th his symptoms became much exasperated; and as they were about to remove him to the hospital on the morning of the 10th, he died asphyxiated. The tonsils were found scarcely enlarged. One of them contained some plastic exudation, mingled with a little pus. The mucous membrane was colourless, save here and there a patch of ecchymosis. The epiglottis and aryteno id-epiglottic folds were considerably swollen, and did not subside under pressure of the finger. The mucous membrane covering them was springled with reddish-brown spots, and was thickened and gorged with blood, a whitish plasma being effused beneath it, much resembling the matter found in cerebro-spinal meningitis, and not removable by scraping. An œdematous state of the glottis existed, especially on the left side; but the obstruction, unless this were greater during life, could scarcely produce asphyxia. The œdema glottidis was in this case dependent upon the phlegmasia of the epiglottis, just as œdema in the vicinity of hepatized portions of lung is dependent upon pulmonary inflammation; and probably several cases of asphyxia, which are attributed to amygdalitis, really depend upon this cause.

M. Bazzoni (*Gazette Médicale*, 1849, No. 7) furnishes a case still more sudden in its termination. A young girl, 20 years of age, in perfect health, and much addicted to very noisy singing, was attacked one evening with shivering, hoarseness, and difficulty of deglutition. At midnight intense dyspnœa set in, accompanied by a small pulse, and two hours afterwards she died, while preparations were making for performing tracheotomy. The only thing discoverable after death was great œdema of the cellular tissue of the entire larynx, unaccompanied either by redness or ulceration.

[To the above cases we are enabled to add another, which occurred a short time since in our own practice. A young man, æt. 20, previously in good health, was affected with slight *angina tonsillaris*, for which a blister and saline aperients were ordered. After the action of these he seemed, next day, much improved, and was left late at night by his friends, talking cheerfully, though in a subdued, almost whispering voice. They heard him making a strange noise about seven the next morning, and on entering the room found him dead. On a careful examination of his body, made by Mr. Swan, no other morbid appearance was discovered (save a few old pleuritic adhesions) than an œdematous state of the glottis and epiglottis, sufficient, doubtless, with attendant spasm, to obstruct the entrance of air. The body, however, at the time of death presented none of the external signs of death from asphyxia. These cases are distressing in the extreme to the practitioner, as from the slightness of the preceding symptoms his prognosis is at fault, and from the suddenness of their termination the appropriate treatment is not available.]

When the disease is of more gradual growth than in the above cases, and set forth by its appropriate symptoms, Dr. Buck (*American Journal Med. Sciences*, No. 33, p. 240) has found scarifying the epiglottis and glottis a very successful practice. The patient is seated in a chair, with his head thrown back, and supported by an assistant. He is directed to keep his mouth wide open, or this is done for him by the insertion of a gag between the molars of the left side. The forefinger of the left hand is now passed in at the right angle of the mouth, until it reaches the epiglottis, and then it may generally with but little difficulty be carried above and behind this part, so as to overlap it, and press it forwards to the base of the tongue. This done, a narrow, curved knife is introduced, with its concavity downwards, along the finger, until its point reaches the nail, when, by raising the handle so as to depress the blade, the cutting extremity is brought between the edges of the glottis, and is rotated from one side to the other, imparting a cutting motion to it during its withdrawal. This may be repeated, without removing the finger, two or three times on either side; and the margin of the epiglottis, and the swelling between it and the base of the tongue, may be scarified still more easily by the knife, or a scissors curved flatwise. The disagreeable sensation of suffocation is soon recovered from; a slight hemorrhage follows, which should be encouraged by tepid water. In all the cases the operation has required one or two repetitions. Eight cases of the disease have occurred in the New York Hospital in eleven months, in five of which scarifications were performed with success; the other three patients, in whom they were not resorted to, dying. Dr. Buck particularly dwells upon the *swollen state of the epiglottis* as a pathognomonic sign, ascertainable by the finger. The author was not aware that any one had performed an analogous operation except Mr. Busk, who treated two cases with success by repeated punctures made at the back of the tongue, uvula, and pharynx. Since he wrote his paper, however, he has learned from Valleix that Lisfranc scarified the glottis with success in five cases.

SURGERY.

On Coxalgia. By M. MALGAIGNE.

BY this term M. Malgaigne designates what is usually named hip-joint disease. He objects to some of the received doctrines concerning it. Thus, he denies that the disease is indicated at all generally by pain in the knee, the hip-joint comporting itself in this particular like other joints. Moreover, when such coincident pains do occur, he believes them to be very rarely of a nervous or sympathetic character, the affection being usually a polyarticular phlegmasia of a rheumatic character.

Then again, the idea that the limb *lengthens* at an early period of the disease, and afterwards *shortens* without dislocation taking place, is quite an erroneous one. In the great majority of cases, if the limb be left to itself, and no force applied, a shortening *appears* to exist. But in these this appearance is produced by the limb lying in a state of *adduction*, obliquely, as respects the pelvis; and if by chance the limb were observed while in a state of *abduction*, an elongation would be thought to be present. It is only by supposing these changes of position to have been overlooked, that we can explain Boyer's strange statement as to the change of length in the limb without dislocation. During the last ten years M. Malgaigne has made diligent search among a vast number of these cases, but has met with no such example. Larrey was accustomed to say a limb was two inches too long, and ordering his favorite moxas, declared soon afterwards that the normal length was restored; but his eye alone saw the miracle! For the explanation of the supposed change, theories have not been wanting. It has been said that a swollen state of the fatty mass in the articulation, or an effusion into the joint, extends the limb, forgetting that the effect of these must be to thrust the head of the bone outwards, and produce shortening. When the synovial fluid is much increased, the head may become quite dislocated, and *some shortening* be produced, as it may also when the solid parts of the joints are attacked, and the head of the bone in part eroded. While surgeons have paid so much attention to this imaginary elongation and shortening, they have paid no heed to the muscular contraction, which, by inducing adduction, produces the apparent shortening.—*Gazette des Hôpitaux*, 1848, No. 138.

[While appreciating M. Malgaigne's ingenious criticisms upon so many of the received doctrines of surgery, which stood in ample need of revision, we can hardly understand how one so familiar with hospital-practice can bring into doubt the actual occurrence of sympathetic pain in the knee in hip-joint disease, and this quite independently of any inflammatory condition, rheumatic or otherwise. As to the difference of length observed in the limbs, independently of consecutive dislocation, he has probably indicated the true cause of the deceptive appearance; and he justly observes elsewhere, that even by measurement, as after accident, slight differences of length in the femur are frequently very difficult of verification.]

On a New Variety of Luxation of the Humerus. By M. Malgaigne.

A man, æt. 63, presented himself to M. Malgaigne, 31st of October, with a luxation of the humerus, which had occurred on the 16th of the preceding August; and the subject being a thin one, he was enabled to convince himself that it was a variety of the accident of which he knew of no other example. The man had been pitched to a distance from the top of a high waggon on to the ground, and had fallen on his shoulder, the arm being at the same time compressed against the trunk. A careful examination showed that the head of the bone had been carried upwards and forwards above the acromio-coracoid ligament and the coracoid process, which was so concealed by the head of the bone as not to be felt. The exact seat of the head was on the acromio-coracoid ligament, anteriorly and superiorly to the coracoid process; so that it might be termed the *supra-coracoid* luxation of the humerus. It was irreducible at so remote a period.—*Rev. Méd.-Chir.*, tom. v, p. 30.

On Ligature of the Left Subclavian Artery. By Dr. Mason Warren.

According to Mr. Norris's tables of 69 cases of ligature of the subclavian, 36 recovered, and 33 died: and in every case in which it has been necessary to place a ligature on the tracheal side of the scaleni, a fatal result has occurred. The present case is remarkable on account of certain anatomical peculiarities requiring the ligature to be applied above the clavicle and enabling this to be done with success. It is further remarkable for the rapid establishment of the collateral circulation, the pulse being felt at the wrist twenty-four hours after the operation; and for the length of time which the ligature remained undetached, viz., ninety-six days.

The patient was a lady of delicate constitution, 30 years of age, having had a congenital club-foot, and consequent double curvature of the spine, both of which deformities had been remedied. She consulted the author for a pulsating tumour the size of a pigeon's egg, situated just above the scapular end of the clavicle, the origin of which was referred back four months, when she felt a sudden crack there while drawing a cork. After examining in vain for the subclavian as it passes over the first rib, the author was surprised to find the vessel passing obliquely upwards, parallel to, and about an inch from the external border of the trapezius, the whole brachial plexus being in contact with it. The first rib, and a part of the second passed obliquely across the neck above the clavicle. "The whole osseous system of the chest seemed to have undergone a partial displacement. The spine and ribs attached had been as it were moved upwards, while the sternum was carried in the opposite direction." The incision, about two inches long, extended from near the outer and upper edge of the sterno-cleido mastoideus downwards in the direction of the scapulo-clavicular articulation, and an inch from the edge of the trapezius, the pulsation of the vessel, in the absence of other anatomical marks serving as a guide. The recovery was ultimately complete.—*Am. Jour. Med. Sciences,* No. 33, pp. 13-17.

On the Treatment of Lupus. By MM. Devergie and Petrequin.

M. Devergie states that M. Emery has somewhat over-estimated the beneficial effects of cod-liver oil in lupus (p. 279); and he considers it a great mistake to depend upon any one remedy for the cure of diseases, especially of the skin, which may be simple or complicated in their nature, though still bearing the same name. With respect to lupus, there are two genera, the *tuberculous,* which affects the deeper parts, and the *serpiginous* or *herpetic* form, which affects the surface. If it has gone on to ulceration it is termed *exedens,* and *vorax* when this proceeds rapidly. Of all these it is the *serpiginous* form, prior to ulceration, that best yields to cod-liver oil, to which remedy lupus of the limbs and trunk is more amenable than is that of the face. In a case under treatment, in which the disease existed both on the body and on the face, the latter alone has not yielded. Many cases of limited *tubercular* lupus may yield to measures which exert a general modificatory power upon the constitution, and to the use of *Canquoin's* caustic, which M. Devergie much employs in these cases. *Herpetiform* lupus that has ulcerated is very advantageously modified by the application of juniper oil to its edges. M. Devergie is far from underrating the beneficial effects of the cod-liver oil, preferring it, indeed, to any other separate remedy. He does not, however, approve of its exclusive use, believing a combination of means, both external and internal, to be the most judicious procedure. To this end he lays down the following as being the principles of treatment which should guide us. 1. To endeavour to establish the regularity of the menstrual functions by means of the syrup of the iodide of iron. 2. To administer cod-liver oil internally. 3. To employ sulphureous or iodine baths. 4. To touch the lupus frequently, as every third day, for example, with the oil of juniper. 5. To apply Canquoin's caustic to tubercles which, during the process of cure, become isolated, but which still are dissipated with difficulty, and to ulcers which will not cicatrize. 6. To apply in some cases a slightly resolvent ointment, or a rubefacient preparation of iodine, to produce a modification in the more obstinate portions of the disease.—*Bulletin de Thérapeutique,* tom. xxxv, p. 466.

M. Chavannes, a pupil of M. Petrequin, of Lyons, furnishes a statement of the great success that practitioner has met with in treating the ulcerated form of
lupus—*lupus exedens*—by means of the *chlor* h is also of great utility
in other forms of skin disease when they t form, as carcinoma
of the face, eczema, tuberculous syphilide, nposed of *very pure*
laminated gold 1 part, *hydrochloric acid* 3 rt. It produces a
temporarily sharp pain, and coagulation of on the surface of
the ulcer, which changes in colour success ow to a purple,
violet, and black. A thick crust is the owever, is no

eschar, for there is no mortification, no loss of substance; but, on the contrary, a vigorous reproduction. If after a while the crust be raised, we see under it a delicate, reddish, newly-formed skin, which needs the strengthening attainable by another slight cauterization. The healing takes place without cicatrix, unless the tissues of the part have already been deeply destroyed; and even the cicatrices produced by other caustics may in some degree be arrested by the application of this one. Next to this caustic the *acid nitrate of mercury* is perhaps the best; but if applied over too large a surface, it may give rise to poisonous effects, while it produces much more pain and a very inferior degree of cicatrization.—*Revue Médicale*, 1848, tom. iii, pp. 45-70.

On the Treatment of Internal Strangulation of the Intestines by Strychnine.

By Dr. Homolle.

In this communication Dr. Homolle relates three cases in which he found very minute doses of strychnine (1 milligramme), repeated every hour, completely efficacious, after various other means had been tried in vain in relieving urgent symptoms of internal strangulation, produced in two of the cases by the return of the hernia *en masse*, and in the other by violent vomiting. It was the knowledge he had of the favorable influence which strychnine exerts upon the peristaltic action of the bowels, in constipation from cerebral paralysis, hypochondriasm, &c., that induced him to employ it in the present cases. The borborygmi, the painless sense of vermicular movement, the rapid cessation of the pain and vomiting, and the expulsion first of gas and then of fæces, which resulted from its employment, confirmed the anticipations he had entertained. M. Amussat, who employs the conjoined force of two or more persons for the reduction of hernia, believes that in this way nine tenths may be reduced; but the taxis is, in point of fact, but the substitution of an external constricting force for the insufficient contraction of the walls of the intestine. The alcaloid by increasing defective, or by correcting irregular, peristaltic action, diminishes the volume of the intestine, and expels the detained fæcal matters, or removes invaginations. It is to be recollected that the obstacle in strangulation of the intestine is hardly ever materially insurmountable. So that this means sometimes may, associated with the taxis, even in strangulated hernia, prevent the necessity of resorting to an operation.—*L'Union Médicale*, 1848, Nos. 138-9.

MIDWIFERY, &c.

Case of Hydrometra occurring in an Unimpregnated Uterus. By Dr. Grandidier.

This case occurred in the person of D. F., æt. 21, unmarried, and of a scrofulous habit of body. In Nov. 1842 her menses, which had been very irregular, were arrested, and the belly began to swell, so that in twelve weeks it resembled that of a pregnant woman in her last months. The distension was as equable as in ascites, but a fluctuation was only very obscurely felt. The extremities were swollen, respiration hurried, and the amount of urine small. Examination, *per vaginam*, having proved the groundlessness of the charge of pregnancy, she was treated by various hydragogues, &c., as for ordinary ascites; and it was not until paracentesis had been in vain attempted that a hydrometra was diagnosed. Ten grains of *secale cornutum* were now ordered every two hours, until uterine action was developed. Great pain and anguish was thus produced, which continued for twenty-four hours, when (120 grains of secale having been taken) a small plug of mucus was expelled from the vagina, and followed by the uninterrupted flow of 6 *maas* (64 oz.) of a clear watery fluid. The patient's sufferings were much relieved; her general condition improved; and while taking Stahl's pills her menses returned. Still her belly never resumed completely its normal size, and at intervals, varying from one to three weeks, there were gradually expelled other 18 maas of the same fluid. In June 1843 she repaired to the sulphureous waters at Nenndorf, the belly being as large as in advanced pregnancy; and the menses again disappearing, 10 maas of fluid were discharged from the vagina. In August, 7 maas; and in September, 39 were discharged; but after this period, the mineral waters and other means employed having removed the deranged condition of the digestive organs under which she had long laboured, and the discharge of water, *per vaginam*, which had probably accumulated during the stasis of the venous abdominal circulation, entirely ceased.—*Neue Zeitschrift für Geburtskunde*, Band xxiv, pp. 261-8.

Of Prolapsus of the Funis during Labour. By Dr. Hoffman.

Dr. Hoffman does not admit the validity of the explanation of this occurrence, which refers it to a disproportionately large pelvis or small head. If this were the case, the accident would be met with far more frequently than it is, and especially during premature labours. He believes it to be dependent upon an irregular contraction of the uterus, whereby the lower segment of the organ becomes unduly relaxed. He has never met with it in cases in which the pains have manifested their normal activity, but only in those in which they have assumed a spasmodic character. Upon these grounds he declares the mere reposition of the funis, unless the character of the pains can also be changed, to be of no avail; for the lower segment of the uterus not being applied to the head in these irregular pains, as it should be, the prolapsus is sure again to recur.

Moreover, the reposition of the funis is opposed by a general law which, however easily demonstrable, the author does not recollect to have ever seen stated. It is that *when during the progress of labour any portion of the ovum has quitted the cavity of the uterus, it can never be replaced.* As soon as any of the liquor amnii is discharged, the walls of the uterus become closely applied to the contour of the child, and the size of its cavity *pro tanto* diminished. Just in proportion as the parts of the child quit this cavity does its size continue diminishing, and neither a spontaneous or artificial return of these is possible. So it is with the funis; the cavity of the uterus having diminished in size since its descent, there remains no longer room for it. The general conclusion to be drawn is, that when the funis is prolapsed, to save the child, we must resort to turning, and place no reliance in the various instruments which have been contrived for its replacement.—*Zeitschrift für Geburtskunde*, Band xxv, p. 45.

On the Purulent Ophthalmia of Infants. By MM. Von Ammon and Mildner.

Dr. Wengler furnishes an account of the treatment pursued by Von Ammon in this destructive disease. A collyrium is prepared, consisting of 3 or 4 grains of ext. belladon., 6 to 8 drops of aqua oxymuriatica, and 3 or 4 oz. of water. By means of a small fine sponge, dipped in this lotion, the morbid secretion is gently removed from between the eyelids, and a few drops then inserted, after which fine linen compresses, also soaked in the lotion, are kept applied to the eyelids. In half an hour all this has to be renewed, the cries of the child announcing the discomfort produced by the accumulation of the secretion; and the removal of which from the ball of the eye is a great source of ease to it. After a while the belladonna is increased to 5 or 6 grains, and the aq. oxym. to 10 or 15 drops; the lotion being continued as long as the morbid secretion is produced, and being used at a higher temperature if the swelling is great, or the discharge ichorous. The indications sought to be fulfilled by its employment are, the diminution of the spasmodic action and turgescence of the eye, and the procuring the enlargement of the pupil by means of the belladonna, while the secretions of the part are improved, and decomposition prevented, through the agency of the aqua oxymuriatica. Internally purgatives are administered.—*Journal für Chirurgie*, Band viii, p. 598.

More than 600 cases form the basis of Dr. Mildner's Essay; and during only fourteen months of 1844-5, 300 infants were treated for this disease in the Prague Foundling Hospital. It commenced most frequently between the 6th and 9th day, and in only 7 out of 300 cases was it confined to one eye. The mean duration of the disease was 29 days. Of the 300 cases, 111 had ulcerations of the cornea, but in 62 these were not sufficiently serious to interfere much with vision. Ten children (in 300) were blinded, and 37 died of various diseases. In 112 cases there was also disease of the digestive organs, in 102 jaundice, and in 94 pharyngeal exsudations.

The *catarrhal form* of the disease may be either local, or a symptom of a general affection; and most of the children, in fact, manifested "an *albuminous crasis* of the blood," characterised by a catarrhal state of the mucous membranes, marasmus, and debility. The influence of atmospheric causes was obvious, and often when the wards were overcrowded with puerperal women, especially if the air were moist and cold, from six to ten infants would be seized on the same day, and usually in both eyes. When, too, the catarrhal affections took on a "septic" character, numerous cases of umbilical phlebitis, purulent infection, gangrenous erysipelas, croup, &c. were observed. The *croupal form* of the disease is characterised by exsudations of various thickness, which may assume the membranous form with newly-created vessels, and is often accompanied with the development of false membranes in the mouth and pharynx. When this variety took on the "septic" character, loss of vision might be predicted, though but a small portion of the cornea was affected.

It is very important to decide whether the disease is merely *local*, or symptomatic of a *general* affection. The former is the case when the mother is healthy and the child well nourished, the influence of an external agent recognisable, symptoms of catarrh, or exudations of the other mucous membranes, absent, and one eye alone affected, at least at first. It then rarely gives rise to large ulcerations, and causes fever only when the inflammation is intense. Its prognosis is favorable, and the influence of caustics is speedy and beneficial. When, however, the reverse of the above circumstances prevail, we should regard pyophthalmia as only symptomatic of a general diseased condition.

Treatment.—During 1836-9, of 454 children affected with pyophthalmia, 29 lost their sight, and during the following years but 22 in 689, and 10 in 300—an amelioration especially due to the improved hygienic conditions effected. When the disease is local and confined to the conjunctiva, especially when produced by the contact of gonorrheal or syphilitic poison, it will yield readily to the nitrate of silver (grs. 1 to 4 to 4 oz.)—*Annales d'Oculistique*, Sér. 4, tom. ii, p. 140.

[Terrible as are the devastations frequently committed by this disease in children's hospitals, it will usually be found rapidly tractable in private practice, providing from the first the hourly injection of a stimulating lotion into the eye be industriously persevered with until the discharge has markedly decreased. The practitioner must, however, show the parent how this is to be done, or the lotion will never come in contact with the diseased part; and he must rigidly insist on this frequent repetition. Either alum (grs. v to x ad oz.) or the nitrate of silver (grs. ii to v ad oz.), which, if the case proves tedious, is preferable, may be employed.]

On Prolapsus Uteri. By Professor HOHL.

PROFESSOR HOHL believes that some very erroneous notions prevail as to the *causes* of this occurrence, and that some light may be thrown upon the subject by considering the changes of position which the uterus normally undergoes at different periods of life. In the mature fœtus the uterus projects considerably beyond the pelvis; and it is only when it has acquired its completed shape and size at puberty, that it is found entirely within the cavity. At the commencement of the menstrual cycle it retains its position, or even rises still higher in the pelvis, while at the termination of this it again sinks, with the loss of blood, in stout young women. In women who seldom or never bear children it sinks still deeper, as it does, too, after the menstrual functions have ceased. In pregnancy the organ rises remarkably, and M. Hohl denies the correctness of the statement that it sinks lower in the pelvis after the second month, the apparent sinking being due to the turgescence of the organ, and especially of its cervix. After delivery the uterus remains high up in the abdomen, and only gradually resumes its ordinary position. In old women it is found deep in the pelvis.

The production of prolapsus is not dependent upon the condition of the vagina, and the ligaments of the uterus. The vital power of the organ may be said to maintain it in position. When this is augmented the uterus is raised, while, when it is diminished or lost, it descends. Other organs, and indeed the whole body, in like manner exhibit strength and power proportionate to their *turgor vitalis*. The increase of the vital activity of the uterus during its development and growth, as also during menstruation and in pregnancy, is attended with elevation of the organ, which sinks again when these conditions prevail no longer. So far from allowing that the prolapse results from defective supporting power of the vagina, we may rather regard the uterus as supporting the vagina, and prolapsus of the latter may occur without any prolapsus uteri.

Thus the author refers the production of prolapsus to a preceding or coexisting condition of health, giving rise to a diminished vitality. This explains why we so seldom meet with the disease in young healthy women; while we know that whatever favours the relaxation of the genital system, and lowers the tone of the fibre, acts predisposingly,—the germ of the evil being found in the puerperal condition, when the uterus, after having been high up in the abdomen, sinks down into the lesser pelvis.

Although prolapsus may be secondarily produced by other affections, as tumours of the belly, prolapsus vaginæ, cystocele, &c. &c., yet far more frequently a change in the direction, rather than in the position of the organ then takes place; and even while the portion of the rectum in connexion with its posterior wall may prolapse entirely, the uterus may retain its normal position.

There may be a diseased condition of the economy in general, or of the uterus in particular, upon which depends this extinction or diminution of its vital power; and accordingly as this is or is not curable, will depend whether the cure of a prolapsus is apparent or real; as mere reposition with mechanical support is not a cure. In some diseases which are attended with an increased activity of the uterus, there is a rising of the organ in the pelvis, as puerperal fever, hydrometra, &c. Disease of the ovaries do not produce any sinking of the organ; nor do tumours or indurations of its substance as long as they are in process of development, nor until they have interrupted its functions, or weighed it down by their great bulk. Polypi also seldom gave rise to prolapsus.

Treatment. Common as is the disease, a radical cure is seldom accomplished. The indications are to remedy the defective or disordered condition of the general vital powers or of those of the uterus in particular. The author especially warns us against the continued use of injections, and the too early employment of pessaries. When the vital power of the sexual system or uterus is exhausted in consequence of age, over-stimulus, or incurable disease, mere palliative treatment should be employed.—*Zeitschrift für Geburtskunde*, Band xxiv, pp. 321-340.

Case of Uterus Bicornis. By DR. GENERALI.

THE following case long excited great interest at Modena, being believed to be an undoubted one of superfœtation, an opinion indeed which the subsequent post-mortem examination does not entirely set aside. Gaetena Boratti had borne six children all at their full time (the hand having to be introduced for the removal of the placenta on each occasion), and was delivered of her seventh, a full-timed vigorous infant, on the 15th February, 1817, the placenta on this occasion being spontaneously discharged. The pains ceased, but the abdomen still remained large, and the movements of another fœtus were perceived. The tumour was confined to one side of the abdomen; and before the birth of the first child, a furrow along the median line seemed to separate the tumefaction into two parts. On the 14th March a second well-formed child was born. The first child lived 45, the second 52 days. In 1822 she had another child at full time. In 1847 she died of apoplexy, and the uterus, normal in respect to its os and cervix, was found to be bicornuated at its fundus, a fallopian tube being inserted into each of its cavities. The preparation is preserved in the Museum at Modena.—*Bulletin delle Scienze Mediche*, vol. xiii, p. 89.

MATERIA MEDICA AND THERAPEUTICS.

On the Employment of Iodide of Potassium in Saturnine and Mercurial Affections.

MM. Guillot and Melsens have recently presented a memoir to the Académie des Sciences, detailing an experimental investigation they have undertaken, respecting the powers of iodine in lead and mercurial saturation of the system. The object in view is to render soluble the metallic compounds which have entered the economy, by associating them with a body of very easy elimination. All the insoluble compounds formed by the salts of mercury with the matters met with in the economy, are soluble in iodide of potassium, which substance is easily and rapidly got rid of by the economy. By analogy we may infer, that the compounds of lead retained in the economy are also very probably dissolved and eliminated in the same manner; and in the memoir, cases of saturnine affections so cured are given. Sulphuric acid, or the sulphates, cannot be regarded as curative agents in the chronic diseases due to the employment of lead, seeing that sulphate of lead is itself a poison, capable of killing animals in a few weeks. If, however, we give to the animal sulphate of zinc and iodide of potassium simultaneously, no injurious effect results. If, nevertheless, we suddenly give a large dose of the iodide to a dog already suffering from disease from lead poisoning, it is speedily killed; while, if we give it in small and gradually increased doses, the animal gets rapidly well.—*Gazette Médicale*, 1849, No. 6.

Administration of Castor Oil.

M. Paul Dubois administers this medicine in a mode which is very palatable to the patient, and is of prompt effect. He gives a teaspoonful every ten minutes in a cup of very hot herb-broth, which entirely masks its taste. Generally three doses suffice; and an hour after the last he administers a simple enema.—*Rev. Méd.-Chir.*, tom. v, p. 51.

On Cannabine. By M. Dorvault.

Cannabine is generally seen as an amorphous substance, of a brown colour in the mass, and greenish in its lamellæ. Heated on platina it liquefies, takes fire, and burns without residue. It has an aromatic and nauseous odour, and a peppery, acrid taste. It is insoluble in water, and hence the impropriety of decoction or infusion of Indian hemp; but is soluble in alcohol, ether, fats, and oils. It was employed by Dr. Willemin in treating cholera at Cairo, and affords a good means of exactly apportioning the dose of Indian hemp, the different preparations of this containing the active principle in very various proportions. It may be given in the form of pills or tincture. The latter is formed by dissolving 1 part in 9 of alcohol, and filtering at the end of some hours; 1 gramme of this contains 10 centigrammes (2-5ths of a grain) of cannabine, which is the dose employed by Dr. Willemin in cholera. It may be given in infusion of tea or chamomile, and its action is assured by taking a cup of coffee at the same time, or soon after. The form of pill is, however, preferable; as the water to which the tincture of cannabine is added, owing to the insolubility of this, becomes turbid.—*Rev. Méd.-Chir.*, tom. iv, 313.

On the Hæmostatic Properties of Cotton. By M. Bourdin.

M. **Bourdin** states that he has long availed himself of the hæmostatic properties of this substance, and relates some of the cases in which it appears to have been very efficacious, as epistaxis, hemorrhage from varicose veins, opening the temporal artery by caustic, &c. It is of no avail in hemorrhage from arteries of large calibre. The wound must be very carefully sponged, and the cotton exactly applied by successive fragments, and moderate pressure maintained over it for several minutes. If the first attempts prove unsuccessful, the cotton should be entirely removed, and a new piece applied. When successful, it adheres to the part with great force, although sometimes a sero-sanguinolent fluid still for a while oozes out, on account of which the cotton need not be removed. The cotton becomes as hard and as difficult to cut as thick pasteboard, resists the action of water, and can only be removed at the expiration of several days, when loosened by suppuration.—*Rev. Médicale*, 1848, tom. iii, pp. 330-41.

On the Treatment of Angina Tonsillaris, by small Doses of Calomel.
By M. René-Vanoye.

A **practice** has sprang up in France of treating certain inflammatory affections by small doses of calomel, which seems deserving of trial in our over-dosing country. M. René-Vanoye, in the present paper, speaks very highly of the efficacy of one-grain doses, given twice a day, in acute *angina tonsillaris*, and which, if commenced prior to the formation of matter, induce resolution far more rapidly than is effected, if effected at all, by the ordinary measures.—*Rev. Méd.-Chir.* tom. iv, pp. 267-70.

On Absorption of Medicinal Substances by the Schneiderian Membrane.
By M. Compérat.

The author comments upon our so diligently searching out for artificial modes of absorption by means of the endermic method, when a natural and extensive surface, and one by means of which a very rapid absorption can be induced, is neglected. Thus, in a case related, some instants sufficed, after insufflation of a small dose of ergot of rye, to restore the contractility of a pupil that had been suspended by means of belladonna.—*L'Union Médicale*, 1849, No. 2.

Administration of Rhubarb.

M. **Martin-Solon** prescribes rhubarb according to an old mode of employing it, which consists in masticating small morsels for half an hour or more, swallowing first the saliva, and then the chewed portion. In this way a small dose acts efficiently in dyspepsia, hypochondriasm, or habitual constipation. It can easily be understood, that a medicine, thus incorporated with and dissolved in the saliva, preserves much more of its power than when given as powder or infusion. Dr. Giacomini states that it is thus administered in Italy with great advantage to delicate and nervous women and to convalescents from fever.—*Rev. Méd.-Chir.*, tom. v, p. 52.

On Sugar as an Antaphrodisiac. By M. Provençal.

Camphor has hitherto, by reason of its prompt effects, been considered as the best antaphrodisiac, and is the best antidote to cantharides. But sugar is a far more powerful and useful remedy, diminishing the venereal ardour, and repairing, in some degree as an aliment, the melancholy effects of over-indulgence. The dose is a pound or more, daily, in a quart of water, wine, or milk, according to the nature of the case. In the case of general irritability, as observed in members of the religious fraternity, and in priapism, it is best given in cold water. When excitement of the genital organs is complicated with irritation of the chest, milk is the best vehicle.—*Rev. Méd.-Chir.*, tom. v, p. 39.

FORENSIC MEDICINE.

On the Condition of Drunkenness as a Complication of Wounds, and as a Cause of Prompt or Sudden Death. By M. Tardieu.

It is highly important to bear in mind, that drunkenness is of itself a direct cause of death; and M. Devergie, in his Essay on Sudden Death (Ann. d'Hyg., tom. xx) has put it down as the determining cause in 14 out of 40 cases. M. Royert Collard observes, that the various lesions discovered show that the alcoholic poison has struck every organ with death, through the intermedium of the blood and nervous system. Attention has perhaps not been sufficiently fixed upon the amount of criminal responsibility which should be attached to the wounding a drunken person. Whenever wounds or other traces of violence are visible after a struggle, fall, &c., it is to these death is usually at once attributed; and the object of a juridical inquiry is to examine into the foundation of this conclusion, and ascertain the true amount of influence exercised. In the present paper many cases are related, of persons dying after injuries committed upon them while intoxicated; and they go to show, that no determination of the above point can be made in an absolute manner; for, if drunkenness is the determining cause of the cerebral and pulmonary lesions leading to death, it is sometimes difficult to avoid attributing its share to the accompanying struggle, emotion, rage, or prolonged exposure to cold—itself no stranger to the production of apoplexy. It is no less true, that the medico-legal question, apart from the special circumstances of the case, must be determined by the principle laid down by Foderé, that only that which depends properly upon the nature of the wound can be imputed to its author.

The cases related exhibit a remarkable similarity in the lesions found after death from drinking. Morgagni long since pointed out effusion into the brain as a cause of sudden death from drinking—the *apoplexia temulenta* of Sauvages. M. Devergie observes, that the brain, lungs, and heart, without offering any local, limited, or circumscribed lesion, exhibit a very general plenitude of the vascular system, as seen in the injection of the vessels of the brain, the fulness of the principal venous trunks going to the heart, and the more or less deep, brick-red of the pulmonary tissue. Acknowledging the justice of this statement, it may be carried still further, by observing that, in all the cases here recorded, the cerebral congestion was followed by hemorrhage, having the peculiarity that the effusion of blood took place into the arachnoid or ventricles. It is not meant to be stated, that drunkenness may not give rise to hemorrhage into the substance of the brain; but it is remarkable that, under its influence, meningeal apoplexy should be so frequently produced. So, too, the lesion of the lungs often consists in a true pulmonary apoplexy. The general statement to be made, then, is—that, in death supervening rapidly on drunkenness, pulmonary apoplexy, and especially meningeal apoplexy, are lesions, if not of constant, at least of very frequent occurrence, and characteristic when they do occur.—*Annales d'Hygiène,* tom. xl, pp. 390-433.

On Poisoning by Sulphate of Zinc. By Professors Platner and Pignacca.

Owing to the emetic properties of this substance, there are few cases on record in which it has produced poisoning, and perhaps none in which it has proved fatal. A brief account of four cases, which have occurred at Pavia, will therefore possess great medico-legal and physiological interest.

The *first* case occurred (November, 1847) in the person of a strong woman, æt. 25, who, believing she was taking a dose of Epsom salts, drank off a solution of an ounce and a half of sulphate of zinc. She instantly vomited, and then became affected with almost incessant retching and purging for half an hour, which continued afterwards at short intervals for three hours, and then gradually diminished. The pulse was frequent and small, and extreme prostration existed, accompanied with distressing restlessness and anxiety; the temperature of the skin was diminished; great pain in the abdomen, limbs, &c., existed, as well as a sense of burning in the throat and stomach. As the prostration increased the pain ceased, and she died 13½ hours after taking the poison, retaining her intellectual faculties to the last. On examination 40 hours after death, the following were the chief appearances observed: great lividity of the external surface; congestion of the brain and its membranes; a congested state of the lungs; heart flaccid, the right cavities being filled with black thick blood; inner surface of the stomach covered with a yellowish pultaceous matter, on the removal of which an uniform yellow-ochrous colour was observed, except towards the great curvature, where it became reddish; a gelatiniform ramollissement of the mucous membrane prevailed, exposing, in some parts, the submucous cellular tissue. The small intestines were somewhat injected, and contained yellowish matters.

The *second* case occurred in the person of a sister of the above, of robust frame, and 35 years of age, who, under the same error, took a similar dose at the same time. With her, vomiting was delayed for 15 minutes; but when it did occur, she found the rejected matters tasted similarly to the fluid she had swallowed, which was not the case with her sister. She had no purging for 10 hours; the other symptoms much resembled those of her sister, the burning sensation in the throat being, however, absent; but at the end of that period she began to rally, her pulse resuming some of its lost power. Next day febrile reaction occurred, which was followed by profuse sweating that soon became excessive. The fever, with pain at the epigastrium, continued until the sixth day; and debility long remained extreme. Neither pain, nor any symptom whatever, was referable to the head. Menstruation came on at its proper period. The treatment in both cases consisted in the encouragement of vomiting by abundant quantities of sugared tepid water, and the administration of magnesia with milk.

The *third* case was observed in a female æt. 31, who was recovering after delivery. She took a quarter of an ounce of the sulphate in mistake, and soon suffered from a sense of burning and constriction of the fauces, and excessive pain at the epigastrium. Efforts at vomiting were rendered availing by abundant drinks of warm water, and a solution of magnesia was given. After a while she rallied from a state of prostration, and six hours after profuse sweating set in, but she was free from suffering. For several days, however, she suffered from headache, delirium, and tremors, which her attendant attributed to acute gastro-enteritis, and treated as such.

A law student of robust habit of body, æt. 20, furnished the subject of the *fourth* case. When seen he was said to have been ill for four days, no further account being attainable from the neighbours, and he himself being moribund, apparently in an apoplectic condition. Upon examination, the brain and membranes were found much congested, and the pleura contained abundance of sanguinolent fluid. The stomach was very vascular, spots of ecchymosis being observable, and near the pylorus slight ulceration. Two drachms of sulphate of zinc were detected in the liver and blood, the fluids of the alimentary canal furnishing but little.—*Gazetta Medica di Lombardia,* 1848, No. 11.

On Death from Suffocation. By Dr. Casper.

Dr. Casper, in his official report, notices two circumstances which he says are much less known than they deserve to be. One of these is, that in death from suffocation, a *congested state of the kidneys* is of far more common occurrence than that of the liver, spleen, omentum, &c.

The other is an appearance highly characteristic of death from suffocation as it occurs in infants, generally from being overlaid by their nurses. It consists of a petechia-like sugillation of the pulmonary pleura, and external surface of the aorta and heart, giving the parts the appearance as if they had been spurted over by means of a pen and ink.—*Wochenschrift,* 1848, No. 45.

STATISTICS.

Modes of committing Suicide in France. By M. Brierre de Boismont.

M. Brierre de Boismont furnishes the following table as the result of an examination which he has made of the returns of 4595 suicides, committed in the Department of the Seine. Death occurred in

1426	by charcoal fumes.
989	by drowning.
796	by hanging.
578	by fire-arms.
207	by cutting-instruments.
158	by poisoning.
424	by precipitation.
16	by being run over.
1	by abstinence.
4595	

Annales d'Hygiène, tom. xl, p. 412.

A Statistical Inquiry, as to whether the proportionate Frequency, the Nature, and the principal Characters of Diseases have undergone in the course of time the great Modifications they are said to have done. By M. Valleix.

The object the author has in view in the present communication, is to determine whether, by the study of certain diseases in a diagnostic point of view, during an epoch not far removed from our own, and one that is remarkable for its revolutions in medical doctrines, we may arrive at certain conclusions, capable of being generalized and applied to anterior periods. It is continually asked, whether such or such a disease existed heretofore?—whether it prevailed as frequently formerly as now?—and whether changes in hygiene, the progress of civilization, or other more or less powerful influences, have not operated changes in our affections? In reply, we are obliged to confine ourselves to vague generalities.

The period M. Valleix has chosen for examination, is the twenty years intervening between 1819-39, during which facts of such an importance have occurred, as to have almost totally changed the face of science, so that a person, conversant with internal pathology only in works written before 1819, would scarcely comprehend the physicians of the present age. In 1818 appeared the last edition of Pinel's 'Nosographie Philosophique,' which had constituted the code of medicine in France for twenty years before. The author had, however, now reached the end of his reign, although the attacks of Broussais had not yet completed his dethronement. The latter published his 'Examen' in 1816, and *his* domination may be said to have continued from 1820 to 1830, to be submitted then, in its turn, to attacks no less vigorous and successful, leaving behind, however, certain general views of undoubted utility, and which have put us on the track of important discoveries. In a third period, from the fall of Broussaisism, we have the prevalence of "rigorous medicine," exact observation and assiduous experiment replacing doctrines founded on hypotheses and ill-observed facts. Typhoid fever now took the place of that gastro-enteritis, which itself had so violently displaced the ataxic, adynamic, bilious, putrid, &c., fevers of former times. Just as the system of Pinel was about yielding to that of Broussais, in 1819, appeared Laennec's work on Auscultation, imparting a precision to the diagnosis of certain diseases, of material service in solving the question before us.

M. Valleix, having had his attention directed to the large number of deaths from *pneumonia*, occurring at the Salpêtrière (the hospital for aged persons), examined the mortuary list for a long period back, and discovered most remarkable differences in the proportional frequency of this and other diseases. Laennec's work, having been published only in 1819, it required some time to educate physicians in auscultation. Thus, we find, from 1819 to 1821, 60 deaths per annum are set down to *pulmonary catarrh*, a number which, now we know how rarely this of itself produces death, seems enormous. From 1821 to 1829, the number averaged but 14 or 15; and from 1829 to 1839, but 19 per annum. Were cases of *phthisis* mistaken for pulmonary catarrh during the first period? No: for to this disease is also assigned a higher figure (39 and 40) than in later years, when it varied from 20 to 24. Nor do cases of mistaken *pleurisy* explain the difference; for the numbers of these, during the three periods, were respectively 30, 10, and 42. This latter discrepancy of numbers is to be explained by the fact, that, when auscultation was little known, many cases were, from the presence of pain in the side, cough, &c., set down as pleurisies. In the middle period, much hesitation prevailed in pronouncing upon such cases, while the more perfected knowledge furnished the high proportion of the third, and which, probably, in future will be maintained. The true explanation of the diminished number of deaths from pulmonary catarrh, is the vast increase of those proved by auscultation to be due to *pneumonia*, in the aged, as we shall presently see.

The consideration of the *cerebral affections* present analogous facts. From 1819 to 1821, *apoplexy* was alone familiarly known. Ramollissement, the paralysis of the insane, and encephalitis were nearly unknown. From 1819 to 1821, there were 158 deaths per annum attributed to *apoplexy*, which, in 1821-29 sank to 59, and continued at much the same figure (61) during 1829-39. A more exact knowledge of cerebral affections, derived from the investigations of Rostan, Lallemand, Calmeil, Foville, and others, explains this. Thus, in 1819-21, *ramollissement* is only assigned twice as the cause of death, and during the next period, information on the subject not yet being very accurate, it was generally confounded with encephalitis; but, during the next period, the number averages 22. Add the 22 to the 61 apoplexies of this period, and abstract it from the apoplexies of the first, and an approximation of numbers commences. As to *encephalitis*, during the prevalence of Broussaisism, most cerebral affections were so termed. Thus, during 1819-21, two or three deaths per annum are noted; during 1821-29, 48; and during 1829-39, when ramollissement asserted its claims, only 16. Add the 48 encephalites to the 5 ramollissements, and the 59 apoplexies of the Broussaisian period, and we have 112; while, if we abstract from the 161 of these diseases of the first period the 53 of the encephalitis and ramollissement of the second, we have 108—a not very different figure from 112. Next, if we add to the apoplexies and ramollissements of the third period, the 16 encephalites, we have the number 99, which forms some approach to that of 123, obtained by abstracting the 38 ramollissements and encephalitis from the 161 of the first period. The *general paralysis* of the insane will serve to fill up the lacunæ. Thus, while, in the two former periods, it only furnished 4 deaths, during the third it furnished 24.

We have now seen a disease, as it were, enveloping in itself and concealing the existence of three others, of a different nature, though occupying the same seat. We next have to observe several others, of apparently different nature, become sunk and lost in one which absorbs them all. During 1819-21, the word *fever* was still pronounced, though tremblingly, and we find 71 deaths attributed to adynamic, and 39 to 40 to ataxic fever.. During 1821-29, the adynamic sank to 19 or 20, and the ataxic to 6 or 7; while, during 1829-34, the diminution is still more complete,—the adynamic sinking to 3 or 4, and, after 1834, disappearing altogether, and the ataxic being noted but once in a year. What has become of all these fevers? During 1819-21, little mention was made of *enteritis* (23 per annum) and *gastro-enteritis* (9); but, during 1821-29, they averaged 36 and 37 respectively. It is evident that many fevers became thus transformed; and the proof of it is, the great diminution of these inflammatory affections since the fall of Broussaisism; but the adynamic and ataxic fevers have not reappeared to replace them. Both the fevers and the inflammations have, in fact, disappeared; and, for the explanation of this, we must return to the consideration of *pneumonia*. During 1819-21, this disease figures at 72 per annum; during 1829-39, at 110; and during 1829-39, at 256. Take from this 256 the 72 of the first period, and we have a residue of 184, which has furnished the material for the ataxic, adynamic, putrid, and other fevers, gastritis, gastro-enteritis, and, as we have seen, fatal pulmonary catarrh. Strange is the fact, that Pinel, himself physician to the Salpêtrière, furnished from this very hospital a description of fevers, recorded at the time with enthusiasm; and that these fevers, now termed typhoid, are no longer found within its walls. Stranger still, that Broussais, who saw imaginary gastritis or gastro-enteritis, could not see a real pneumonia! It can now, however, be no longer doubted, that pneumonia is *the* acute disease of the aged, and is, in fact, almost the only one, after a certain age, that occurs.

The general conclusion to be drawn from all that precedes is, that we must not blindly rely upon the statements of authors, to the effect that different diseases have appeared or disappeared at certain epochs, or that they have undergone much change in severity or frequency. We should first assure ourselves that such authors have not been deceived by a simple change of name, and have not thus transformed one disease into another.—*L'Union Médicale*, vol. ii, Nos. 101-4-7.

Statistics of the Hospitals and the Medical Profession in Paris.

THE administration of the Parisian hospitals employs 2500 individuals, and possesses a budget of from fifteen to sixteen million francs. There are fifteen hospitals, furnishing 7174 beds, and receiving 90,000 patients per annum. Besides these, there are 4 large hospices, and 7 retreats for 8000 aged and infirm persons. More than 100,000 receive *secours à domicile;* and above 25,000 foundlings are provided for. The following are the names and number of beds of the various hospitals:

General Hospitals.		Special Hospitals.	
Hôtel-Dieu . . .	810 beds	St. Louis . . .	825 beds
St. Marguerite . .	300 „	Du Midi . . .	300 „
La Pitié . . .	621 „	De l'Orsine . . .	300 „
La Charité . .	494 „	Enfans Trouvés . .	600 „
St. Antoine . .	320 „	Maison d'Accouchement .	514 „
Necker . . .	329 „	Maison des Cliniques .	120 „
Cochin . . .	325 „	Maison de Santé St. Denis	150 „
Beaujon . . .	438 „		
Bon Secours . .	323 „		
De la République .	600 „		

A year or two since, some of the Parisian medical journals, alarmed at the constant increase of the numbers of the profession, called aloud for some legislative means of repression. The number of qualified doctors of medicine steadily increased from 1090 in 1833, to 1442 in 1847; added to which, there were, in this last year, 175 of the lower qualified practitioners, termed *officiers de santé*, giving 1617 legalized practitioners for little more than a million souls (including hospitals, garrison, and other unremunerating bodies); while in London we had, at the same period, but 2500 regular practitioners for our two millions. Moreover, the midwives are, in Paris, a numerous body (480 in 1847), absorbing much remunerative practice, which, in London, falls to the practitioner. Then, again, classes of persons there resort to hospitals, who here pay for their attendance. If the remaining patients had been equally divided among the 1617 regular practitioners, it was calculated that 150 per annum would fall to each,—the charge for visits being 5, 3, 2 francs, or even less, and no bad debts being recoverable at the expiration of a year. One of the effects of the revolution has been, to produce at least a considerable temporary diminution of the numbers. Thus, at the commencement of the present year, there were but 1389, instead of 1442 doctors; the whole number of qualified practitioners diminishing from 1617 to 1555, and that of midwives, from 480 in 1847, to 385. The *pharmaciens* are the only portion of the body-medical who have held their ground, their numbers being 345 in 1847, and 363 in 1849.

Mortality. Of the 1423 doctors at the commencement of 1843, there died 37 during 1843-4; of the 1430 in 1845, there died 50 during 1845-6; and of the 1442 in 1847, there died 56 during 1847-8.—*Rev. Méd.-Chir.*, tom. v, p. 61.

BIOGRAPHICAL NOTICE OF DR. J. C. PRICHARD.

[*Abridged from a Memoir, read to the Ethnographical Society by* Dr. HODGKIN, *and kindly placed in our hands by the Author.*]

ALTHOUGH from the earliest periods of history her pages have been chiefly occupied by the records of the rise and fall and collisions of nations, history and even more ancient tradition have not failed to preserve the memory of superior individuals, and the most remarkable and exciting public events have not placed in the shade the light of those individuals who have been coeval with them. This does not merely apply to those historic characters, who, from the important part which they have taken in public affairs, have been handed down to posterity as the benefactors or the destroyers of mankind on an extended scale. The poet, the historian, and the philosopher, have shared with the lawgiver and the conqueror in this enduring celebrity; and it is the characteristic of the intensity of their unborrowed light, that it is uninfluenced by any appreciable distance of time. It may seem strange that I should thus introduce a notice of our late distinguished President by an allusion to those worthies of antiquity whose names, though unconnected with any great exploits, have been preserved in fame solely by the enduring character of virtue and talent. But it has forced itself upon my reflection that the year 1848, which must ever be remarkable amongst the years of the nineteenth century for the savage atrocities that have signalised those wars of races, which have disgraced it, will also be remembered as the year which closed the life of the greatest writer who has treated of the science of Ethnology, and investigated and classified the nations, and kindreds, and tongues of voice-varying men.

James Cowles Prichard, the eldest son of Thomas and Mary Prichard, was born on the 11th of 2mo (Feb.) 1786, at Ross, in Herefordshire, where his family had resided for several generations. His parents were members of the Society of Friends, and in its principles he was himself educated. The name of Dr. Prichard may therefore be adduced with those of Fothergill, Willan, Lettsom, Sims, Birkbeck, and the very distinguished Thomas Young, in his own profession, and with those of Dalton, Goff, Howard, Allen, and of many others out of it, in refutation of the opinion that the principles of this society are unfriendly to the cultivation of the intellectual faculties. Thomas Prichard (the Doctor's father) was educated at a school at Burford, in Oxfordshire, which was kept by Thomas Huntley, who enjoyed considerable reputation as a classical teacher, amongst the Society of Friends; and who, if he was not a profound scholar, had considerable facility in Latin composition, and possessed the faculty of imparting a taste for study to his youthful pupils. Luke Howard and Bracy Clark, both distinguished for laborious research in very different subjects, were educated by him.

From an extended memoir of Thomas Prichard's life, written by Dr. Prichard, and which I have had the opportunity of consulting, it appears that Thomas Prichard left school with only an imperfect acquaintance with the classics, but with a strong taste for study, which he pursued privately, not merely for the improvement of his knowledge of the ancient authors, but also for the acquisition of French, German, and Hebrew. He married when he was about twenty years of age, and whilst yet a young man, was left a widower with four children, to the care and education of whom he most sedulously devoted the time which could be spared from the mercantile pursuits in which he was engaged. I am informed by one who knew him well, that he was a man of a refined and cultivated mind, great poetical imagination, of fervent piety, and of a depth of feeling and affection that could only be appreciated by those who had the privilege of intercourse with him. He lived to see the fruits of his early training, and to bless his grandchildren and great grandchildren.

James C. Prichard, the object of this notice, was never sent to school, but his ardent thirst for knowledge kept him closely applied to his books. He was taught Latin and arithmetic by an Irishman named John Barnes, French by an emigrant named De Rosemond, and Italian and Spanish by Mordenti, who called himself a Roman. It formed a part of his father's plan early to introduce a practical acquaintance with French as well as English, and for this purpose it was his practice to devote most evenings to reading English from a French book, often from Rollin's history. He then required his children to give in French what he had said in English. Familiarity with French and a taste for history were thus imparted together.

The taste for those researches for which our late President was so justly distinguished, exhibited itself at this early period of his life. He was fond of tracing the genealogies of kings of the most remote historic times; and as his father was then residing in Bristol, he employed himself in finding out and examining the specimens of the natives of different countries who were to be found amongst the shipping of that port; and he would occasionally bring a foreigner to his father's house. His familiarity with Spanish and the modern Greek was in part attributable to this cause.

On Thomas Prichard's retiring from business, he left Bristol and returned to Ross, where his son James continued his studies under private tutors.

From Ross, James C. Prichard was sent to Bristol, to enter upon the study of medicine. He had made choice of this pursuit, not from any special predilection for medical subjects, but, regarding a profession as more favorable than commerce to those studies to which he was devoted, he rather accepted than chose Medicine, as the only one which at that time was regarded as accessible to the members of the society to which he belonged. He went to Bristol in 1802, where he studied under Thomas Pole, an ingenious American, who was likewise a member of the Society of Friends, and who had for years been engaged in London in practising and teaching the obstetric art. He was much devoted to collecting and preserving preparations and specimens, and has left a work on the art of making preparations, which is still esteemed as one of the best on the subject.

In the summer of 1802, J. C. Prichard was removed to Staines, where he practised pharmacy under Robert Pope and E. Tothill, Friends engaged in extensive general practice, the former of whom was subsequently known as Dr. Pope, in frequent professional attendance at Windsor Castle.

A member of Edward Tothill's family has stated to me in a letter, that Dr. Prichard applied himself with diligence to the object for which his father brought him to Staines, and that at other intervals he pursued his various studies with great assiduity. He gladly availed himself of the opportunity of acquiring much practical knowledge from E. Tothill, and of profiting by the long experience which a very extensive practice had furnished. He always acknowledged his obligations to this source, which contributed to cement a mutual esteem between the master and pupil. His conduct in the family was marked by amiability, and this, together with very buoyant spirits, rendered him a very pleasant associate to those near his own age. This notice of the doctor's early industry and application to study is well worthy of attention. It holds up an example which, like the early years of other great men, should stimulate the youth of future generations to follow the same course, and ever to remember the importance of taking advantage of the seed-time of life, by well-directed and assiduous labour, if they have any laudable wish to attain to eminence, or even to escape mediocrity in later life.

From Staines, James C. Prichard proceeded to London, and pursued his medical studies at St. Thomas's Hospital, where his late colleague in the Lunacy Commission, Dr. Turner, was at that time engaged as a lecturer.

He went to Edinburgh in the autumn of 1806; and it was during his residence as a student in that University, that he first embodied his ideas on the varieties of the human race. His friend and fellow-student, Dr. Arnould, of Wallingford, thus speaks of him: "From the year 1807 we were very much together, and from that time, during our stay in Edinburgh, the history of his book is the history of his life, for it was the continual occupation of his mind. In our daily walks it was always uppermost—a shade of complexion—a singularity of physiognomy—a peculiarity of form—would always introduce the one absorbing subject. In the crowd and in the solitude it was ever present with him. I well remember when one evening we were wending our way amidst the mountains in the neighbourhood of Loch Katrine, not so much frequented then as it has been since the 'Lady of the Lake' appeared; it was near the going down of the sun, when, amidst the wildest scenery, we saw a Highlander on a distant crag, standing out clear and distinct, and seemingly magnified to a large size, and his huge shadow stretching out towards us. The effect for my friend was magical—fatigue was felt no longer, and he at once resumed all his powers of mind and body and poured out a most splendid dissertation on the history of the Celtic nations—the dark, fearful gloomy, and savage rites of the Druids—and conjured up the horrors we should have endured, if in those earlier times we had been lonely wanderers in that remote district; and beguiled the weariness of the way till we reached our place of rest at night.

"His favorite topic was a frequent subject of discussion in a private debating society called the Azygotic. It consisted of six members, Charles and Patrick Mackenzie, Hampden, Estlin, Prichard, and Arnould. We met at each other's houses one evening in the week for literary, scientific, and philosophical discussion. On the night of Prichard's paper, which was the basis of his thesis for his doctor's degree, we had a very long, animated, and interesting debate."

I have understood that the young ethnologist maintained a correspondence with his father on the subject of his investigations, and that the good man not only took a lively interest in the inquiry, but expressed his desire that his son would maintain the orthodox side of the question with respect to the unity of our race. Judging from the uniform tenor of Dr. Prichard's mind, I am induced to believe that to this side his own views were always disposed to incline, although he has collected and stated the arguments on both sides with perfect fairness and impartiality.

It may seem rather paradoxical, yet I cannot withhold the observation, that this bias is more favorable to the attainment of the true solution of the question, than the opposite tendency, and a readiness to admit independence of origin in several distinct pairs. This last assumption, by affording a ready explanation of the several varieties of form, colour, and stature, must tend to damage the ardour of research; whereas the desire to discover the proofs of connexion, in spite of these diversities, is like a lantern to our path in the obscurity of night; whilst the objections of opponents, whether urged or anticipated, must ever be ready to recall erring steps, where any deviation is made from the path of truth.

Amongst the names of those with whom Dr. Prichard became acquainted during his stay in Edinburgh, and whose friendship he maintained in after life, I must mention that of Dr. Thomas Hancock, likewise a member of the Society of Friends, and subsequently known as the author of two very interesting works, the one relating to the instincts of the lower animals, the other an historical account of the principal epidemics which have been placed on record, with an inquiry into the laws which have appeared to regulate them.

Having completed his curriculum, and taken his degree in Edinburgh, Dr. Prichard passed a year at Trinity College, Cambridge,—the superior liberality of that University allowing dissenters the privilege of studying, though not of graduating there. I have not been informed of the doctor's course of study whilst at Cambridge; but as he was in after life a very fair mathematician, it is more than probable that his readings at Cambridge were chiefly mathematical. It is certain that he must have spent a portion of his time on theology; for it was at this period of his life that, on the ground of conviction, he separated himself from the Society of Friends. I never learned, and it would be foreign to this notice, here to discuss the doctrinal points which occasioned this separation; but I cannot forego the pleasure of recording that which I had myself the opportunity of observing, and which was alike characteristic of the excellence of his natural disposition, and of his true Christian charity,—that he retained in after life a most kind and amiable feeling and interest in relation to the society and its members. The change to which I have just adverted enabled the doctor, who had joined the communion of the church of England, to enter as a student in the University of Oxford. He first resided in St. John's, but afterwards transferred himself to Trinity College, of which he was a gentleman commoner. Knowledge, not title, was his object, and he sought no degree from the University.

In 1810 he settled at Bristol as a physician, and continuing his researches on the Physical History of Man, he brought out the first edition of his work on that subject towards the close of the year 1813. The views which he at that time adopted, and the scope embraced by this work, the extension of which in subsequent editions occupied so large a portion of his attention, and justly procured him universal reputation, cannot be better stated that in the doctor's own words:

"The nature and causes of the physical diversities which characterise different races of men, though a curious and interesting subject of inquiry, is one which has rarely engaged the notice of writers of our own country. The few English authors who have treated of it, at least those who have entered into the investigation on physiological grounds, have, for the most part, maintained the opinion that there exist in mankind several distinct species. A considerable and very respectable class of foreign writers, at the head of whom we reckon Buffon and Blumenbach, have given their suffrages on the contrary side of this question, and have entered more diffusely into the proof of the doctrine they advocate.

"My attention was strongly excited to this inquiry many years ago, by happening to hear the truth of the Mosaic records implicated in it, and denied, on the alleged impossibility of reconciling the history contained in them with the phenomena of nature, and particularly with the diversified characters of the several races of men. The arguments of those who assert that these races constitute distinct species, appeared to me at first irresistible, and I found no satisfactory proof in the vague and conjectural reasonings by which the opposite opinion has generally been defended. I was at last convinced that most of the theories current concerning the effects of climate and other modifying causes, are in great part hypothetical, and irreconcilable with facts that cannot be disputed.

. . . . . .

"In the course of this essay I have maintained the opinion that all mankind constitute but one race, or proceed from a single family, but I am far from wishing to interest any religious predilections in favour of my conclusions. On the contrary, I am ready to admit, and shall be glad to believe, if it can be made to appear, that the truth of the Scriptures is not involved in the decision of this question. I have made no reference to the writings of Moses, except with relation to events concerning which the authority of those most ancient records may be received as common historical testimony, being aware that one class of persons would refuse to admit any such appeal, and that others would rather wish to see the points in dispute established on distinct and independent grounds."

In this work Dr. Prichard set forth the differences of colour, hair, stature, and form, and examined the value of each as an evidence of difference of race, and inferred from the occurrence of these and similar differences, where identity of race could not be doubted, that they must not be received as evidences against the unity of our species. He successfully combated the old opinion that the influence of the sun continued through several generations has produced the blackness of the Negro, and adduced instances in proof of the continuance of black or brown and the white complexion through numerous generations, in almost every latitude and climate. He inquired into the production and permanency of varieties in man and in inferior animals,—examined some of the causes which may tend to produce them, and following up an idea adopted by John Hunter, that cultivation is a powerful cause of producing variety, and of lowering the intensity of colour in animals and plants, he makes the suggestion that civilization has been the operative cause which has produced the white varieties of the human species, of which he supposed that the first pair were black. He related many curious facts collected from several parts of the globe in support of this bold and ingenious theory, the announcement of which excited both surprise and interest. Though the doctor ventured to offer this conjecture, the work was throughout an appeal to fact and evidence; and not satisfied with merely inferring that resemblance in form, colour, language, and habits are proofs of a community of origin amongst the inhabitants of distant islands, he adduced the instances of canoes with their crews having lost their way, and being conveyed by winds or currents to a distance of hundreds of miles across the ocean. The work contains a description of the known varieties of man, in which the author adopted the division proposed by Blumenbach, and exhibited a great amount of research in the number of authors from whom his descriptions were collected. Even at this early period of the author's researches, a large amount of labour and erudition were devoted to the ancient Egyptians and Hindoos.

About thirteen years intervened between the publication of the first and second editions of the doctor's work; and as his growing celebrity as a physician had in the mean time raised him to eminence in his profession, it may not be amiss here to make a digression from his history as an ethnologist, in order to speak of him as a medical man, in which character he would have been distinguished, had he written nothing upon ethnology. It has already been stated that Dr. Prichard did not embrace the profession of medicine from any strong and early predilection. But what is of far greater importance to the study of the wide range of subjects which the science of medicine embraces, he brought to it that accurate observation which is the result of habitual exercise; and that aptitude for continued and varied study, which springs from the union of talent with early education, and is the surest preparation for sound professional knowledge, and safe and successful practice. And I may here be allowed to remark that nothing is more absurd than the vulgar error, that there may be an intuitive knowledge and natural gift which of themselves confer on their possessors a marvellous skill in the healing art. Dr. Prichard applied himself with as much zeal to the practice as he had done to the study of his profession. He established a dispensary. He became physician to some of the principal medical institutions of Bristol. He had not only a large practice in his own neighbourhood, but was often called to distant consultations. Notwithstanding the engrossing nature of these occupations, he found time to prepare and deliver lectures on physiology and medicine, and wrote an essay on Fever and one on Epilepsy, and subsequently a larger work on Nervous Diseases.

Amongst the patients who came under the doctor's care in public practice were the inmates of a lunatic asylum; and combining the results of his own observation and experience with that laborious research which he was accustomed to employ on all the subjects to which he directed his attention, he was enabled to produce an excellent treatise on Insanity, which was first published as one of the articles which he contributed to the 'Encyclopædia of Practical Medicine.'

Notwithstanding his numerous avocations, Dr. Prichard continued his literary and scientific studies; yet many of these had more or less a bearing upon his favorite subject—the history of Man. He acquired the German language, in which so many profound works on philology and history are composed; and as an exercise, he prepared and published, in conjunction with his friend W. Tothill, a translation of Müller's General History. He wrote an article on the Mithridates of Adelung. He continued his researches on Egyptian mythology and history, in which he investigated their relations to those of India. He contributed various articles to reviews and other periodicals, of which I have not been able to obtain a complete list, but the following may be mentioned: A paper on Snowden—three papers on the Mosaic Cosmogony, in Tilloch's Journal.—Papers on the Universities—on the Zodiac—on Isis and Osiris—on Faln and Schlegel.—Articles on Delirium, Hypochondriasis, Somnambulism, Animal Magnetism, Soundness of Mind, and Temperament, in the 'Cyclopædia of Practical Medicine;' and several chapters on similar subjects in the 'Library of Medicine.' Also a small volume on Insanity connected with Jurisprudence, and a highly interesting essay on the Vital Principle.

The study of the Hebrew language was alike congenial to his religious feelings and philological taste. An essay on the Song of Deborah, which he wrote for the gratification of his friends, is an interesting piece, in which, though short, the doctor appears in both characters.

Study was so thoroughly identified with his life, that even the hours which he could spare from social intercourse were made subservient to his literary pursuits, and Greek readings with a few learned friends occupied the time which other men devote to light or frivolous pursuits. A poetical translation of the *Birds of Aristophanes* may be mentioned amongst the fruits of these *horæ subsecivæ*.

In the year 1826 the doctor published the second edition of his 'Researches into the Physical History of Man.' In the interval of nearly thirteen years which had elapsed, he had not only collected a great amount of valuable materials, but had brought to bear upon the difficult questions which his subject presents, a variety of collateral knowledge for their elucidation, thereby not only enhancing the value of his own researches, but pointing out to future inquirers the path to truth, in which he had made such important advances. In the first volume he treated largely on the curious subject of the diffusion of organized beings, both vegetable and animal, entering into a most minute examination of a question which had previously occupied the attention of the great Linnæus, who maintained that in every species of plants, as well as of animals, only one pair was originally produced. "Unum individuum ex hermaphroditis et unicum par reliquorum vivantium fuisse primulùs creatum sana ratio videtur clarissime ostendere."

In this edition increased precision was given to characteristic differences of form, complexion, hair, and stature, the circumstances under which they occur, and the causes by which they may be influenced. The descriptions of the numerous families of mankind were greatly multiplied, and at the same time given with greater minuteness. But it must be observed, that in a work of this kind, the author's own personal observations must, even in the case of a great traveller, be comparatively limited; whilst the author who writes in his own fixed residence, though he enjoys the largest amount of collected materials, must, nevertheless, be subjected to the serious inconvenience of being supplied with statements which may be either seriously defective, or absolutely inaccurate, without his being able at the time to correct or even to detect them. Renewed research and the division of labour are indispensable for the completion of the task, in the progress of which there will be much to interest and reward the ethnologist who will take Dr. Prichard for his guide and instructor.

The diffusion of mankind presents one characteristic of the highest importance for its elucidation, which is altogether peculiar to our species. The characteristic to which I allude is that of *language*. It may be said, that, in this respect, it resembles many other characteristics resulting from the progressive cultivation of successive generations, which is the peculiar privilege of our race. Language, it is true, is subjected to the influence of this progressive cultivation, and preserves an important record of its advances. Yet there is, nevertheless, something peculiar in the subject of language, which places philology, as applied to the study of the human race generally, in a most exalted and important position amongst the abstruse sciences. I have only to appeal to the elaborate disquisitions of our learned associate, Dr. Latham, for the proof of this assertion.

But to return to Dr. Prichard. The philological portion of the subject, in the second edition of the work, was greatly enriched by a survey of the different relations of languages to each other—by the announcement of his discovery of the affinity of the Celtic languages with Sanskrit and other members of the Indo-European family, and by a tabular view of the known families of man, with their localities and languages, arranged according to their geographical distribution.

The affinities of the Celtic languages formed the subject of a separate volume, which Dr. Prichard published in 1831.

It will be readily understood, that, by a man of Dr. Prichard's learning and strong predilection for linguistic study, the philological element of ethnology would be by no means underrated. In two able reports, which he presented to the British Association for the Advancement of Science, he assigns to it its true and important place. In the Report of 1832, he successfully employed it as a corrective of classification founded on external characters only, which had led even the great and learned Cuvier to fall into palpable inaccuracies in his principal divisions of the human race.

In 1838, Dr. Prichard published an analysis of the Egyptian Mythology, which was a considerable extension of a former work which he had published on the same subject, with a critical examination of the remains of Egyptian Chronology. This treatise was translated into the German language at the wish of Professor Welcke, of Bonn, and a preface to it written by the learned archæologist, Augustus William von Schlegel. Professor Schlegel, while paying a just tribute to the learning and acuteness of the author, and to the profound character of the work in question, combats the general conclusion derived by Dr. Prichard from his comparison of Egypt with ancient India, in regard to the most important elements of their religion and political constitution. That general conclusion is, "that the same fundamental principles are to be traced as forming the groundwork of religious institutions, of philosophy, and of superstitious observances and ceremonies among the Egyptians and several Asiatic nations, more especially the Indians." It would be out of place here to enter at length into the character of the evidences adduced by Dr. Prichard in support of this conclusion. The treatise itself presents an ample and methodical arrangement of the authorities on the subject of Egyptian mythology and philosophy, from the writings of Pagan and Christian authors. What remains of ancient literature and philosophy, bearing upon Egyptian history, has been copiously collected and carefully applied to the illustration of this obscure and intricate branch of the history of mankind. As in all other of Dr. Prichard's writings, there is no straining of evidence to support a favorite hypothesis, but a careful statement of facts and circumstances, with a view to the elucidation of truth. The conclusion drawn from the remarkable coincidences and relations which Dr. Prichard pointed out as existing between Egyptian and Indian modes of thought, has received considerable support from a quarter, the least expected. Recent investigations into the structure of the old Egyptian language, revealed to us by the successful interpretation of the hierogrammatic writing, have demonstrated an early original connexion between the language of Egypt and the old Asiatic tongues. By this discovery, the Semitic barrier interposed between the Egyptian and the Asiatic races is broken down, and a community of origin established, which requires the hypothesis neither of the immigration of sacerdotal colonies, nor the doubtful navigation of the Erythræan sea. The profound views which led Dr. Prichard to assert, that, "although many obstacles present themselves to the supposition that direct intercourse subsisted between the Egyptians and the nations of Eastern Asia, there appear, even on very superficial comparison, so many phenomena of striking congruity in the intellectual and moral habits, and in the peculiar character of mental culture displayed by those nations, and particularly by the Egyptians, when compared with the ancient Indians, that it is extremely difficult to refer all these analogies to merely accidental coincidence," have thus been remarkably confirmed. His comparisons of individual personages of the mythologic system of either nation may not bear the test of measurement by the more extended knowledge of the subject which a quarter of a century has produced; but the terms of the general conclusions which are deduced from his 'Analysis' may be fairly taken to be past all dispute.

The 'Critical Examination of the Remains of Egyptian Chronology' is a remarkable monument of Dr. Prichard's sagacity, and of his aptitude for the elucidation of an obscure and intricate subject. The difficulty of the task which he here undertook, he has not overrated when, after laying before the reader the lists of Manetho and Eratosthenes, the old Chronicle, and the dynastic chronology of Herodotus and Diodorus, he says, "nothing can be more discouraging than the first survey of the fragments we have extracted. When I first examined these fragments, with a view of computing from them the Egyptian chronology, they appeared to me to be an inextricable tissue of error and contradiction. I repeated my attempt several times, at intervals, before I obtained the smallest hope of success, or a ray of light to guide me through the labyrinth. At length I thought I discovered a clue, which I have followed, and have persuaded myself that it has enabled me to unravel the mystery."

That clue was discovered by the same kind of investigatory process, which has been applied in all Dr. Prichard's researches,—the obtaining fixed points of coincidence or agreement, with which to form a standard of comparison for apparently discordant materials.

The view taken by Dr. Prichard, founded on the internal evidences of the documents themselves, as to the relative characters of the lists of Manetho and Eratosthenes, is in its leading features, and especially as relates to the earlier period of the Egyptian chronology, fully borne out and confirmed by later experience; the conclusion deduced from a comparison of the lists, being very nearly the same with that arrived at by the Chevalier Bunsen, aided by an examination of original and all but complete monumental and documentary chronological records of Egypt. Bunsen makes the first twenty-two sovereigns of Eratosthenes correspondent to the first, third, fourth and sixth dynasties of Manetho, rejecting from the list of Manetho the second and fifth dynasties, as had been done by Dr. Prichard.

That in other points the chronological comparisons instituted by Dr. Prichard should not have been confirmed by subsequent discoveries, is by no means extraordinary. Professor Schlegel has truly observed of this treatise, that the learned industry and the intelligence of the procedure of its author are worthy of all commendation; and it may be safely affirmed that its production at a period when the chronology of Egypt was almost a blank in history, is an enduring testimony to the critical acumen and profound sagacity, no less than to the extensive learning, of its author.

On accepting the office of Inspector of the Lunatic Asylums, Dr. Prichard relinquished private practice, resigned his post as physician to the Infirmary, which he had held for more than twenty-six years, and transferred his residence from Bristol to London. To this change our society is indebted for the privilege which we have enjoyed of having the greatest of ethnologists as our president. He succeeded our first president, Sir Charles Malcolm, to whose able exertions at its origin, and during the progress of its formation, the Ethnological Society of London is incalculably indebted.

After his settlement in London, Dr. Prichard completed the third edition of his work, which, we know, is extended to five closely printed volumes, forming a mass of learned and scientific research and laborious compilation, far superior to anything which had been previously produced on ethnology, and scarcely surpassed in the literature of any other science.

Having already noticed the principal divisions of the subject in speaking of the doctor's previous writings, I will not now trespass on the time of the Society with any further observations on this great work. Whilst its publication was in progress, Dr. Prichard produced a smaller work on the same subject, which appeared in illustrated numbers, designed to encourage and popularise the study of ethnology by consulting the taste of the day. On the completion of the larger work, Dr. Prichard observed that he considered his literary labours as accomplished;

yet we cannot doubt that, had his life and health been spared, his ever active mind and confirmed habits of study and labour would have continued to gratify and instruct us by further productions of his well-stored mind; in fact the subject of my last conversation with him, as we walked together from the last meeting of this Society at which he presided, was the publication of a collection of plates of human skulls illustrative of ethnology, somewhat on the plan of the 'Crania Americana' of my friend Dr. Morton, of Philadelphia.

It cannot fail to be a matter of surprise and wonder, when the nature of the doctor's private practice and the character of his official duties, which called him much from home, are considered, how he was able to accomplish so much. I have been informed that he not only had acquired the rare and invaluable habit of saving and occupying those detached fragments of time which it is most difficult not to lose, but that he also possessed the remarkable faculty of being able at once to resume and proceed with his compositions at the point at which he had left them.

Dr. Prichard appeared to be in possession of his usual health till within a few weeks of his death; yet it is probable that the unusual dampness of the latter part of the last year, to which may be ascribed the remarkably low and atonic characters of almost every case of illness, had produced a latent influence on his system, and prepared it to yield to the exciting causes which were applied.

He had left his home, and was engaged in one of his official tours, when he was seized with a severe feverish attack while visiting the lunatic asylums in the neighbourhood of Salisbury, on Monday, the 4th of December, 1848, and was confined in that city until the 17th, when he was conveyed to his own house in London. The fever proved to be of a rheumatic and gouty character, baffling all the efforts of medical skill, and terminating his life, after much suffering by pericarditis and extensive suppuration in the knee-joint.

It is scarcely necessary that I should say anything respecting the personal appearance of our late friend and President, which must be indelibly impressed on the minds of the members of the Ethnological Society; but for the sake of gentlemen who may not have had the privilege of being acquainted with him, I may be allowed to quote the following graphic description which was penned about ten years since by my friend Professor Gibson, of Philadelphia, when on a visit to this country.

"Dr. Prichard is about fifty years of age, is a short, compact, close-made man, with bluish-gray eyes, large and prominent features, and expression uncommonly mild, open, and benevolent; so much so, that almost any one would naturally inquire who he was. His hair is thin and scattering, and so white as to make him look older than he is, whereas, in former days, it was light chesnut, and so remarkably thick, bushy, and upright, as to form one of his striking characteristics. In dress he is singularly plain, simple, and unostentatious, and, if in drab attire, might pass readily for a Quaker. Starchness and formality, however, make no ingredient in his composition. On the contrary, he is very cheerful, sociable, frank, easy, and unpretending in his discourse and manners, and has so much modesty, artlessness, and child-like simplicity about him, that no one would be prepared to say, upon slight acquaintance, that he was anything more than an ordinary, sensible, well-disposed man, however much they might be pleased, which they would not fail to be, with his benign and agreeable countenance. But it is impossible to be in his company long, and to hear him talk on any subject, without being strongly impressed with the depth and originality of his views, his sterling good sense and wisdom, his profound and varied information, his clear and luminous conceptions, his ardent and unbounded love of science, his extreme liberality towards every nation under the sun, his entire freedom from envy or jealousy of any description, and from professional rivalry and bitterness, his singleness of purpose, his goodness of heart, and his reverence for all the duties that belong to a Christian, an accountable being, and a man."

As a practitioner of medicine, Dr. Prichard was remarkable for decision on the character of disease, and for a promptness and energy in the application of remedies. Many have been the instances where, in extreme cases, the boldness of his practice was followed by unexpectedly happy results. In his intercourse with professional brethren and colleagues, his conduct was straightforward, honorable, and generous; to his patients he was gentle, attentive, and kind.

High moral and religious principle, an affectionate disposition, an instinctive sentiment of delicacy, propriety, and consideration of the feelings of others, and a retiring modesty and simplicity of deportment, as much distinguished and endeared him in the domestic and social relations of life, as his literary and scientific attainments elevated him to the eminence he held in public estimation; he furnished, indeed, a bright example of the scholar, the gentleman, and the Christian.

Dr. Prichard's great attainments and learned and important works justly acquired universal reputation, and the honours and distinctions of literary and scientific societies were poured in upon him. When he attended the meeting of the Provincial Medical Association at Oxford, the University conferred upon him the Doctor's degree. The National Institute of France elected him a corresponding member, and he received the same distinction from the Academy of Medicine and Statistical Society there, from the Academy of Natural Sciences of Philadelphia, the American Philosophical Society, the Oriental Society of America, the Ethnological Society of New York, the Scientific Academy of Sienna, and from other bodies. He was likewise fellow of the Royal Society, and member of the Royal Irish Academy.

However gratifying it must have been to Dr. Prichard to receive these proofs of respect and esteem from the learned men of his own and other countries, they were only the fruitless rewards which intellectual merit is wont to receive. The home government of our country was not insensible of his great merits, and doubtless considered that it was conferring something more than empty honour in appointing him to the arduous and responsible office of Commissioner of Lunacy, with a well-earned salary of £1800 a year. It is far from being my object to censure the government upon an appointment which did credit to their selection, and secured the performance of an important service on terms which the strictest economy would approve; but I would nevertheless invite attentive consideration to the contrast which may be drawn between this reward bestowed on intellectual merit and that which is awarded to military achievements. The reward of the man of learning and knowledge consists in giving him the opportunity of deriving a moderate emolument from continued exertion, and exposure to fatigue and danger—a danger which in the case of our late honoured President was doubtless fatal. For had not duty required him to travel during an inclement season, you would probably be now receiving instruction from his own lips, instead of listening to this imperfect sketch of his shortened life from your associate in lamenting the irreparable loss which we have sustained.

BOOKS RECEIVED FOR REVIEW.

Demonstrations of Anatomy; being a Guide to the Knowledge of the Human Body by Dissection. By George Viner Ellis. Second edition, rewritten. London, 1849. Part I; pp. 464.

A Manual of the Dissection of the Human Body. By Luther Holden, F.R.C.S. Part I. London, 1849. pp. 106.

On the Nature of Limbs. A Discourse, delivered on Friday, Feb. 9, at an Evening Meeting of the Royal Institution of Great Britain. By Richard Owen, F.R.S. With Illustrations. Lond. 1849. 8vo, pp. 120.

The Homologies of the Human Skeleton. By Holmes Coote, F.R.C.S. London, 1849. 8vo, pp. 100.

Lectures on the Parts concerned in the Operations on the Globe, and on the Structure of the Retina. By W. Bowman, F.R.S., F.R C.S. London, 1848. 8vo, pp. 46.

Surgical Anatomy. By Joseph Maclise, M.R C.S. Fasciculus II. Folio. Four Plates. London, 1849.

Healthy Skin: a Treatise on the Management of the Skin and Hair, in relation to Health. By

Erasmus Wilson, F.R.S. Third Edition. London, 1849. Fcap. 8vo, pp. 238.

Elements of Electro-Biology, or the Voltaic Mechanism of Man; of Electro-Pathology, especially of the Nervous System; and of Electro-Therapeutics. By Alfred Smee, F.R.S., &c. London, 1849. 8vo, pp 164.

On the Dependence of Animal Motion on the Law of Gravity. By Henry Wiglesworth, M.B. Lond. 1849. 8vo, pp. 210.

On Healthy and Diseased Structure, and the True Principles of Treatment for the Cure of Disease, especially Consumption and Scrofula; founded on Microscopical Analysis. By William Addison, M.D., F.R.S. London, 1849. 8vo, pp. 320.

On Cancerous and Cancroid Growths. By John Hughes Bennett, M.D., F.R.S.E., Professor of the Institutes of Medicine in the University of Edinburgh. Edinburgh, 1849. 8vo.

Thoughts on Pulmonary Consumption; with an Appendix on the Climate of Torquay. By William H. Madden, M.D. London, 1849. 12mo, pp. 220.

Hooper's Medical Dictionary. Eighth Edition, edited by Klein Grant, M.D., &c. London, 1848. 8vo, pp. 1392.

On Infantile Laryngismus: with Observations on Artificial Feeding, as a frequent Cause of this Complaint, and of other Convulsive Diseases of Infants. By James Reid, M.D., &c. London, 1849. Post 8vo, pp. 204.

An Essay on the Cerebral Affections occurring most commonly in Infancy and Childhood. By Valentine Duke, M.D., F.R.C.S.I. Dublin, 1849. 8vo, pp. 90.

Essays on Infant Therapeutics: to which are added, Observations on Ergot. By John B. Beck, M.D. New York, 1849. 12mo, pp. 118.

Eruptions of the Face, Head, and Hands; with the latest Improvements in the Treatment of Diseases of the Skin. Illustrated with coloured Plates. By T. H. Burgess, M.D. London, 1849. 8vo, pp. 254.

Elements of Materia Medica and Therapeutics. By Jonathan Pereira, M.D., F.R.S. & L.S., &c. Third Edition, enlarged and improved. Vol. I. London, 1849. 8vo, pp. 898.

Cod-Liver Oil: its Uses, Mode of Administration, &c. By John Rayner. London, 1849. 12mo, pp. 16.

A Sketch of a Popular and Novel Treatment for Diarrhœa, Dysentery, and English and Asiatic Cholera; with Illustrative Cases of the Diseases. By Edmund Skiers, M.D. London, 1849. 8vo, pp. 92.

The Leading Phenomena of Epidemic Cholera, with some Plain Suggestions for its Better Treatment and Prevention. By Ambrose Blacklock, Assist.-Surg. Madras, 1848. 8vo, pp. 68.

The Code of Safety; or Causes, Effects, and Aids, Preventive and Curative, as well of other Epidemics, as also of Asiatic Cholera. By G. F. Collier, M.D. London, 1849. 8vo, pp. 96.

Cholera Gleanings, a Family Hand-book. By J. Gillkrest, M.D., Inspector-General of Army Hospitals. Gibraltar, 1848. 8vo, pp. 86.

Two Lectures on Cholera and Intermittent Fever. By Charles W. Bell, M.D., F.L.S. London, 1849. 16mo, pp. 102.

An Essay on some of the most important Diseases of Women. By W. Jones, M.D. London, 1848. 12mo, pp. 42.

On the Use of Chloroform in Dental Surgery. By J. Chitty Clendon. London, 1849. 8vo, pp. 20.

Elements of Chemistry, Theoretical and Practical, including the most recent Discoveries and Applications of the Science to Medicine and Pharmacy, &c. &c. With 230 Woodcuts. By Sir Robert Kane, M.D., M.R.I.A., &c. Second edition. Dublin, 1849. pp. 1070.

The Chemistry of Vegetable and Animal Physiology. By G. J. Mulder, M.D. Part IV. London, 1849.

The Hunterian Oration, delivered before the Royal College of Surgeons of England. By Cæsar Hawkins. London, 1849. 8vo, pp. 36.

Sequel to Outlines of Medical Proof. By Thomas Mayo, M.D., F.R.S. London, 1849. 8vo, pp. 42.

Medical Missions; an Address to Students, introductory to a Course of Lectures on this Subject. By James Miller, F.R.S.E., F.R.C.S.E. Edinburgh, 1849. 12mo, pp. 86.

On the Science of those Proportions by which the Human Head and Countenance, as represented in Works of Ancient Greek Art, are distinguished from those of ordinary Nature. By D. R. Hay, F.R.S.E. Edinburgh, 1849. 4to, pp. 80. With 25 plates.

The Serpentine "as it is" and "as it ought to be," and the Board of Health "as it is" and "as it ought to be." By Edward John Tilt, M.D. London, 1848. 8vo, pp. 68.

The Medical Remembrancer, or Book of Emergencies. By Edward B. L. Shaw. Third Edition. London, 1849. 18mo, pp. 108.

Dissertation on Scientific Nomenclature, Medical and General; exhibiting the Defects, Anomalies, Errors, and Discrepancies of its present Condition; with Suggestions for its Improvement. By R. G. Mayne, M.D. London, 1849. 8vo, pp. 82.

Some Thoughts on Natural Theology, suggested by a work entitled "Vestiges of the Natural History of Creation." London, 1849. 8vo, pp. 246.

A Brief Sketch of the "Hounslow Inquest," &c. &c. By Horatio Grosvenor Day. London, 1849. 8vo, pp. 20.

London and Provincial Medical Directory for 1849.

The Undercliff of the Isle of Wight: its Climate, History, and Natural Productions. By George A. Martin, M.D. London, 1849. Post 8vo, pp. 366.

Mémoire sur les Faits relatifs à la Révocation de M. Bouillaud des fonctions de Doyen de la Faculté de Médecine de Paris, &c. &c. Par M. J. Bouillaud. Paris, 1849. 8vo, pp. 144.

La Porrette et Monte-Catini. Par Robert Maunoir, D.M. Florence, 1848. 8vo, pp. 256.

INDEX TO VOL. III

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

PAGE
Absorption of virus 531
    of medicinal substances . 544
Académie Royale, Memoirs of . . 123
Acland, Dr., on education at Oxford . 215
Alcoholic liquors, use of . . . 111
Alkaloids, artificial formation of . 505
Allen, Mr., on cholera . . . 1
Amenorrhœa, use of electricity in . 380
American Medical Biography . . 210
Ammon, Von, on purulent ophthalmia 540
Anatomy, Surgical, Mr. Maclise's 206, 510
    Quain and Sharpey's . . 478
    Ellis's Demonstrations of . 512
Angina tonsillaris, treatment of . . 544
Antaphrodisiac properties of sugar . ib.
Arnott, Mr., on tumour of uterus . 435
Articular tissues, Mr. Birkett on . 337
Ashwell, Dr., on diseases of females . 138
Asphyxia, Dr. J. Reid on . . . 309
Asphyxia of variola 534
Asthma in infant 533
Ballard, Dr., on gelatiniform cancer . 444
Bandaging, &c., Dr. Sargent on . 452
Barker, Dr., his case of laryngitis . 437
Bartlett, Dr., on certainty in medicine 509
Baxter, Mr., on electricity in secretion 521
Becquerel, M. Ed., on electricity . 373
Bégin, M., on gunshot wounds . 271
Bellary, cholera at 26
Bernard, M. Ch., on saliva . . 261
    on origin of sugar . 526
    on pancreatic fluid . 527
Bile, diseases of 123
    composition of . . . 259
Biliary calculi 129
Billing, Dr., on cholera . . . 1
Bird, Dr. Golding, on the chemistry of therapeutics 285
    on electricity . 373
Birkett, Mr., on articular tissues . 337
Blanchard, Dr., on circulation in insects 257
Blindness, rheumatic, case of . . 266
Blood, composition of . . 288, 294
    condition of in fever . . 95
    in gout, &c., Dr. Garrod on . 440
Boggie, Dr., on hospital gangrene . 358
Botany, Dr. Lindley's Introduction to 515
Bowman, Mr. J. E., his Practical Chemistry 212

PAGE
Buchanan, Dr., on cholera . . 1
Calomel, influence of on bile . . 278
    small doses of . . . 544
Cancer, gelatiniform, of peritoneum . 444
Cannabine, preparation of . . 543
Castor oil, administration of . . 543
Cavendish Society's Reports, &c. . 502
Cerebral affections of childhood, Dr. Duke on 514
Chapman, Mr., on the treatment of ulcers 198
Chemical analyses, Rose and Normandy on 203
Chemical pathology 52
Chemical Reports and Memoirs . 502
Chemistry, practical, Dr. R. D. Thomson's 212
    Mr. J. E. Bowman's ib.
Chemistry, Graham's Elements of . 219
Chemistry of therapeutics . . 285
Children, diseases of . . . 406
Cholera, infantile 269
    diffusion of 23
    in St. Petersburg . . 1
    its course in Russia . . 7
    its characters in Russia . 9
    in India 22
Chorea, use of electricity in . . 378
Christison, Dr., his Dispensatory . 169
Clinical Midwifery, Dr. R. Lee's . 211
Clinical Lectures, Graves and Duncan's 455
Cock, Mr., on strangulated hernia . 339
Cod-liver oil in lupus . . . 279
Cold, endurance of 111
Colchicum, vin. sem., in gonorrhœa . 280
Coleridge on Life 194
Condylomata, structure of . . . 356
Convulsions of children . . . 416
Corns, structure of 343
Corfe, Mr. on impure water . . 117
Cotton, hæmostatic properties of . 544
Coxalgia, M. Malgaigne on . . 536
Crawford, Dr., on cholera in Russia . 1
Cuvier's Animal Kingdom . . 513
Dalrymple, Mr., on the pathology of the human eye 224
Dalrymple, Mr. D., his case of extra-uterine fœtation 449
Daltonism, case of 259
Daremberg, Dr., his Bibliothèque . 519

PAGE
Day, Dr., on diseases of old age . 486
Dendy, Mr., on cerebral diseases of children 406
Devergie, M., on treatment of lupus . 538
Devilliers, M., on the dropsy of pregnant women 273
Dictionary of Medicine, Dunglisson's 220
  Hooper's 517
Diet, Dr. Robertson on . . . 108
Digitalis, poisoning by . . . 280
Directory, London and Provincial . 520
Diseases of advanced life . . . 486
  ear 66
  children 406
Dispensatory, Dr. Christison's . . 169
Donovan, Mr., on electricity . . 373
Dropsy of pregnant women . . 273
Drunkenness, M. Tardieu on . . 545
Dubois, M., on anomalies of menstruation 276
Duke, Dr., on cerebral affections . 514
Dunglisson, Dt., his Dictionary of Medical Science 220
Duncan, Dr., his clinical lectures . 455
Dysmenorrhœa, Drs. Ashwell and Meigs on 142
Ear, Dr. Schmalze on diseases of . 66
Electricity in secretion, Mr. Baxter on 523
Electricity, therapeutic employment of 373
Eighth pair, functions of, Dr. J. Reid on 324
Ellis, Mr., his Demonstrations of Anatomy 512
Eneuresis; belladonna in . . . 280
Epilepsia peripherica . . . 265
Ether, effect of, on respiration . . 259
Eye, pathology of the, Mr. Dalrymple on 224
Extra-uterine fœtation, case of . . 449
Fauconneau-Dufresne, M., on diseases of the bile 123
Females, diseases of . . . 138
Fergus, Mr., on laceration of the liver 436
Fever, pathology of 74
  nature of 97
  principles of treatment of . 99
  Dr. Graves on . . . 458
Fibrous tumours, multiple . . 530
Flourens, M., on absence of vomiting in horse 524
Forget, Dr., on poisoning by digitalis 280
Fractures, M. Malgaigne on . . 388
Fracture, ununited, treatment of 400, 446
France, Mr., his ophthalmic cases . 336
Freke, Dr., on organization . . 194
Funis, prolapsus of 540
Garrod, Dr., his Lectures on the Chemistry of Therapeutics . . 285
  on the blood of urine . 440
Gastric juice, acid of . . . 260
Gay, Mr., on femoral rupture . . 165

PAGE
General pathology, Dr. Stillé's . . 207
Glosso-pharyngeal nerve, functions of 324
Glottis, œdema of 535
Gonorrhœa, treatment of, with colchicum 280
Gout, Dr. Graves on . . . 466
Graham, Professor, his Elements of Chemistry 219
Graves, Dr., his Clinical Lectures . 455
Gunshot wounds, discussion on . 271
Guy's Hospital Reports . . . 333
Hæmoptysis, laws of . . . 244
Hall, Dr. C. R., on sympathetic . 364
Hall, Dr. M., on irritability of muscle 447
  on convulsive affections of infants . . 406
Hare-lip, on operating for . . 270
Heart, malformation of . . . 438
Hernia, Mr. Cock on . . . 339
  use of aqua saturni in . . 271
  Mr. Luke on operation for . 441
History of Medicine, Sprengel's . 208
Hodgkin, Dr., his memoir of Dr. Prichard 550
Hohl, Dr., on prolapsus of uterus . 538
Holden, Mr., his Manual of Dissection 512
Hooper's Medical Dictionary . . 517
Horse, absence of vomiting in . . 524
Hospital gangrene, Dr. Boggie on . 358
Hospitals, Parisian, statistics of . 549
Hughes, Dr. H., on pneumonia . . 333
Humerus, new dislocation of . . 537
Hunt, Mr., on the Poetry of Science . 495
Hydatid disease of liver . . . 334
Hydrometra, case of . . . 539
Hydrophobia, spontaneous . . 264
Icterus, M. Fauconneau Dufresne on . 136
India, cholera in 22
Infancy, diseases of 406
Infants, cholera of 269
Influenza and ozone . . . 529
Injections into the veins . . . 263
Insects, circulation in . . . 257
Intestinal obstructions . . . 433
Iodide of potassium, in saturnine affections 543
Iodine, clinical detection of . . 262
Irritability of muscle, Dr. M. Hall on 447
Jobert, M., on gunshot wounds . 271
Jones, Dr., his syphon douche . . 521
Juice of flesh, absence of urea in . 262
Keir, Dr., on cholera . . . 1
Kidneys, state of, in death by suffocation 546
Kirkes, Dr. his Handbook of Physiology 217
Knapp, Dr., his chemical technology . 215
Kopp, Prof., on formation of alkaloids 505
Laceration of liver, case of . . 43
Lactic acid in animal fluids . . 261
Laryngismus 420

PAGE
Laryngitis, Dr. Barker's case of . 437
Lead, acetate of, use of in hernia . 271
Lebert, M., on multiple tumours . 530
Lee, Dr. R., his Clinical Midwifery . 211
Life, Coleridge on 194
Lindley, Dr., his Introduction to Botany 515
Liver, action of in producing sugar . 526
Locock, Dr., on uterine polypi . . 450
Lupus, cod-liver oil in . . . 279
  MM. Devergie and Petrequin on 538
Maclise, Mr., his Surgical Anatomy 206, 510
Malaria 80
Malarious fevers 82
Malgaigne, M., on coxalgia . . 536
  on luxation of humerus . 537
  on fractures 388
Martin-Solon, M., on bilious pneumonia 268
Martin, Dr., on the Undercliff . . 522
Materia Medica, works on . . 285
M'Whinnie, Mr., his case of ophthalmia 438
Medical Biography, American . . 210
Medical Dictionary, Hooper's . . 517
  Dunglisson's . 220
Medical Jurisprudence, Dr. A. S. Taylor's 221
Medical organization and reform . 31
Medicines, chemical action of . . 308
Medico-Chirurgical Transactions . 433
Meigs, Dr. C. D., on diseases of females 138
Meigs, Dr. J. F., on diseases of children 406
Menorrhagia, Drs. Ashwell and Meigs on 146
Menstruation, anomalies of . . 276
  Dr. Ashwell on . . 140
Mialhe, M., on prescribing . . 285
Miasmata 81
Miasmatic fevers 84
Michea, M., on calomel . . . 278
Microscope, Mr. Quekett on . . 188
Midwifery, Dr. Tucker's Principles of 207
Milk, human, analysis of . . . 526
Minor surgery, Dr. Sargent on . . 452
Mohr, Dr., his Practical Pharmacy . 516
Monneret, M., on thoracic vibration . 262
Müller, Dr. C., on cholera in Russia . 1
Muscular contractility, Dr. J. Reid on 319
Muscular fibre, irritability of, Dr. M. Hall on 447
Muscular rheumatism . . . 265
Neligan, Dr., on diseases of scalp . 498
Neuralgia, ciliary 533
Normandy's introduction to chemical analysis 203
Œdema of glottis 535
Oldham, Dr., on uterine displacements 341
Old age, diseases of 486
Ophthalmia, destructive, case of . 438
  purulent, of infants . 540
Organization, Dr. Freke on . . 194
  medical . . . 31
Ormerod, Dr., on fever . . . 74
Ovariotomy, Dr. Ashwell on . . 163
Oxford university, education at . 215
Ozone and influenza . . . 529
Paget, Mr., on physiology . . 217
Pallas, M., on electricity . . . 386
Pancreatic fluid, properties of . . 527
Paracentesis, M. Trousseau on . . 533
Paralysis, use of electricity in . . 382
Paris, medical statistics of . . 549
Par vagum, functions of . . . 326
Paterson, Dr., on cholera . . . 1
Peacock, Dr., on malformation of heart 438
Pectoral undulation 262
Pereira, Dr., his Materia Medica . 285
Periodoscope, Dr. Tyler Smith's . 214
Petersburgh, St., cholera at . . 1
Pharmacy, practical 516
Phellandrium aquaticum, seeds of . 279
Phillips, Mr. B., on intestinal obstructions 433
Phthisis, report on, Dr. Walshe's . 225
  in relation to sex, &c. . ib.
  course of 228
  question of hereditary nature of 235
  comparative progress of in two lungs . . . 240
Physiology, Hand-book of . . 217
Pneumonia, bilious 268
  post-mortem appearances in 333
Poetry of science, Mr. Hunt on . 495
Poisons, febrific 78
Poisoning by digitalis . . . 280
  by sulphate of zinc . . 545
Polydipsia, M. Vigla on . . . 534
Polypus of uterus, Dr. Locock on . 450
Pregnancy, dropsy of . . . 273
Prichard, Dr., his Natural History of Man . . . 222
  memoir of . . . 550
Prolapsus of funis 540
  of uterus, Prof. Hohl on . 541
Prout, Dr., on stomach and renal diseases 49
Purulent ophthalmia of infants . . 540
Quain's Anatomy 478
Quekett, Mr., on the microscope . 188
Rachitis, M. Trousseau on . . 267
Redwood, Mr., his Practical Pharmacy 516
Rees, Dr. G. O., on disease of liver . 334
Reform, medical 31
Regimen, Dr. Robertson on . . 101
Reid, Dr. W., on cholera . . 1
Reid, Dr. Jas., on descent of uterus . 451
Reid, Dr. John, his researches . . 318
Respiration, effect of ether on . . 259
Rhubarb, administration of . . 544
Rheumatism, muscular . . . 265
Robertson, Dr., on diet, &c. . . 101

PAGE
Rogers, Mr., his reports on cholera . 22
Rose's treatise on chemical analysis . 203
Rosenbaum, Dr., his edition of Sprengel's History of Medicine . . 208
Rupture, femoral, Mr. Gay on . . 165
Russia, cholera in 7
Sadler, Mr., on cholera . . . 1
Saliva, chemical properties of . . 261
Sandras, M., on seeds of phellandrium aquaticum 279
Sargent, Dr., on minor surgery . 452
Scalp, eruptive diseases of . . 498
Schmalze, Dr., on diseases of the ear 66
Schneiderian membrane, absorption by 544
Scrofula, analogies with tubercle . 530
Serres, M., on variola . . . . ib.
Sharpey, Dr., his edition of Quain's Anatomy 478
Sims, Dr., on trismus nascentium . 276
Simon, Dr. Gustav, on diseases of the skin 342
Skin, diseases of, Dr. Simon on . 342
  portraits of diseases of . . 222
Smith, Dr. W. Tyler, his periodoscope 214
Spermatozoa, development and functions of 524
Sprengel's History of Medicine . . 208
Statistics, of suicide 547
  medical, of Paris . . 549
Statistical inquiries, by M. Valleix . 547
Strangulation, internal, strychnine . 539
Stillé, Dr., his General Pathology . 207
Stomach, converting powers of . 54
Strecker on the bile 259
Strumous diatheses, Dr. Prout on . 62
Strychnine, in internal strangulation . 539
Subclavian, ligature of . . . 537
Suffocation, death by . . . 546
Sugar, origin of in animals . . 526
  as antaphrodisiac . . . 544
Suicide, statistics of 547
Sulphate of zinc, poisoning by . . 545
Sympathetic nerve, Dr. C. R. Hall on 364
Tardieu, M., on drunkenness . . 545
Taylor, Dr. A. S., on medical jurisprudence 221
Technology, Chemical, Dr. Knapp's . 215
Teeth, Mr. Tomes on . . . 175
Thomson, Dr. R. D., his School Chemistry 212
Thurnam, Dr., his cases of imperfect development of hair, &c. . . 439
Tomes, Mr., on the teeth . . . 175
Trismus nascentium, Dr. Sims on . 276

PAGE
Trousseau, M., on belladonna in eneuresis . . . 280
  on cholera of infants . 269
  on materia medica . . 285
  on paracentesis . . 533
  on rachitis . . . 267
Tubercle, analogies with scrofula . 530
Tucker, Dr. his Principles of Midwifery 207
Twins, extraordinary size of . . 278
Typhoid fever, pathology of . . 74
Ulcers, blue coloration of . . . 260
  Mr. Chapman on . . . 198
Undercliff, Dr. Martin on the . . 522
Urea, on estimation of . . . 525
  in vitreous humour . . . 262
Urine, composition of . . 287, 297
  in gout, &c., Dr. Garrod on . 440
Uterine displacements, Dr. Oldham on 341
Uterine sound, abuse of . . . 149
  supporter, Dr. Simpson's . 160
Uterus bicornis 542
  descent of, Dr. Reid on . . 451
  diseases of, Drs. Ashwell and Meigs on 147
  prolapsus of 541
  tumour of extirpated during labour 435
Vaccinia, simultaneous with variola . 531
Valleix, M., on muscular rheumatism 265
  his statistical inquiries . 547
Variola, influence of physical agents in 530
  treatment of asphyxia of . 534
  simultaneous with vaccinia . 53
Variolous pustule, structure of . . 349
Veins, effects of injections into . . 263
Vibration, thoracic 262
Vigla, M., on polydipsia . . . 534
Virus, absorption of 531
Wagner and Leukhardt on spermatozoa 524
Walshe, Dr., his Report on Phthisis . 225
Warren, Dr. M., on ligature of subclavian 537
Water, effect of impurities of . . 117
West, Dr., on diseases of . . . 406
Williams, Dr., his American Medical Biography 210
Williams, Dr. T., on aquatic respiration 338
Wilson, Mr. E., his Portraits of Skin-Diseases 222
Wolverton, impurity of water at . 117
Wonders of the human body . . 220
Wood, Dr., on the practice of medicine 455

END OF VOL. III.

AND J. ADLARD, PRINTERS, BARTHOLOMEW CLOSE.

3220 113
m

3 2044 10